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amination; but to the law of the phænomena, considered as a general fact allowed to be consonant to experience. Now one of the most curious points in Laplace's theory of capillary action[*], is a demonstration, deduced from the equation of the curve surface, which proves that the volume of elevated fluid, or the space made void by the capillary force in the case of a depression, is proportional to the interior periphery of any cylindrical or prismatic tube immersed in the fluid. And it is correct to affirm generally, that what is called Dr. Jurin's theory, is no other than a mathematical consequence flowing from the two principles we have laid down.

There is one set of facts very proper to bring the exact agreement of the mathematical theory with nature to a severe trial. We allude to the depression of the mercury in the tubes of barometers of various diameters. The accuracy requisite in modern philosophical pursuits has drawn the attention of experimentalists to determine the quantity of the depression, in order to derive the true height of the mercury, from the observed height. In tubes from about one tenth of an inch in diameter, to seven or eight tenths, the convex curvature of the surface and the depression are found to vary very quickly and notably; and the comparison of the theory with such a series of connected experiments, cannot but furnish a delicate test of its exactness. Here, however, a difficulty occurs. The mathematical determination of the depression is a problem of great difficulty, which does not yield to the methods of investigation usually employed by analysts, and which has not yet been solved in a satisfactory manner. The remainder of this article is an attempt to overcome this difficulty.

I put y for the vertical ordinate of a point in the convex surface of the mercury, or the distance below the general level; r for the distance of the same point from the axis of the tube; and z for the sine of the inclination of the vertical section of the curve surface to r, or to the horizon: according to what is taught in geometry, $\frac{dr}{dz}$ is the radius of curvature of the vertical section; and $\frac{r}{z}$, the radius of curvature of the section at right angles to the vertical section: and hence we obtain from the principles laid down,

$$\frac{dz}{dr} + \frac{z}{r} = 4\beta^2 y,$$

$$\frac{dy}{dr} = \frac{z}{\sqrt{1 - z^2}},$$

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[*] *Suppl. à la Théorie de l'Action Capillaire*, p. 10.

$4\beta^2$ being a quantity to be found by experiment in tubes of any given matter.

In the glass tubes of which barometers are made, $4\beta^2$ is very nearly equal to 496, and β to 7; and the value of z at the surface of such tubes, or the sine of the inclination of the mercury to the horizon, may be taken equal to 0·735. These numbers, which are very convenient, accord nearly with the results of the best experiments, or at least approach to them within the limits of the errors to which all such experimental determinations are liable.

Instead of the curve surface actually formed in the tube, I shall now consider another similar surface, of which the linear dimensions are increased fourteen times, or in the proportion of 2β to 1. If y represent the vertical ordinate of this new curve, and y stand for $2\beta r$, we shall have these equations, viz.

$$\frac{dz}{dx} + \frac{z}{x} = y$$

$$\frac{dy}{dx} = \frac{z}{\sqrt{1-z^2}}.$$

Exterminate y, and put $t = \frac{x}{2} = \beta r$; then

$$\frac{ddz}{dt^2} + \frac{dz}{t\,dt} - \frac{z}{t^2} = \frac{4z}{\sqrt{1-z^2}}$$

It may be proper to observe here, that the depression is the value of the vertical ordinate, when z and x vanish together, in which case also $\frac{z}{x} = \frac{dz}{dx}$. Hence the depression is equal to $\frac{2z}{x}$ or $\frac{2dz}{dx}$, that is, to $\frac{z}{t}$ or $\frac{dz}{dt}$, when z and t are both evanescent.

Next assume, $z = qtc^{\int \omega dt}$,

c being the base of Napier's logarithms, and q a constant quantity, which is no other than the depression. Substitute the values of z and its fluxions in the last equation, leaving untouched the radical on the right-hand side; then,

$$\omega^2 + 3\frac{\omega}{t} + \frac{d\omega}{dt} = \frac{4}{\sqrt{1-z^2}}$$

$$\frac{dz}{dt} = z\left(\omega + \frac{1}{t}\right).$$

In the foregoing operations z and ω are considered as functions of t: but I shall now suppose that z is a function of t, and ω a function of the independent variables z and t. In the last equation we must therefore write $\frac{d\omega}{dt} + \frac{d\omega}{dz}\cdot\frac{dz}{dt}$ for $\frac{d\omega}{dt}$;

$\frac{d\omega}{dt}$; which being done, and the value of $\frac{dz}{dt}$ substituted, we shall get this equation in partial fluxions, viz.

$$\omega^2 + \omega \frac{d\omega}{dz} z + \frac{d\omega}{dz} \cdot \frac{z}{t} + 3\frac{\omega}{t} + \frac{d\omega}{dt} = \frac{4}{\sqrt{1-z^2}}, \qquad (1)$$

to which we must join the value of $\frac{dz}{dt}$, viz.

$$\frac{dz}{dt} = z\left(\frac{1}{t} + \omega\right). \qquad (2)$$

I next write the equation (1) in this manner,

$$\frac{1}{t} \cdot \frac{d.\omega z^3}{z^2 dz} + \frac{d\omega}{dt} + \frac{d.\omega^2 z^2}{2z dz} = \frac{4}{\sqrt{1-z^2}};$$

and assume, $\omega = \varrho + Az^2 + Bz^4 + \&c.$

ϱ, A, B, &c. being functions of t. Substitute this value of ω, and expand the radical on the right-hand side of the equation; then, by equating the coefficients of the like powers of z, we shall get,

$$\left.\begin{aligned} &\varrho^2 + 3\frac{\varrho}{t} + \frac{d\varrho}{dt} = 4 \\ &4\varrho A + 5\frac{A}{t} + \frac{dA}{dt} = 2 \\ &6\varrho B + 7\frac{B}{t} + \frac{dB}{dt} = \frac{3}{2} - 3A^2 \\ &\&c. \end{aligned}\right\} \qquad (3)$$

These equations give us,

$$\begin{aligned} &\varrho = t - \frac{t^3}{6} + \&c. \\ &A = \frac{t}{3} - \frac{t^3}{6} + \&c. \\ &B = \frac{3}{16}t - \frac{7}{48}t^3 + \&c. \\ &\&c. \end{aligned}$$

There is another consequence of the first of the equations (3) which it is necessary to notice. Put

$$\lambda = c^{\int \varrho dt};$$

then we readily derive from the equation mentioned,

$$\frac{dd\lambda}{dt^2} + 3\frac{d\lambda}{t dt} = 4\lambda;$$

and by integrating in a series, without introducing any arbitrary constants, which the present purpose does not require, we shall get,

$$\lambda = 1 + \frac{t^2}{2} + \frac{t^4}{2^2.3} + \frac{t^6}{2^2.3^2.4} + \frac{t^8}{2^2.3^2.4^2.5} + \&c.$$

If l be the diameter of the tube, then $t = \beta r = \frac{7}{2} . l$, and $t^2 = 12{\cdot}25\, l^2$: thus we have,

$$\begin{aligned} \lambda = 1 &+ 6{\cdot}125\, l^2 \\ &+ 12{\cdot}505\, l^4 \\ &+ 12{\cdot}756\, l^6 \\ &+ 7{\cdot}819\, l^8 \\ &+ 3{\cdot}193\, l^{10} \\ &+ 0{\cdot}931\, l^{12} \\ &+ \&c. \end{aligned} \qquad (A)$$

In all tubes a few terms of this series will give the value of λ with sufficient exactness.

Substitute the value of ω that has been found in the equation (2); then

$$\frac{dz}{z\,dt} = \frac{1}{t} + \varrho + Az^2 + Bz^4 + \&c.$$

The quantities ϱ, A, B, &c. evidently vanish when t is equal to zero; let us then inquire what are their values when t is infinitely great. When t is infinite, the equation (1) becomes simply,

$$\frac{d\,.\,\omega^2 z^2}{2z\,dz} = \frac{4}{\sqrt{1-z^2}};$$

hence,

$$\cdot\cdot = \frac{2\sqrt{2-2\sqrt{1-z^2}}}{},$$

or

$$\omega = 2 + \frac{z^2}{4} + \frac{7\,.\,z^4}{64} + \&c.$$

Thus it appears that while t increases from zero to be infinitely great, ϱ, A, B, &c. increase from zero to the finite quantities 2, $\frac{1}{4}$, $\frac{7}{64}$, &c. The terms in the values of ω, and of $\frac{dz}{dt}$, which are omitted, do not, therefore, affect the approximation except in a very limited degree. The same conclusion may be obtained another way; namely, by solving the equations (3) in serieses of the descending powers of t. If this be done, the parts of ϱ, A, B, &c. independent of $\frac{1}{t}$, will agree with the values above assigned.

Put $2a$ for the value of ω, or of $\frac{dz}{z\,dt}$, when t is infinite; then,

$$a = \frac{\sqrt{2-2\sqrt{1-z^2}}}{z} = 1 + \frac{z^2}{8} + \frac{7z^4}{128} + \&c.$$

Subtract $a\varrho$ from both sides of the equation before found, then,

$$\frac{dz}{z\,dt} - a\varrho = \frac{1}{t} - (a-1)\,\varrho + Az^2 + Bz^4 + \&c.:$$

and, by making,

$$A' = A - \frac{\varrho}{8} = \frac{5}{24}\,t - \frac{7}{48}\,t^3 + \&c.$$

$$B' = B - \frac{7\varrho}{128} = \frac{17}{128}\,t - \frac{35}{256}\,t^3 + \&c.$$

we get, $$\frac{dz}{z\,dt} - a\varrho = \frac{1}{t} + A'z^2 + B'z^4 + \&c.$$

The left side of this equation is equal to zero when t is infinite; for $a\varrho$ then becomes $2a$. From this it follows that A', B', &c. are evanescent both when t is equal to zero and when it is infinite. The terms on the right side, except the first, do not therefore increase indefinitely, but always remain inconsiderable.

Assume, $$\frac{dz}{az} = \frac{d\sigma}{\sigma}:$$

then, $$\sigma = z - \frac{z^3}{16} - \&c.$$

$$z = \sigma + \frac{\sigma^3}{16} + \&c.$$

$$a = 1 + \frac{\sigma^2}{8} + \frac{9\sigma^4}{128} + \&c.:$$

then, by dividing all the terms of the foregoing equation by a, and introducing σ for z, we shall get,

$$\frac{d\sigma}{\sigma\,dt} - \varrho = \frac{1}{a}\cdot\left\{\frac{1}{t} + A'\sigma^2 + \left(B' + \frac{A'}{8}\right)\sigma^4 + \&c.\right.$$

Next put, $$\frac{d\sigma}{\sigma\,dt} - \varrho = \frac{ds}{s\,dt} \because \sigma = s\,c^{\int\varrho\,dt} = \lambda.s.$$

Let a' stand for the same function of s that a is of σ, or of λs: then, making,

$$A'' = A' = \frac{5}{24}\,t - \frac{7}{48}\,t^3 + \&c.$$

$$B'' = B' + \frac{A'}{8} = \frac{61}{384}\,t - \frac{119}{768}\,t^3 + \&c.$$

we shall get,

$$\frac{a'ds}{s\,dt} = \frac{a'}{a}\cdot\left\{\frac{1}{t} + A''\lambda^2.s^2 + B''\lambda^4.s^4 + \&c.\right.$$

The fraction $\frac{a'}{a}$ must next be reduced to a series of the powers of s. Now,

$$\frac{a'}{a} = \frac{1 + \frac{1}{8}s^2 + \frac{9}{128}s^4 + \&c.}{1 + \frac{1}{8}\lambda^2 s^2 + \frac{9}{128}\lambda^4 s^4 + \&c.}:$$

and, by division,

$$\frac{a'}{a} = 1 - t\,\mathrm{P}.\,s^2 - t\,\mathrm{P}'.\,s^4 - \&c.$$

$$\mathrm{P} = \frac{\lambda^2 - 1}{8t} = \frac{t}{8} + \frac{5}{96}\,t^3 + \&c.$$

$$\mathrm{P}' = \frac{7\lambda^4 + 2\lambda^2 - 9}{128\,t} = \frac{t}{8} + \frac{41}{384}\,t^3 + \&c.$$

The series for $\frac{a'}{a}$ must next be substituted, and, after having multiplied and reduced, we shall get,

$$\frac{a'\,ds}{s\,dt} = \frac{1}{t} + \mathrm{A}'''\,s^2 + \mathrm{B}'''\,s^4 + \&c.$$

$$\mathrm{A}''' = \mathrm{A}''\,\lambda^2 - \mathrm{P} = \frac{t}{12} + \frac{t^3}{96} + \&c.$$

$$\mathrm{B}''' = \mathrm{B}''\,\lambda^4 - \mathrm{P}' - \mathrm{A}''\,\lambda^2.\,\mathrm{P}t = \frac{13}{384}\,t + \frac{23}{768}\,t^3 + \&c.$$

The principle of all this analysis lies in this, That, the left side of the equation being equal to zero when t is infinite, all the terms on the right side except the first must be evanescent both when t is zero and when it is infinite. These terms, therefore, remain always inconsiderable, and of no value except in some tubes of small diameter. In order to exhibit the equation in the most simple form possible, I finally put,

$$a'\,ds \qquad du$$

$$u = s + \frac{s^3}{16} + \&c.$$

$$s = u - \frac{u^3}{16} - \&c.:$$

and, by introducing u for s,

$$\frac{du}{u\,dt} = \frac{1}{t} + \mathrm{A}^{\mathrm{iv}}\,u^2 + \mathrm{B}^{\mathrm{iv}}\,u^4 + \&c.$$

$$\mathrm{A}^{\mathrm{iv}} = \mathrm{A}''' = \frac{t}{12} + \frac{t^3}{96} + \&c.$$

$$\mathrm{B}^{\mathrm{iv}} = \mathrm{B}''' - \frac{\mathrm{A}'''}{8} = \frac{3}{128}\,t + \frac{11}{384}\,t^3 + \&c.$$

We must now integrate, and for this purpose write the last equation in this manner,

$$\frac{d.\frac{u}{t}}{dt} = \frac{\mathrm{A}^{\mathrm{iv}}}{t}\,u^3 + \frac{\mathrm{B}^{\mathrm{iv}}}{t}\,u^5 + \&c.:$$

then assume, $\frac{u}{t} = q + \mathrm{Q}\,u^3 + \mathrm{Q}'\,u^5\ \&c.$

q being a constant, and Q, Q′ &c. functions of t. By differentiating,

$$\frac{d\,.\frac{u}{t}}{dt} = \frac{dQ}{dt}u^3 + \frac{dQ'}{dt}u^5$$

$$+ \frac{du}{u\,dt}(3\,Q\,u^3 + 5\,Q'u^5 + \&c.)$$

Substitute the value of $\frac{du}{u\,dt}$, and equate the coefficients of the two equal quantities, then

$$\frac{d\,.\,Qt^3}{t^3\,dt} = \frac{A^{iv}}{t},$$

$$\frac{d\,.\,Q't^5}{t^5\,dt} = \frac{B^{iv}}{t} - 3\,A^{iv}\,Q:$$

and hence, $$Q = \frac{t}{48} + \frac{t^3}{576} + \&c.$$

$$Q' = \frac{t}{256} + \frac{3\,t^3}{1024} + \&c.$$

These coefficients being known, we have the value of the constant q, which it is most convenient to arrange according to the powers of t, viz.

$$q = \frac{u}{t} - t\left(\frac{u^3}{48} + \frac{u^5}{256}\right)$$

$$- t^3\left(\frac{u^3}{576} + \frac{3\,u^5}{1024}\right).$$

It has already been noticed that the depression is the value of $\frac{z}{t}$ when z and t are both evanescent. But it is obvious that the vanishing fractions, $\frac{z}{t}$ $\frac{\sigma}{t}$, $\frac{s}{t}$, $\frac{u}{t}$ have all the same limit: wherefore q is the depression. It does not however belong to the surface of the mercury in the tube, but to a surface increased fourteen times in its linear dimensions. To get the real depression we must therefore divide by 2β. Now $t = \beta r = \frac{\beta}{2}\,l$, and $t^2 = 12{\cdot}25\,l^2$; and we thus obtain the following formula for the real depression,

$$q = \frac{u}{49\,l}\,. - l\left(\frac{u^3}{192} + \frac{u^5}{1024}\right)$$

$$- l^3\left(\frac{12{\cdot}25}{2304}u^3 + \frac{36{\cdot}75}{4096}u^5\right).$$

The symbol u stands for a known function of s; but in practice it will be most convenient to have q expressed immediately in terms of s. The necessary operations being performed, we finally obtain,

$$q = \frac{1}{49\,l} \times \left(s + \frac{s^3}{16} + \frac{5\,s^5}{256} + \frac{119\,s^7}{12288} + \frac{393\,s^9}{65536}\right) \qquad \text{(B)}$$
$$- \,l \times \left(\frac{s^3}{192} + \frac{s^5}{512}\right).$$

The term multiplied by l^3 being omitted, as it does not affect the value of q in the fifth decimal place. In order to find s, we must compute σ, for which purpose we readily obtain this series,

$$\sigma = z - \frac{z^3}{16} - \frac{z^5}{128} - \frac{35\,z^7}{12288} - \frac{137\,z^9}{98304} + \&\text{c.}:$$

and z being 0·735, we get $\sigma = 0{\cdot}70805$. Then

$$s = \frac{{\cdot}70805}{\lambda},$$

λ being computed by the formula (A).

As the diameter l decreases, s approaches indefinitely to σ, and u to z; so that, for tubes of an extremely small bore, the depression is,

$$q = \frac{z}{49\,l} = \frac{{\cdot}015}{l}.$$

On the other hand, the diameter of the tube increasing, s decreases very rapidly; and when the diameter is half an inch, or greater, all the powers of s are inconsiderable, and we have simply,

$$q = \frac{s}{49\,l} = \frac{{\cdot}70805}{49\,l \cdot \lambda} = \frac{{\cdot}01445}{l \, . \, \lambda}.$$

The first term of q in the formula (B) being the expansion of a known function, it may be continued to any degree of approximation; but the terms set down are sufficient even for very small tubes. Suppose $l = \frac{1}{20}$, thus $\lambda = 1{\cdot}01539$, $s = {\cdot}69736$, and

$$q = \frac{{\cdot}72274}{2 \cdot 45} - \frac{{\cdot}002}{20} = {\cdot}29490.$$

Next, let $l = \frac{1}{2}$, then $\lambda = 1{\cdot}5460$, $s = {\cdot}1997$, and

$$q = \frac{{\cdot}1997}{24 \cdot 5} = 0{\cdot}0815.$$

I have subjoined a Table of the depressions in tubes of various bores, and have added the experimental determinations published by Lord Charles Cavendish; by which it will be seen that the agreement of the theory with experiment is very satisfactory.

Depression of the Mercury in the Tubes of Barometers.

Diameter. inches.	Depression. inches	Observed by Ld. Cavendish.
0·05	0·29490	
·10	·14026	0·140
·15	·08628	·092
·20	·05811	·067
·25	·04077	·050
·30	·02919	·036
·35	·02110	·025
·40	·01534	·015
·45	·01117	
·50	·00815*	·007
·60	·00431*	·005
·70	·00228	
·80	·00119	

Dec. 14, 1827. J. IVORY.

II. *Heights of some of the principal Beds of Ingleborough Hill and Moughton Fell, Yorkshire.* *By* JOHN NIXON, *Esq.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN the xith volume of the Annals of Philosophy, I proposed to the geologist the measurement, by one barometer only, of the heights and dip of strata known to be nearly horizontal, and furnished at the same time the requisite instructions and tables. From the numerous measurements of this description made by me in different parts of the north-west of Yorkshire, I beg to transmit you a selection of such as may serve to determine the thicknesses and dip of the principal beds of Ingleborough Hill and Moughton Fell.

Having carefully measured by trigonometry the heights of the summits and other elevated parts of the hills, as well as of different places at their bases, barometrical observations were regularly made in the course of the day at the lowest and highest of the trigonometrical stations within reach, in order to ascertain and correct the error of the measurements by the barometer. When the station at the base could not conveniently be revisited, the observation at the superior one, together with their difference of altitude, served to determine the fall or rise of the barometer.

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* In the Supplement to the Encyclopædia, Article FLUIDS, there is an arithmetical blunder in each of these numbers, as will appear by having recourse to the formula. I was obliged to notice this point on a former occasion, in order to stop the triumph of an antagonist, who has stuck to my skirts for a period of at least six years, and who seems to have great confidence in the efficacy of the adage, *If one way will not do, another will.*

The following list, comprising the chief beds of the Ingleton Fells, is arranged conformably to their order of superposition.

A 1. Various beds of grits; Yorkshire paving grits (very siliceous), micaceous sandstones (flagstones and roofing slates) with beds of shale, bearing on some hills three seams of coal.
2. Very rough millstone grit (240 feet thick on Pen-y-gent).
B. The top lime of the Ingleton Fells and the upper part of Wharfdale. It is of very variable thickness, and contains lead in Dod and Settron Fells.
C. 1. A thick bed of various shales with grits and flagstone beds interstratified. It contains a seam of coal on Noughtberry hill at the Garsdale, King's-cross, and ——— pits.
2. Various beds of paving grits, flagstone beds, and shales. In the lower half of this section are two or three very thin beds of limestone.
3. Yorkshire paving grit and shale.
D. 1. Limestone shale containing beds of Dent black marble.
2. Gray limestone without shale, &c. Lead has been found in this bed south-west of the summit of Ingleborough, and is at present procured on the north-west side of the hill.
3. (In some places) a very thin bed of clayshale containing a slender seam of coal.
E. Grauwacke, (clayslate, &c.?)

Heights of different points of these sections, with their bearings and distances from the summit of Ingleborough.

A. 1 & 2. Height of the *station* on the loftiest point of Ingleborough, 2374 feet above the Irish Sea.
B. Height of the upper surface of the top lime on the summit of Simon Fell (part of Ingleborough) one mile E.N.E., 2125 feet.
C. 1. Height of the under surface of the top lime (at a spring) half mile E.S.E., 2062 feet;—(at a spring dividing Simon Fell from Ingleborough) half mile E.N.E., 2061 feet;—(on Simon Fell) 1¼ mile E.N.E., 2035 feet.

feet. Height of some quarries of grit slate, ¾ mile S., 2090 feet;—(on Simon Fell, east side), 1¼ mile E. by N., 1968 feet.

C. 2. Height of the upper surface of a thin (15 to 20 feet) bed of limestone on Simon Fell, 1¾ mile E., 1608 feet. Height of another bed of limestone near the last, 1479 feet.

3. Quarry of thin grit slate, 1¾ mile E., 1427 feet.

D. 1. Height of the upper surface of the Dent marble bed, 1½ mile S. by W. (in the direction of Newby), 1467 feet;—1½ mile S. by E., 1368 feet;—1½ mile S.S.E., 1395 feet;—(on Simon Fell) 1¾ mile E.S.E., 1350 feet;—(near Selside) 2 miles E., 1270 feet;—(on Park Fell, part of Simon Fell,) 2¼ miles N.E., 1166 feet.

E. Height of the grauwacke (in the Ribble under New-Inn Bridge near Harton), 4¼ miles E.S.E., 740 feet;—(near the blue slate quarries at the south-east end of Moughton Fell) 4½ miles S.E., 1116 feet. (This is the loftiest point of the denuded grauwacke.) At Hunterstyle (near Cromack) 3 miles S.E., 1047 feet;—(under the huge grauwacke boulders near Austwick) 3¾ miles S.S.E., 705 feet;—(in Clapham beck, a patch a little below the recently discovered cave in the limestone,) 3 miles S. by E., about 600 feet;—(in the Greta, 3 miles above Ingleton,) 1¼ mile N.W., about 750 feet.

Dip of the beds above the grauwacke.—A comparison of the heights of C. 1. and especially of D. 1. to the *south* and *north* of the station on Ingleborough, indicates the dip of these beds to be northerly. Near Newby the height of D. 1. is 1467 feet; and on Park Fell, 3⅔ miles to the N.E. 1166 feet. A fall of 300 feet in a distance of 19,360 feet is equal to a dip of $0^\circ\ 53'\ 16''$; but as the line of maximum depression is probably more to the north, its angular value will exceed 1°.—(At the marble quarries east of Dent and N.N.E. of the *station*, the height of this bed was found to be 1030 feet*.)

Dip of the grauwacke beds.—In the Greta, 80° N.E.;—at Hunterstyle, and at the south-east end of Moughton Fell, 45° S. by E.;—on Swartmoor, 80° northerly.

Thicknesses of the beds.—A. 2. and perhaps a portion of 1. about 234 feet on Ingleborough.

B. The top lime is about 80 feet thick on Ingleborough and Simon Fell.

* Visually estimated the dip appears to be most rapid to the south of the summits of Ingleborough and Whernside.

C. On Ingleborough, Simon Fell, and Park Fell, about 700 feet thick.

D. As the upper surface of the grauwacke, &c. apparently diversified with hill and dale previous to the deposition of the superincumbent beds, is not a regular plane, the thickness of this section must vary in conformity. In some parts of Wharfdale it is very probably upwards of 1200 feet in thickness.

I have the honour to be, Gentlemen,
Your most obedient servant,

Leeds, Dec. 8, 1827. John Nixon.

III. *On the Thermal Waters of the Alps.* *By* R. Bakewell, *Esq.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

When we approach a range of lofty mountains, like that of the Pennine Alps, and observe the calcareous strata on the outer part of the range, bent and contorted in various directions; when we further observe beds of limestone and puddingstone alternating and placed in an elevated position, as we advance to the central part of the range; and that the beds of granite in the central part are frequently vertical; we feel assured that their present contorted or vertical position is not the original one. The opinions of geologists have been much divided respecting the cause or causes that have elevated mountains and given a vertical position to beds that once formed the bottom of the ocean. Those who maintain that subterranean heat has expanded and broken the solid crust of the globe, and has raised from vast depths the ancient bed of the ocean, appeal to a cause that is known to exist, and which seems sufficient to explain most of the various appearances which alpine regions present.

In opposition to this theory, it is asserted that there are no remaining vestiges of the action of subterranean fire in the Alps;—but this I am convinced is erroneous. It is true that from near the source of the Rhone, to the foot of the Little St. Bernard, there does not occur any known rock of a volcanic character, with the doubtful exception of some rocks in the valley of Sass, and in the Valorsine. I have examined various parts of this range on the northern side of the highest mountains in the Alps, along a line of one hundred and twenty miles; and though I could discover no indications of the action of subterranean heat in the rocks themselves, I was greatly surprised to observe, the numerous thermal springs that are abundantly gushing out at the feet of the primary mountains, near the junction of the mica-slate, or the dark schist passing into mica-slate with the lowest calcareous beds of that vast series of limestone strata, which forms the outer ranges of the Alps. Numerous as these hot springs are on the northern side of the Alps, and not unfrequent on the southern side also, it appeared to me remarkable, that they had hitherto been regarded as isolated phænomena; and their geological position had not been noticed. It is true, some of the warm springs in the Valais and in Savoy had been long known and visited, but the greater number have been discovered since Saussure published his *Voyages dans les Alpes;* and it appears probable, that they would every where be found near the junction of the primary and secondary rocks, were it not for *eboulements* that have covered them with a heap of ruins, or that torrents from the glaciers have mixed with them, and reduced their temperature. Since I visited Savoy in 1821 and 1822, another considerable warm spring has been discovered near the village of Chamouni, at the foot of a glacier; and in 1820 several thermal springs were discovered in that branch of the Alps which extends to Grenoble.

I shall here briefly enumerate the principal known thermal waters in the Pennine Alps, and add some observations and inferences, which I trust will be acceptable to several of your readers.

NATERS *in the Haut Valais.*—The warm spring rises under a rock of mica-slate on the north side of the Rhone. The temperature when I visited the place was 86° Fahrenheit, but it is variable from the intermixture with surface-water. At the time of the great earthquake of Lisbon in 1755, the mountain above the spring, I was informed, opened and threw out a considerable quantity of hot water.

LEUK *in the Haut Valais,*—situated in a deep gorge on the northern side of the Rhone. There are twelve springs, varying in temperature from 117° to 126°. These springs have been long known, and are visited by patients from various parts of Europe.

THE VALLEY OF BAGNES *in the Bas Valais.*—The warm springs in this valley were buried under a heap of debris from the fall of part of a mountain, which destroyed the baths, the village of Bagnes and one hundred and twenty inhabitants in the year 1545. The name of the valley is obviously derived from the baths. The temperature of the water unknown.

CHAMOUNI.—The thermal waters at this place have been discovered since I visited Chamouni in 1821. I have received no account of the temperature; baths have recently been erected. The situation is near the junction of mica-slate with the lowest beds of secondary limestone.

St. Gervaise,—situated on a deep gorge on the north-east side of Mont Blanc. The thermal water rises near the junction of mica-slate and limestone. The temperature 94° to 98°. This spring was discovered about the year 1806: it is very copious. Baths have recently been erected, and are much frequented.

Aix les Bains *in Savoy;* the temperature from 112° to 117°. The thermal waters rise in great abundance from two springs situated at the foot of a lofty calcareous mountain, and are near the bottom of the great calcareous formation that forms the outer range of the Alps: there are also numerous hot springs in the vicinity, which the Sardinian government will not allow to be opened. Of the mode of douching at these baths, I have given a particular account in the first volume of my Travels in Savoy, Switzerland, and Auvergne. The thermal waters of Aix were well known to the Romans.

Moutiers *in the Tarentaise.*—The thermal waters rise in great abundance from the bottom of a nearly perpendicular mass of limestone. From the position of this rock, and its connection with those on the opposite side of the valley, in which the hot springs rise, I have no doubt that it is the lowest calcareous bed in that part of the Alps; but its junction with mica or talcose slate is not here seen. The thermal waters of Moutiers contain about two per cent of saline matter, chiefly common salt. The process of extracting it, I have described in the Philosophical Magazine, vol. lxiii. p. 86.

Brida *in the Tarentaise.*—The thermal waters of Brida were noticed in the ancient records of Savoy, but they were covered during a sudden inundation of the valley, and their situation was concealed for many years. In the summer of 1819, another inundation, occasioned by the breaking down of the side of a glacier, laid open the spring again. The rock from which the spring rises, is a greenish talcose slate passing into mica-slate: it is in junction with limestone. The temperature of the water is from 93° to 97° Fahrenheit. The geological position of this spring is more obvious than that of any of the other thermal waters which I visited, being situated close to the steep bank of the river Doron, where both the rocks are laid bare. There are some warm springs on the opposite bank of the river which rise in limestone; but the temperature is lower, owing to an intermixture with common water.

Saute de Pucelle, or *Virgin's Leap.*—There is a very copious thermal spring rising from the bottom of a perpendicular rock near the Isere, between the town of Moutiers and St. Maurice

St. Maurice at the foot of the Little St. Bernard; but owing to the difficulty of access to it, I did not visit it, to ascertain its temperature.

Beside the above thermal waters in the Pennine Alps, various thermal springs were discovered in the adjacent Alps, near Grenoble, in the year 1820: and it seems probable, that a series of these springs might be found, were proper search made, extending westward to the thermal waters of the Pyrenees; for in this line we should approach the southern border of the volcanic district of France. On the Italian side of the Pennine Alps there are also thermal waters: the warm baths of Cormayeur and of St. Didier are situated almost immediately under the southern escarpment of Mont Blanc. I was prevented by the weather from examining the geological position of these springs: their temperature is stated to be 94° of Fahrenheit*.

The inference that may be drawn from the geological position of these thermal waters near the junction of the calcareous beds with mica-slate, or the dark schist which passes into mica-slate, is, that the waters do not rise from the upper strata, but spring out of the lower or primary rocks; and as they break out near the feet of the highest range of the Alps, that extend from the northern side of the Simplon through the Valais and Savoy into France, we may with much probability infer, that these mountains are situated over or near to one common source of heat, by the agency of which they were originally elevated, and their beds placed in a position nearly vertical. This inference is in some degree supported by the well attested fact, that the districts where the hot springs are situated are subject to great and frequent convulsions, particularly in the upper valley of the Rhone. In the year 1755 and 1756, at Brieg, Naters, and Leuk, the ground was agitated by earthquakes every day from the 1st to the 27th of February; some of the shocks were so violent, that the steeples of the churches were thrown down, the walls split, and many houses rendered uninhabitable: many of the springs were dried up, and the waters of the Rhone were observed to boil. At three different times the inhabitants abandoned their houses and fled for safety into the fields. It has been before mentioned, that the mountain above the warm spring at Naters opened during the time of the great earthquake at Lisbon, and threw out hot water; at the same period the warm saline springs at Moutiers ceased to flow for forty-eight hours. When the water returned, the quantity was said to be increased, and the saline impregnation was weaker. Former and more formidable agitations of the earth are recorded in the Haut Valais, particularly in the district where the principal hot springs are situated. The last earthquake of consequence in the Valais took place in January 1803.

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* Nearly all the thermal waters in the Alps emit sulphureous vapours, and are slightly saline, except the waters of Leuk, which have the highest temperature, and are inodorous and free from saline impregnation.

I am informed that several of the retired valleys on the Italian side of the Alps, at the foot of the central chain, are subject to earthquakes, during which the ground has opened or sunk down in various parts, though these effects have been too local to excite attention at a distance. From these facts it seems as reasonable to infer that the thermal waters of the Alps owe their high temperature to subterranean fire, as that the hot springs in countries that have formerly been volcanic, derive their warmth from an internal unextinguished, but quiescent source of heat. No person who has attentively examined the lofty granitic plain to the west of Clermont Ferrand in France, and observed the granite in various parts pierced through by ancient volcanos that have poured currents of lava over its surface, or seen other parts where the granite itself has been changed by its contiguity to subterranean fire, or upheaved and intermixed with volcanic rocks;—no one, I say, who has observed this, can doubt that the hot springs of Mont Dor and Vichy derive their high temperature from a source of heat situated beneath the granite mountains, though ages have passed away since the volcanos of that country have been in an active state: and the only proof of the present existence of subterranean fire in Auvergne, is to be found in the hot springs themselves*. Nor can any adequate reason be assigned for attributing the high temperature of the thermal waters in the Alps, to any other cause than to a source of subterranean fire under these mountains,—a cause which is sufficient also to have produced their original elevation. It is however proper to state, that in some of the mountains of the Alps the temperature may be slightly increased by a cause hitherto unnoticed. In the upper part of the secondary formations covering the granite, there are beds of gypsum, and this gypsum is anhydrous; but when exposed to air and moisture, it combines with water, and passes to the state of common gypsum: and during this combination we may suppose heat to be evolved, but the process must be extremely slow, and the heat evolved must be totally inadequate to raise the temperature of powerful streams to 126°. Saussure found the temperature of the water in the lower part of the salt mines of Bex, which are situated in the vicinity of gypsum, to be four degrees of Reaumur higher than the mean temperature of the earth. It is not improbable, though Saussure was not aware of the circumstance, that this small increase of temperature in the mines of Bex might be partly owing to the combination of water with gypsum: however, an increase of temperature it is well known, is observed in deep mines, far removed from the gypsum formation.

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* I visited the extinct volcanos of Auvergne in the spring of 1822; and in 1823 I published an account of my observations in the second volume of my Travels, accompanied with cuts, and a section and outline of the country near Clermont, which, as far as I know, was the first attempt to render in this manner the structure of this volcanic district intelligible to the general reader. I discovered the bones of large mammalia in the fresh-water limestone under the volcanic tufa of Mount Gergovia,—a fact at that time unknown to Messrs. Brongniart, Cordier, and Brochant, to whom I mentioned it on my return through Paris. I was therefore surprised to see it intimated in the Quarterly Review for October 1827, that Dr. Daubeny and Mr. Scrope were the only Englishmen who had given an account of the extinct volcanoes in France.

In reply to what I have advanced respecting the thermal waters in the Pennine Alps, it may be said that few thermal springs have been yet discovered in the northern range of the Alps which form the Bernese Oberland; but the difference in the geological structure of the two ranges will, I conceive, be sufficient to explain why hot springs are more rare in the latter, than in the southern range. Most of the highest mountains in the Bernese Alps are covered with secondary strata; and the valleys are chiefly excavated in these strata, or in enormous beds of sandstone and conglomerate, that form a thick intervening mass between the surface and the primary rocks, sufficient to obstruct the rise of thermal waters; for it has before been stated, that all the thermal waters in the Pennine Alps issue from the primary rocks, or near their junction with the lowest calcareous strata.

Hampstead, Dec. 8, 1827. ROBERT BAKEWELL.

IV. *On the Integration of Linear Differential Equations having Constant Coefficients and last Term any Function of the Indeterminate Quantity x.* *By* JOHN HERAPATH, *Esq.*

[Concluded from vol. ii. p. 425.]

WE shall now proceed to a few EXEMPLIFICATIONS of the formulæ.

Ex. 1. Suppose $n = 2$ then (17) becomes

$$y = e^{r_1 x} \left\{ c_1 + \int e^{(r-r_1)x} \left\{ c + \int \mathrm{X} e^{-rx} \right. \right.$$

the same as we have given in vol. ii. p. 420; for $A = -r - r_1$ by the well-known property of equations.

Ex. 2. Putting $n = 3$ our (17) gives

$$y = e^{r_2 x}\Big\{ c_2 + \int e^{(r_1 - r_2)x} \Big\{ c_1 + \int e^{(r - r_1)x} \Big\{ c + \int X e^{-rx} \qquad (19)$$

just as we should have it from (6) by putting for A its value $-r - r_1 - r_2$ and for r_1, which is a root of the reduced quadratic from the equivalent cubic, $r_1 - r$ found by (16). The solution however here given, exhibits to advantage the great superiority of our general investigation. Had we been contented with the process of successive eduction at first adopted, it would have been almost impossible to develop the simple and elegant law which reigns in the exponents.

Ex. 3. To carry on the reduction of the general formulæ to differentials of the 4th, 5th, &c. orders would be superfluous, the application being so very obvious. Let us therefore suppose that any two of the roots in (19) as r_1 and r_2 are equal. In this case because dx, dx^2, are understood as factors respectively to c_1, c, (19) is

$$y = e^{r_2 x}\Big\{ c_2 + c_1 x + \int^2 e^{(r - r_1)x}\Big\{ c + \int X e^{-rx}$$

or $$y = c_2 e^{r_2 x} + c_1 x e^{r_2 x} + \frac{c e^{(r - r_2)x}}{(r - r_2)^2},$$

supposing $X = 0$.

Ex. 4. Let us now imagine that all three roots are equal. Our (19) under these circumstances, it is evident gives

$$y = c_2 e^{rx} + c_1 x e^{rx} + \frac{c x^2 e^{rx}}{2} + e^{rx}\int^3 X e^{-rx},$$

or when $X = 0$

$$y = c_2 e^{rx} + c_1 x e^{rx} + \tfrac{1}{2} c x^2 e^{rx},$$

in which as before all three arbitrary constants appear. We have therefore the solutions directly with the full complement of arbitrary constants without any of the refined artifices with which D'Alembert found it necessary to aid the half-guessed imperfect solutions of Lagrange.

Demonstration of the Completeness of the preceding general Solution.

Because the roots $r, r_1, r_2, r_3, \ldots$ will admit of a great number of combinations of their differences, besides those we have assumed, it may reasonably be imagined that our general formula (17) or (18) not containing the whole, cannot be complete. Our attention will, therefore, now be directed to the consideration of this point. The formula most convenient for this object is (18), which is precisely the same as (17) but under a different form.

Now that part of (18) which consists of the sums of the exponentials clearly remains the same, whatever commutation takes place in the order of the roots: we must therefore apply our attention to the part affected with the successive integral signs. Taking the lowest integral sign, and integrating by parts,

$$\int e^{-rx}X = -\frac{e^{-rx}}{r}\left\{X + \frac{dX}{r} + \frac{d^2x}{r^2} + \frac{d^3X}{r^3} + \dots\right.$$

of which the general term is

$$\frac{d^zX}{r^{z+1}}$$

Hence employing the sign σ to signify *produces*, *gives*, &c.

$$\int e^{(r-r_1)x}\int c^{-rx}X \;\sigma\; -\int\frac{e^{-r_1x}d^zX}{r^{z+1}}.$$

And because the part under the integral sign is of the same form as before, the general term of the quantity will be $\frac{e^{-r_1x}d^{2z}X}{r_1^{z+1}r^{z+1}}$. Therefore

$$\int e^{(r-r_1)x}\int e^{-rx}X \;\sigma\; \frac{e^{-r_1x}d^{2z}X}{r_1^{z+1}r^{z+1}},$$

Pursuing the same course we at length have

$$e^{xr_{n-1}}\int e^{(r_{n-2}-r_{n-1})x}\dots\int e^{-rx}X \;\sigma\; \frac{d^nX}{r^{z+1}\,.\,r_1^{z+1}\dots r_{n-1}^{z+1}}$$

taking no notice of the algebraic sign. This may be regarded as the general term of the last number, and hence as the index of the form it assumes when all the integrations have been executed. But this term would manifestly have the same value and form in whatever order the roots $r, r_1, r_2, \dots$ be taken; and therefore the member itself would have the same value and form, and consequently the solution we have given comprehends all the varieties which can arise from the different arrangements that may be given to the roots, and is therefore the complete solution.

Failure of Lagrange's Method.

Having established the accuracy and completeness of our own method, we will now transfer our attention to that proposed by Lagrange, which has been received and relied on as correct by the scientific world for nearly half a century. That we may treat this method with perfect justice and fairness, we propose to work out as directed the identical example with which it is in all the works of the present day illustrated, and then show its absurdity by direct differentiation. The equation usually taken is one of the third order,

$$d^3y + \mathrm{A}d^2y + \mathrm{B}dy + \mathrm{C}y = \mathrm{X}. \qquad (20)$$

Suppose e^{rx}, e^{r_1x}, e^{r_2x}, or y_1, y_2, y_3 be the three partial solutions when $\mathrm{X} = 0$; then if we take

$$y = pe^{rx} + p_1e^{r_1x} + p_2e^{r_2x} \text{ or } = py_1 + p_1y_2 + p_2y_3$$

p, p_1, p_2 being instead of arbitrary constants arbitrary functions of x, these arbitrary functions may, it is conceived, be subjected to certain restrictions, and yet satisfy the conditions of (20). For instance in the value of dy in

$$dy = p\,dy_1 + p_1\,dy_2 + p_2\,dy_3 + y_1\,dp + y_2\,dp_1 + y_3\,dp_2$$

the first three terms are the same as would result if p, p_1, p_2, were constant coefficients; and Lagrange therefore assumes that he may put

$$y_1\,dp + y_2\,dp_1 + y_3\,dp_2 = 0$$

as one condition for determining the functions p, p_1, p_2, which leaves

$$dy = p\,dy_1 + p_1\,dy_2 + p_2\,dy_3$$

This being a second time differentiated becomes

$$d^2y = pd^2y_1 + p_1d^2y_2 + p_2d^2y_3 + dy_1dp + dy_2dp_1 + dy_3\,dp_2$$

which for similar reasons Lagrange again decomposes into

$$d^2y = p\,d^2y_1 + p_1\,d^2y_2 + p_2d^2y_3,$$

and

$$dy_1\,dp + dy_2dp_1 + dy_3dp_2 = 0$$

Differentiating once more we have

$$d^3y = pd^3y_1 + p_1d^3y_2 + p_2d^3y_3 + d^2y_1\,dp + d^2y_2dp_1 + d^2y_3dp_2$$

Substituting the several values of y, dy, d^2y, d^3y for these quantities in the proposed equation there results

$$\begin{aligned} & p\ \{d^3y_1 + \mathrm{A}d^2y_1 + \mathrm{B}dy_1 + \mathrm{C}y_1\} \\ & + p_1\{d^3y_2 + \mathrm{A}d^2y_2 + \mathrm{B}dy_2 + \mathrm{C}y_2\} \\ & + p_2\{d^3y_3 + \mathrm{A}d^2y_3 + \mathrm{B}dy_3 + \mathrm{C}y_3\} \\ & + d^2y_1dp + d^2y_2dp_1 + d^2y_3dp_2 = \mathrm{X} \end{aligned}$$

But because the parts multiplied by p, p_1, p_2 are assumed to satisfy each, the equation proposed deprived of its last term X they separately vanish, and leave

$d^2y_1dp + d^2y_2dp_1 + d^2y_3dp_2 = X$ which, together with

$$dy_1dp + dy_2dp_1 + dy_3dp_2 = 0$$

and $$y_1dp + y_2dp_1 + y_3dp_2 = 0$$

furnishes three several equations from which two of the quantities dp, dp_1, dp_2 may be eliminated, and the value of the third p, p_1, p_2 be obtained by a simple integration. And this performed with the others will afford us the values of p, p_1, p_2; and of course, according to Lagrange, the complete solution of the differential equation.

In order to investigate this with every possible regard to generality, let as before r, r_1, r_2 be the roots of the equivalent algebraic equation

$$r^3 + Ar^2 + Br + C = 0;$$

and let $$y_1 = e^{rx},\ y_2 = e^{r_1x},\ y_3 = e^{r_2x}$$

which give $$dy_1 = re^{rx},\ dy_2 = r_1e^{r_1x},\ dy_3 = r_2e^{r_2x}$$

and $$d^2y_1 = r^2e^{rx},\ d^2y_2 = r_1^2e^{r_1x},\ d^2y_3 = r_2^2e^{r_2x}$$

Hence the three equations give

$$r^2e^{rx}dp + r_1^2e^{r_1x}dp_1 + r_2^2e^{r_2x}dp_2 = X$$

$$re^{rx}dp + r_1e^{r_1x}dp_1 + r_2e^{r_2x}dp_2 = 0$$

$$e^{rx}dp + e^{r_1x}dp_1 + e^{r_2x}dp_2 = 0$$

from which eliminating dp_2 we obtain

$$(r^2-rr_2)e^{rx}dp + (r_1^2-r_1r_2)e^{r_1x}dp_1 = X$$

$$(r-r_2)e^{rx}dp + (r_1-r_2)e^{r_1x}dp_1 = 0$$

and then eliminating dp_1 from these

$$(r^2-rr_2-rr_1+r_1r_2)e^{rx}dp = X$$

or $$p = \frac{c+\int Xe^{-rx}}{r^2-rr_2-rr_1+r_1r_2}.$$

In the same way the other coefficients p_1, p_2 are found; but as this one will answer our present purpose, we shall not trouble ourselves to determine them. From this coefficient we obtain,

$$py_1 = ace^{rx} + ae^{rx}\int Xe^{-rx}$$

putting a for the numeral reciprocal. This is one value that must satisfy the proposed equation. Now in this value r

being a root of the equivalent algebraic equation, the part ace^{rx} must vanish when the successive differentials of the value are taken and respectively multiplied by A, B, C to answer the conditions of the given differential.

Therefore the part $ae^{rx}\int Xe^{-rx}$ must also satisfy the differential, that is

$$Cae^{rx}\int Xe^{-rx} = Cae^{rx}\int Xe^{-rx}$$

$$Bd.ae^{rx}\int Xe^{-rx} = Bare^{rx}\int Xe^{-rx} + BaX$$

$$Ad^2.ae^{rx}\int Xe^{-rx} = Aar^2e^{rx}\int Xe^{-rx} + AarX + AadX$$

$$d^3.ae^{rx}\int Xe^{-rx} = ar^3e^{rx}\int Xe^{-rx} + ar^2X + ardX + ad^2X$$

or $\quad (r^2 + Ar + B)\,aX + (r + A)\,adX + ad^2X = X$

the other four terms vanishing in consequence of r being a root. Putting for A its value $= -r - r_1 - r_2$, and for B its value $= rr_1 + rr_2 + r_1r_2$ according to the theory of algebraic equations, and restoring the value of a, the above equation becomes

$$r_1r_2X - (r_1 + r_2)\,dX + d^2X = (r^2 - rr_2 - rr_1 + r_1r_2)\,X$$

or $\quad d^2X - (r_1 + r_2)\,dX - (r^2 - rr_2 - rr_1)\,X = 0$

a linear equation of the second order with respect to X. This equation therefore limits X to a particular form (namely e^{bx} b being determined from $b^2 - (r_1 + r_2)\,b = r^2 - rr_2 - rr_1$), whereas it ought to remain indefinite. Consequently the solution which requires this limitation cannot be a solution to the equation proposed having X unlimited.

In looking round for the cause of Lagrange's failure we perceive that it must arise from the limitations which his equations of condition introduce. He assumes that the functions p, p_1, p_2 are arbitrary, and hence thinks the limitations he has given them will not affect the accuracy of the solution. If, however, we consider that e^{rx} is not a solution of the equation proposed, but of this equation deprived of its term X, it is obvious that the functional factor p has certain conditions to fulfil, namely, to render the solution e^{rx} of

$$d^3y + Ad^2y + Bdy + Cy = 0$$

a solution of the equation

$$d^3y + Ad^2y + Bdy + Cy = X$$

and if at the same time we consider that the difference between the solutions of the former and latter equation depends entirely on the influence of the quantity X, it is clear that p which transforms one solution into the other must also depend on X, and for this reason cannot be arbitrary. Consequently the very first assumption of Lagrange is erroneous. and the arguments which he founds on this assumption productive of error. It is most extraordinary that mathematicians have for upwards, I believe, of half a century suffered this to pass unobserved and uncorrected; and, as if to crown their conduct with absurdity, have actually praised Lagrange's method as the most perfect and general that could be given.

Verification of our own Solution.

The simple manner in which we have, step by step, deduced our general formula of solution cannot, we presume, leave a doubt as to its truth. But since we have shown the failure of Lagrange's method by differentiating one of his examples, it is but fair that we should apply the same test to a similar example given by our own method.

It is obvious by (12) and (16) that

$$dX_t = X_{t-1} e^{-xR_{t-2}} = X_{t-1} e^{x(r_{n-2} - r_{n-1})}$$

and in the equation just above (17), that

$$y = e^{xr_{n-1}} X_n$$

Therefore putting $n = 3$ we have

$$dy = r_2 e^{r_2 x} X_3 + e^{r_1 x} X_2$$

$$d^2y = r_2 e^{r_2 x} X_3 + (r_2 + r_1) e^{r_1 x} X_2 + e^{rx} X$$

$$d^3y = r_2^3 e^{r_2 x} X_3 + (r_2^2 + r_2 r_1 + r_1^2) e^{r_1 x} X_2 + (r_2 + r_1 + r) e^{rx} X_1 + X$$

If we multiply these values of d^2y, dy, y by $A(= -r_2 - r_1 - r)$, $B(= r_2 r_1 + r_2 r + r_1 r)$, $C(= r_2 r_1 r)$ respectively, it is clear, r_2 being a root of the equivalent algebraic equation, that the sum of the left-hand column will $= 0$. It is also obvious from the value of A, that the two terms which constitute the third column $= 0$. But the numeral part of the second column, putting for A and B their values, is plainly

$$r_2^2 + r_2 r_1 + r_1^2 - (r_2 + r_1) \cdot (r_2 + r_1 + r) + r_2 r_1 + r_2 r + r_1 r = 0$$

So that all the terms vanish except X, and leave X = X as it should be.

We might here develop some other properties of our solution, both as it regards integration and even the doctrine of algebraic equations; and we might show how to extend a similar method to linear equations having variable coefficients; but these things we think it better to reserve for another opportunity, more especially as we contemplate investigating the latter subject on much more general principles than we have employed in that of which we have been treating.

JOHN HERAPATH.

Errata in the preceding part of this paper, printed in the last Number of the Philosophical Magazine and Annals.

Page 424, last line, *for* $R^{t}{}_{-2}$ *read* R_{t-2}

425, in (16) for $r + R_1 + R_2$ read $r + R + R_1 + R_2$

425, in (18) for e^{xrn-1} read $e^{xr}\,n-1$

V. *On the Occultation of* β Scorpii, *September* 25, 1827. *By* E. RIDDLE, *Esq.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE double star β *Scorpii* has been occulted almost every lunation during the present year; but only the occultations in May, July, and September, occurred at such times as admitted of their being seen in this country. I happened to see that which took place on September 25th, at about six o'clock in the evening; and as it was not seen by any other person that I have met with, and was attended by some circumstances which I believe were unexpected, it seems desireable to ascertain, if possible, whether it was witnessed by any one else in this neighbourhood, and to what extent the phænomena differed from those which I observed.

Perhaps the insertion of this notice in the Philosophical Magazine may elicit a communication on the subject from some gentleman who observed it with means superior to mine, as I had merely a 30-inch achromatic, with a power of about 80.

The stars are about 14″ apart, the smaller one the more northerly; and though the elements of the occultations of both stars are given in the Nautical Almanac, and both marked there as visible at Greenwich, where I observed, which was in lat. 51° 28′ 53″ N., long. 8″ W., and about 120 feet below the level of the axis of the transit instrument, at the Royal Observatory, *the larger star was not occulted at all.* When I first turned the telescope on the moon, I saw both the stars very distinctly, a little to the eastward of the southern enlightened horn. The smaller one vanished behind the dark limb, a little before it reached the horn. I expected every instant as the larger one approached the enlightened edge, that it would be occulted by some of the luminous protuberances near the horn; but it glided steadily along, appearing in my telescope, nearly if not quite in contact with the enlightened limb. *It never* *was invisible;* I never lost sight of it, till I saw both it and the smaller one quite clear of the moon.

But the larger one was so nearly occulted here, that it must have been quite so at a very small distance to the north-eastward; and it would be curious, and not quite useless as connected with the question of lunar parallax, if it should be found that the occultation was observed where the star's apparent path was an accurate tangent to the moon's limb.

I am, Gentlemen, yours, &c.

Greenwich Hospital, Dec. 10, 1827. E. RIDDLE.

VI. *On the Crystalline Form of some Salts. By Mr.* E. F. TESCHEMACHER.

To the Editors of the Philosophical Magazine and Annals.

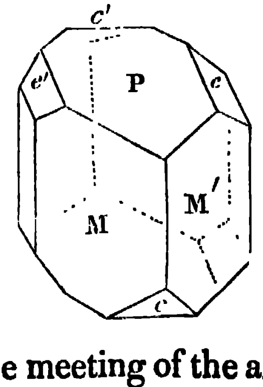
Gentlemen,

OBSERVING a paper in your Journal of December last, on some new double chromates, I beg to remark, that during an investigation which I made last winter, of some of the combinations of chromic acid with the different metals, I met with the same salts; and by an examination of their crystallographical character arrived at the same results as Mr. Henry Stokes, with regard to the combinations of sulphate of nickel and sulphate of zinc with chromate of potash, which, as they have not been before described, it may be interesting to detail.

Struck with the similarity in form of the crystals deposited by the solution containing nickel with some crystals of sulphate of nickel and potash given to me by Mr. H. J. Brooke, and described by him in the Annals of Philosophy, New Series, for December 1823; I subjected some very brilliant crystals of this new salt to an examination by Dr. Wollaston's reflective goniometer, which gave the following measurements.—Primitive form an oblique rhombic prism.

Chromo-sulphate of Nickel and Potash.

P on M or M′	101° 37′
P on *e* . . .	154 30
M on M′ . .	107 37



Sulphate of Nickel and Potash.

P on M or M′	102° 15′
P on *e* or *e*′.	154 32
M on M′ . .	109 10

A small difference in the meeting of the angles of these two salts will be observed. From this similarity of form I concluded this new salt to be a sulphate of nickel and potash combined with a small quantity of chromic acid, which is now further confirmed by comparing the analysis of the former salt made by Mr. Cooper, and inserted also in the same paper of Mr. H. J. Brooke, with that of the new salt made by Mr. Stokes.

Sulphate of Nickel and Potash.		Chromo-sulphate of Nickel and Potash.	
Sulphuric acid . .	18·95	Sulphuric acid . .	18·260
Oxide of nickel .	8·77	Oxide of nickel .	8·200
Potash	10·24	Potash	9·862
Water	12·04	Water	12·700
		Chromic acid . .	0·978
	50·		50·

Primitive form of both, an oblique rhombic prism.

Chromo-sulphate of Zinc and Potash.		Sulphate of Zinc and Potash.	
P on M	101° 47′	P on M . . .	102° 20′
P on *e*	154 31	M on M′ . .	108 40
M on M′	108 45		

This latter salt, the sulphate of zinc and potash, I formed by adding pure potash to a solution of sulphate of zinc, separating the precipitate, and allowing the clear solution still containing oxide of zinc to crystallize. Not having made the analysis of this latter salt, I am unable to compare it with the analysis of the chromo-sulphate of potash and zinc as given by Mr. Stokes.

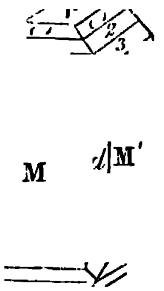
These are further instances in which the combination of minute quantities of matter vary the measure of the angles of crystals, and which show the value of Dr. Wollaston's goniometer in immediately detecting their existence.

I take this opportunity of sending you the measurements of the crystals of two other substances, which I am not aware have been previously given.

Primitive form a square prism.

Hematin.

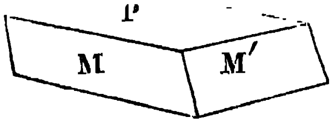
P on M	90°
M on M′ . . .	90
M on *d*	135
M on c^1	122 10′
c^2	118 15
c^3	116 15



This substance was found in the interior of a piece of logwood ready formed in a crystalline mass; I merely dissolved the crystalline part in spirit of wine and obtained the hematin in regular crystals.

Tartrate of Strontian. Primitive form an oblique rhombic prism.

P on M	....	92° 35′
M on M′	....	125 20



This salt was formed by adding a solution of tartrate of potash to one of nitrate of strontian.

On platina wire with borax before the blowpipe the chromo-sulphate of nickel and potash gives a transparent light brown glass; the chromo-sulphate of zinc and potash a transparent yellowish-green one. Yours, &c.

Barnsbury Park, Dec. 10, 1827. E. F. TESCHEMACHER.

VII. *Remarks on* Mr. J. Taylor's *Rain-gauge. By* B. BEVAN, *Esq.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN the last Number of your Magazine, at page 406, is a description of a rain-gauge by Mr. J. Taylor, in which a reference is made to my self-registering gauges, as described in a former Number *.

From the tenor of the paper, it may be supposed that the gauge invented by Mr. Taylor was an improvement upon mine. Now setting aside as far as may be, a partiality for our own inventions, I am not able to see in what respect it can be denominated a *registering* instrument: it may serve, and doubtless will, to point out the total quantity collected in the intervals of inspection, and so will the old simple floating index; but neither of them will show the *time* of *commencement*, the *duration*, and the *rate* of raining, for each shower,—which was effected by my gauges for some years: and although it was necessary to wind up the clock, and replace the paper on the cylinder once a week, it could not be considered any very objectionable trouble, considering the information it afforded; the whole operation might possibly occupy about five minutes, and as this occurred but once a week, it was not quite equal to the proportion of one minute per day.

I am aware, that to a gauge, such as is described by Mr. Taylor, additional apparatus might be applied, so as to render it a proper registering instrument; but at present it does not seem to possess any great superiority to the common floating gauges.

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* See Philosophical Magazine and Annals, vol. ii. p. 74.

The metals used by me in the construction of the receiver and conducting tubes, were, plate iron and tin, painted, to prevent oxidation; and they were found to last several years.

The above remarks are not intended to discourage improvements in such instruments, but to prevent an erroneous impression upon your readers.

I am, Gentlemen,
Your obedient servant,
Leighton, Dec. 10, 1827. B. BEVAN.

[We think that Mr. Taylor's expressions can hardly be considered as intending to propose his instrument to supersede Mr. Bevan's, or to be an improvement upon it.

He describes his object as indeed a different one, which may suit others as well as himself, and which is simply to have a rain-gauge which will record quantities between periods of observation,—if the term registering be objected to,—and without the care or management of persons skilled in such matters.

Mr. Taylor seems to us to acknowledge the merits of Mr. Bevan's instrument, and does not appear to have thought of any rivalry between the two inventions.—EDIT.]

VIII. *Reply to* F.R.S. L.'s *Remarks on Compound Interest.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

YOUR correspondent F. R. S. L. in his "Remarks on the Principle of Compound Interest," seems to have bewildered himself sadly in his attempt to defend Dr. Young's paper on that subject; nor is it in my power to extricate him out of the labyrinth. "It is," he says, "lawful to receive $\frac{5}{365}l.$ each day that 100*l.* is in the hands of the borrower, but at the end of the year it is only lawful to receive 5*l.* for the use of the 100*l.* without any interest on the interest."—All this is true if the interest is payable *yearly;* but if it is made payable at shorter intervals, what law restrains the lender from improving that interest during each interval, like any other part of his capital? The borrower pays only at the rate of 5*l.* per cent; nor does the lender improve his money at a higher rate, the advantage in his favour arising only from the more frequent opportunity of adding the interest to his capital. From the nature of compound interest, it is evident that the value of an annuity payable at shorter intervals than one year must vary less and less from the value of the same annuity payable yearly, as the term is lengthened; so that the values of the perpetuity in both cases must differ from each other by less than any assignable quantity, and therefore must be equal, agreeable to Dr. Price's theorems, and so far prove the correctness of his solution: but this plain and obvious truth appears to F.R.S.L. to be a *revolting paradox;* nor is it to be wondered at, considering his method of reasoning on the subject.

In attempting to prove that Dr. Price employs two different rates of interest in his method of computing, it is observed, that "the discount on ten shillings, receivable at a certain moment as a payment of the half yearly annuity, is greater in the computation than the discount on ten shillings receivable at the same moment as half an annual payment."

Now as the present value of ten shillings, or a moiety of 1*l.* to be received at the end of six months, and the present value of ten shillings to be received at the same time as the half-yearly payment of the annuity, are expressed by the same fraction $\frac{\cdot 5\cdot 35}{1\cdot 025}$, it would, I believe, puzzle any person, however well acquainted with the subject, to produce different results in this case.

At the conclusion of your Correspondent's remarks, Mr. Morgan's documents respecting the probabilities of human life are said to be "greatly calculated to encourage the popular delusion of the improved healthiness and greater longevity of the people of this kingdom." Had he indeed asserted, as Dr. Young states in his paper in the Philosophical Transactions, that only one in 1500 died in the year among the members of the Equitable Society, this accusation would have been just; but I should be glad to know in what part of his works has Mr. Morgan been so absurd? In the Nosological Table at the end of his Treatise on Annuities he represents the number of deaths in each year to be in the proportion of 1930 to 151,754, or as 1 to 78 nearly. By what rule in arithmetic these numbers are made to be in the proportion of 1 to 1500, I shall not stop to inquire.

F.R.S.

IX. *On the ultimate Composition of simple alimentary Substances; with some preliminary Remarks on the Analysis of organized Bodies in general.* By William Prout, *M.D. F.R.S.*[*]

[With two Engravings.]

THE present being the first of several communications on the same subject which I hope to have the honour of laying

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[*] From the Philosophical Transactions, for 1827, Part ii.

ing before the Royal Society, a few observations on the origin and object of the whole series may not be deemed irrelevant.

Many years ago I published an anonymous paper, containing some views, at that time new, connected with the doctrines of chemical proportions*. Though this paper, for reasons which need not be here stated, was drawn up and published in a very hasty and imperfect manner, it attracted some notice: and the views therein advanced gradually gained ground, and at present appear to be generally admitted in this country†. When this paper was published, it was my intention to have pursued the subject further, but I soon found my progress obstructed by insuperable difficulties. The first and chief of these was the want of accurate data; and the infinity of objects comprehended by chemistry prevented the hope of acquiring, by individual exertion, however unremitting, a sufficiency for the establishment of general laws. Professional duties still further limited my exertions, and at length obliged me to relinquish chemistry in general, and confine my attention solely to the chemistry of organized substances; a subject that has occupied the greater portion of my leisure hours for the last ten or twelve years.

Organic chemistry is confessedly one of the most difficult departments of the science; and though much has been done, and more attempted on the subject, it is yet in a very imperfect and unsatisfactory state; and it must be frankly admitted that physiology and pathology have derived less advantage from this most promising and really powerful of the auxiliary sciences, than might have been expected. To explain this perhaps would not be difficult; but as the explanation would be misplaced here, I shall merely observe, that, dissatisfied with the old modes of inquiry, I determined to attempt a different one, and keeping in view the notions I had originally formed respecting chemical combinations, proposed to myself to investigate the modes in which the three or four elementary substances entering into the composition of organized bodies are associated, so as to constitute the infinite variety occurring in nature.

With these views my first object was to determine the exact composition of the most simple and best defined organic compounds, such as sugar, and the vegetable acids; a point that had been several times before attempted, but, as it appeared to me, without complete success. About the same time also albumen and other animal products, as urea, lithic acid, &c. were examined with similar views. The subject of digestion, however, had for a long time occupied my particular attention; and by degrees I had come to the conclusion, that the principal alimentary matters employed by man, and the more perfect animals, might be reduced to three great classes, namely, the *saccharine*, the *oily*, and the *albuminous:* hence, it was determined to investigate these in the first place, and their exact composition being ascertained, to inquire afterwards into the changes induced in them by the action of the stomach and other organs during the subsequent processes of assimilation. In conformity with this plan, the object of the present communication is the consideration of the first class or family above mentioned, namely, the *saccharine.*

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* Annals of Philosophy, vi. 321, and vii. 111. (O. S.) The object of the second Paper was simply to correct some oversights in the first.

† Dr. Thomson's Chemistry, and his attempt to establish the first principles of chemistry by experiment. Also, Dr. Henry and Mr. Brande's Elements of Chemistry, &c.

Preliminary observations on the analysis of organized substances.

Vegetable substances contain at least two elements, hydrogen and carbon; and most generally three, hydrogen, carbon, and oxygen. Animal substances are still more complicated; and besides the above three, usually involve a fourth element, namely, azote, to which they appear to owe many of their peculiar properties. These general facts have been known ever since the elements themselves have been recognized as distinct principles, though the determination of the exact proportions in which they enter into any particular substance, has always proved a most difficult problem. To enumerate all that has been done on this subject would be loss of time; and it need only be mentioned, that all idea of separating the different elements from one another, so as to obtain them *per se*, has been long since abandoned, if indeed it was ever entertained; and the general principle on which the analysis of organic products has been usually conducted, has been to obtain their elements in the form of binary compounds, either by destructive distillation, as was formerly practised; or by combining the elements with some other element with which they possessed the property of forming definite binary compounds, from the quantity and known composition of which, those of the original elements might be readily obtained by calculation. For this latter purpose oxygen has been the principle usually employed, which, as is well known, forms water with hydrogen, and carbonic acid gas with carbon; two compounds not only as well understood as any in chemistry, but likewise, from their physical properties, well adapted for the purpose. When azote is involved, other means must be adopted, which will be fully considered hereafter.

The modes in which chemists have attempted to combine oxygen with the hydrogen and carbon of vegetable substances have differed very considerably. The illustrious Lavoisier attempted their union by burning the substance at once in oxygen gas, a method subsequently followed by Saussure and others. Afterwards the metallic oxides were employed for the purpose; and Berzelius in particular informs us, that so early as 1807 he had tried the oxide of lead, but did not succeed with it*. In 1811, Gay Lussac and Thenard published the analysis of different organic substances made by means of the chlorate of potash; and, considering the nature of the apparatus they employed, they obtained admirable approximate results†. Berzelius, in 1814, published an elaborate paper on the subject of vegetable analysis, in which he likewise employed the chlorate of potash, but in quite a different manner; and to this celebrated chemist I believe we are indebted for the improvement subsequently adopted by most of his successors, of introducing the mixture of the substance to be analysed, and the oxide, into a narrow tube, and submitting the different portions of it to heat in succession. The results of Berzelius were in general more accurate than those of his predecessors, especially as far as related to the quantity of carbon, but his method was not well adapted for determining the proportion of hydrogen‡. In 1816, Gay Lussac seems to have thought of employing the oxide of copper for the purposes of analysis§, the introduction of which undoubtedly constituted one of the greatest improvements hitherto made in organic analysis; and the use of which has continued to the present time, and will perhaps never be entirely superseded. The oxide of copper has however some disadvantages, which it is one object of the present remarks to point out; another is, to propose a form of apparatus free from most of the objections to which those hitherto in use have been more or less liable.

There are two methods of arriving at the quantity of water formed during the combustion of an organized substance; either actually to collect and weigh it, as Berzelius did, or to estimate the quantity by the loss of weight sustained by the tube after the combustion. The latter in general is the best method, and was that adopted by me from the first: it has since been followed by Dr. Ure, and others‖. Whichever method is adopted, it obviously becomes necessary that no extraneous water be present; but all pulverulent substances, and oxide of copper among the rest, are more or less hygrometric, and rapidly attract moisture from the atmosphere. This circumstance seems to have struck the French chemists, and it occurred to me at a very early period. Dr. Ure, however, was the first who published a method of obviating this difficulty; and his method, if this were the only difficulty to contend with, is capable of considerable precision. But there is another, and perhaps still more troublesome property, possessed by the oxide of copper, in common with many other powders, namely, that of condensing air as well as water *; and these two difficulties, taken together in conjunction with another mentioned in a note below, render great precision almost out of the question†. To conquer these, every means

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* Annals of Philosophy, iv. 403.

† *Récherches Physico-chimiques*, ii. 265.

‡ Annals of Philosophy, iv. 323.

§ *Annales de Chimie*, xcvi. 306.

‖ Phil. Trans. 1822. p. 457.

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* See Saussure's paper on the absorption of the gases by different bodies. Annals of Philosophy, vi. 241. Also Gilbert's *Annalen der Physick*, xlvii. 112.

† As I am unwilling that so much labour should be lost, particularly as it may be of some use to other inquirers, I have thrown into the form of a note a few of the principal circumstances connected with the inquiry mentioned in the text. In my earlier experiments, tubes of iron, copper, &c. were employed instead of glass, and charcoal instead of spirits, as the medium of heat; and during this period most of the modifications of apparatus which have been since proposed as novelties or improvements, were tried and rejected. I first took the hint of employing a spirit lamp from Mr. Porrett, and was certainly among the first that did so employ it. Various forms of lamps were tried, but at length I was induced to relinquish the use of the horizontal apparatus for the vertical one[a]; and this, I have no hesitation in saying, is by far the best form of apparatus hitherto proposed for the substances to which it is adapted; nor would any other have been required by me, at least, had it not been for the properties of the oxide of copper alluded to in the text, which render this and all other forms of apparatus depending on the employment alone of that substance perfectly useless when great accuracy is required. It has been objected to the lamp, that the heat produced by it is not sufficient; but this is a mistake; at least I have never met with any substance that resisted its action, provided the oxide of copper was well shaken up in the tube, or, if necessary, taken out of the tube and retriturated, and afterwards exposed to heat a second time, one or other of which *ought to be done in all instances*, whatever be the medium of heat employed; for no ordinary heat will induce the oxide to part with its oxygen to a combustible substance at some distance off, and not immediately in contact with itself. A great heat is also attended with some disadvantages, and among others, that of causing the oxide to adhere together in hard and solid masses, which thus becomes more difficult to be removed from the tube, and much less adapted for future experiments. In general, organized bodies are more difficult of combustion, and require more heat than crystallized ones. The lamp described in the text I have only recently employed, and it answers the purpose in all respects better than any I have yet seen.

With respect to the sources of error above mentioned, it was found that

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[a] Described in the Annals of Philosophy, xv. 190 (O. S.); and more completely in Dr. Henry's Chemistry, ii. 167, ninth edition.

that could be thought of, as likely to succeed, were tried, but without effect, and I was obliged to relinquish the matter in despair, and endeavour to contrive some other mode of analysis that should be free from these difficulties altogether. After a good deal of consideration I was induced to adopt a method which had occurred to me long before, but which I had never put in execution. This method is very simple, and founded on the following well known principles.

When an organic product containing three elements, hydrogen, carbon, and oxygen, is burnt in oxygen gas, one of three things must happen. 1. The original bulk of the oxygen gas *may remain the same*, in which case *the hydrogen and oxygen in the substance must exist in it in the same proportions in which they exist in water;* (for it is well known that oxygen gas by being converted into carbonic acid gas is not altered in its bulk): or, 2. The original bulk of the oxygen may be *increased*, in which case the oxygen must exist in the substance in a *greater* proportion than it exists in water; or 3. The original bulk of the oxygen gas may be *diminished*, in which case the *hydrogen must predominate.*

Hence it is obvious, that in the first of these cases the composition of a substance may be determined by simply ascer-

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200 grs. of the oxide of copper, recently ignited, gained, after ten or fifteen minutes, exposure to the air, a quantity varying from ·02 to ·05 gr. one half of which, or even more, was acquired before it became cold; that is to say, before it had cooled down to 100°, considerably above which point it began to acquire weight. Of the increase of weight above mentioned, it was found that about $\frac{1}{3}$, or $\frac{1}{4}$, (for the proportion varied from causes that I could not discover,) was due to the condensation of air, the rest was due to moisture. The oxide I employed was perfectly pure, and prepared by exposing metallic copper to heat. Dr. Ure states, "that 100 grs. of the oxide prepared from the nitrate of copper exposed to a red heat merely till the vapours of nitric acid were expelled, absorbed in the ordinary state of the atmosphere from ·1 to ·2 gr. in the space of an hour or two, and about half that quantity in a very few minutes."

In determining the quantity of water formed by the oxide of copper in the usual manner, there is yet another difficulty to contend with, to which we have alluded above, and which we shall here briefly notice. It has been stated that complete combustion seldom or never takes place during one exposure to heat, and that in many cases the oxide ought to be removed from the tube and retriturated. Now it was found, almost invariably, that during the second exposure to heat, the tube, instead of *losing* additional weight, actually became *heavier*, sometimes to the amount of ·03 gr. though often much less than this. I was a good deal puzzled to account for this anomaly at first, but believe that it arose chiefly from the reoxydation of the partially reduced oxide, by the air of the atmosphere.

Since this paper was read before the Royal Society, I have observed one or two other singular facts connected with this subject, which will be noticed when we come to speak of the analysis of substances containing azote.

taining the quantity of carbonic acid gas yielded by a known quantity of it; while in the other two, the same can be readily ascertained by means of the same data, and by noting the excess or diminution of the original bulk of the oxygen gas employed. Such are the simple and universally admitted principles on which the following method of analysis is founded; the only novelty in which, therefore, is the form of the apparatus; and of this it is hoped the following summary sketch, and annexed figures, will convey every requisite information.

Fig. 1. Plate I. represents a front view or elevation of the whole apparatus in the act of being employed. A B is a platform, two feet square, surrounded by a ledge about 2½ inches high, for preserving any mercury that may chance to fall about, and furnished with four adjusting screws (of which two, C C, are sectional views), by means of which it may be placed perfectly horizontal. Into this platform, in the manner represented, are fixed perpendicularly four square pillars, D E, D E, about four feet and a half high, at the top of which is placed another small platform, F F, about four inches wide, and which may be fixed or removed at pleasure by means of the brass pins *a b*, *a b*. I I are glass tubes graduated with the utmost care to hundredths of a cubic inch, and which are cemented at bottom into semicircular iron tubes inclosed in the blocks K K (as represented by the dotted lines). These iron tubes project a little below the wood at the lower part, where they are furnished with iron stop-cocks, S S, for drawing off the mercury when it may be necessary. Into the other end of these semicircular tubes are likewise cemented the glass tubes L L (of smaller dimensions, and a little longer than the tubes I I), and forming with them, when taken together, inverted syphons. The smaller tubes, L L, are represented as surmounted by funnels, R R, furnished with stop-cocks, the object of which is to permit the mercury to flow into them with any velocity that may be required. On the tops of the larger tubes, I I, are cemented the vertical stop-cocks, M M, of which fig. 2. Plate II. is a sectional view on a larger scale, and which renders the construction so obvious, that perhaps no further remark is necessary, than merely stating that the cup, *a*, is filled with oil, and that the plug, *b*, which is square at the upper part, and adapted to a key, is furnished with a shoulder, on which the screw-cap, *c*, rests, and by means of which it may be tightened at pleasure*.

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* These syphons are fixed together independently of the general frame-work, and may be removed at pleasure by taking out the pins *c d*, *c d*, and *e f*, *e f*. This admits of their being replaced by others of different sizes. Those of a larger size have balls near the top, as represented

On the platform, F F, (fig. 1.) is a thin piece of wood, capable of being raised or depressed at pleasure, by means of the screws, O O; on this the lamp Q is placed, which may thus be placed at any distance that may be required from the tube, P. Fig. 3. is an enlarged view of this lamp: it consists of two reservoirs, *d e*, for holding the spirit, connected together by means of the tube, *f*, into which are placed, at the distance from one another of about $\frac{1}{3}$ of an inch, a number of vertical burners, *g g*, &c. about $\frac{1}{12}$ of an inch in diameter, and $\frac{3}{4}$ inch long, and *made as thin* as possible, with the view of preventing the conduction of the heat. These burners are each furnished with a few threads of cotton, and are bent a little alternately like the teeth of a saw, in order that their flame may envelope the tube, P, (fig. 1.) more completely. *h* is a cover for the wick of the lamp when not in use. The tube, P, (fig. 1.) is of green or bottle glass, moderately stout, and about $\frac{1}{5}$ of an inch internal diameter. It is fixed between the horizontal parts of the vertical cocks, M M, so as to be perfectly air-tight; and when required, the whole, or any part of it, may be heated by means of the lamp, Q, at the pleasure of the operator.

When the apparatus is to be employed, both the syphon gasometers, I L, I L, are to be filled with quicksilver, and a small green glass retort, containing the requisite quantity of chlorate of potash, (and which had been previously heated so as to completely expel the common air, and to fill it with oxygen gas,) is to be attached to one of the cocks, as represented in fig. 4, by means of a caoutchouc tube. Heat is then to be applied, and any quantity of oxygen gas that may be required, introduced into the tube, I. After the whole has acquired the temperature of the atmosphere, the exact quantity of the gas is to be accurately noticed, as well as the state of the barometer and thermometer at the same time. The tube, P, containing the substance to be analysed, is then to be firmly fixed between the cocks, M M,* and subjected to heat, during which the oxygen gas is to be transferred from one syphon to another, through the red hot tube, with any velocity that may be required, and which may be regulated by means of one of the

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sented by the dotted lines, and may contain as much as 20 cubic inches of gas. It much facilitates the process of determining the exact quantity of gas contained in the apparatus, to have both legs of the syphon graduated, which may be easily done so as to obviate the effects of capillary attraction when the tubes are not both of the same calibre.

* I have tried various modes of connecting the tube so as to insure its being air-tight. Caoutchouc answers very well; but the best substance I have hitherto employed are slips of thin moistened hogs' bladder, tied on very tightly with fine *dry* twine. The twine is then to be moistened also, and the whole kept in this state till the end of the experiment.

the stop-cocks of the funnels, R R, and the stop-cock, S, of the opposite syphon.

Such is a general view of the apparatus, and the principles of its operation: but perhaps a few practical remarks on some of the circumstances to be attended to during its employment, may not be deemed superfluous.

The substance to be analysed may be placed in a small tray made of platina foil, and introduced alone into the tube P, and gradually submitted to the action of heat and oxygen gas; but this does not answer well with organic compounds, as a portion of them is apt to escape combustion. Another method is to mix the substance with pure siliceous sand, and to retain the mixture in the centre of the tube by means of asbestos. But this method often fails, except there be about an inch of the oxide of copper at each end of the tube, which must be kept red hot during the experiment, and in this case it succeeds completely with many substances. Another method, and that which the most generally succeeds, is to mix the substance with peroxide of copper, to heat these together in the tube in the first place, and afterwards to open the other stop-cock and send the oxygen gas through the ignited and partially reduced oxide, by means of which it again becomes peroxidized; and any portion of the substance that had escaped complete combustion in the first part of the experiment, is now completely burnt. This last method is also that employed when it is required to determine the quantity of carbonic acid gas yielded by a given quantity of any substance; only in this case, of course, oxygen gas is not required, and the contents of the tube P, must be taken out and well triturated, and subjected to heat a second time. If it should be required to analyse the gas formed, one method of removing it from the tube I, is represented at fig. 5; and others will readily occur to the practical chemist.

The following are some of the advantages of this apparatus, and mode of analysing organic compounds. In the first and chief place, *there is nothing to be apprehended from moisture.* Whether the substance to be analysed be naturally a hydrate, or in whatever state it may be with respect to water, the results will not be affected; and the great problem, whether the hydrogen and oxygen exist in the substance in the proportions in which they form water, or whether the hydrogen or oxygen predominates, will be equally satisfactorily solved, and that (of course within certain limits), independently of the weight of the substance operated on*. When however it is the object to ascertain the quantities of carbonic acid gas and water yielded by a substance, it is, of course, necessary to operate on a known weight; but this being once determined, there is no fear, as in the common methods, of exposing the substance to the atmosphere as long as may be necessary. The hygrometric properties of the oxide of copper, as well as its property of condensing air, are also completely neutralized; for the whole, at the end of the experiment, being left precisely in the same state as it was at the commencement, the same condensation of course must take place, and any little differences that may exist are rendered quite unimportant from the bulk of oxygen gas operated on, which of course should, in all instances, be considerably greater than that of the carbonic acid gas formed. Another advantage of this method is, that more perfect combustion is insured by it than by any other that I am acquainted with. There is also no trouble of collecting or estimating the quantity of water,—a part of the common process attended with much trouble, and liable to innumerable accidental errors besides those already mentioned, and which there is no method of obviating or appreciating: here, on the contrary, the results are obtained in an obvious and permanent form, and, from the ease with which they are thus verified, comparatively very little subject to error.

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* It is to be observed, that, throughout the experiments, great care is taken

It need scarcely be stated, that the form and principles of this apparatus render it well adapted for many other chemical operations besides the analysis of organized substances. Such, for example, as the reduction of oxides by hydrogen, and a variety of others that will readily occur to the practical chemist.

[To be continued.]

X. *Notices respecting New Books.*

Account of a New Work, entitled, A Treatise on the Steam-Engine; Historical, Practical, and Descriptive. By John Farey, *Engineer. London. Published by* Longman, Rees, and Co. in 4to. Price Five Guineas.

CONSIDERING the great extension which has been given to all branches of our national manufactures, by the application of steam power to expedite laborious operations, it is surprising that no competent engineer has hitherto undertaken to publish on this

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taken that the gases are *saturated with moisture:* the errors from this cause are thus rendered definite, and are easily corrected by tables calculated for the purpose from the most accurate data, and which will be given in a subsequent communication.

subject, in a detailed manner, to be instructive to men of business and practitioners. It is evident, from the repeated instances of dreadful explosions, failures, and misapplications of capital, which have happened (even recently), that engineers as a body are not yet sufficiently informed on this branch of their art. Whilst on the other hand, the continued success of the best engineers proves that real skill does exist amongst them, and may be expected to be attainable by others, with study and proper instruction.

As a means of such instruction, the present work cannot fail to produce public good.

The importance of a scientific guide to students and young practitioners cannot require to be pointed out. Merchants, manufacturers, and men of business, who must call in the aid of engineers to construct engines and apply steam power for them, may easily gather as much knowledge from such a work as will enable them to distinguish between those who are competent to execute what is required, and others who will be likely to lead them into similar errors to those by which so many have suffered. And it will be a most important aid to practical engineers, in directing those studies by which they may render themselves competent to execute what they undertake.

At present, many who are well acquainted with the mechanical structure of steam-engines, and are even accustomed to make them in established forms which they have copied from others, have only very imperfect and even incorrect notions of their operations when in use; they are in the condition in which a physician would be who had only studied the anatomical structure of dead animals, without much knowledge of the physiology and pathology of their living functions. Such engineers often succeed very well in their ordinary business, and acquire a reputation which their knowledge will scarcely bear out, when they are required to depart from their established models, and make such modifications of their parts as is necessary for applying steam power to new purposes.

To obtain these useful results from a publication, it must be composed with great care by a practitioner of competent knowledge and experience; for a self-sufficient guide in an unknown road would be worse than no guide at all: and but little could be expected from literary men, who can have no other acquaintance with the subject than what they may acquire in collecting information from others, merely for the purposes of publication.

Mere descriptions of steam-engines will not prove of any very great utility, even if accurate, and however they may be elucidated by drawings of such extent as can be added to any publication: for to general readers such descriptions will prove tediously minute, and to practical men they will afford no information which is not already habitually imprinted in their memory. To form an instructive guide, the descriptive part must be accompanied by accurate statements of the actual and relative magnitudes of all the parts, and of the quantities of matter and of action that each part is required to possess, together with such a complete development of the principles on which their operations depend, as can only be obtained from a very long and intimate acquaintance with the subject, and a natural habit of observation exercised in an extensive field.

Mr. Farey has been long known as one of those who possess the knowledge required for such an undertaking; and the great length of time which has elapsed since this work was first promised to the public, must have given ample time for its execution in a careful manner.

We have not had time to give this volume the examination that the subject demands, but the objects and general tenor of it may be gathered from the following extracts from the Preface.

" The object of the present treatise is to give a historical, practical, and descriptive account of the steam-engine, and its application to useful purposes.

" The historical part is intended to form a complete account of the invention, from its first origin, to its present state of perfection, from which the statesman and political economist may observe the influence that the adoption of steam power has exercised upon our national prosperity and advancement, during a century past, and may form an opinion of the future advantages to be expected from more extended applications of the same principles.

" The practical and descriptive part is intended to form a course of instruction for professional students, in the practice and principles of making and using steam-engines. At present, such students are left to form and digest their own crude and imperfect observations; and for want of a scientific guide, their conclusions are liable to be tinctured with many erroneous notions and false assumptions. The same part is also intended to form a manual, which, by the aid of tables and theorems for calculation, will facilitate the practice of experienced professional engineers; and will tend to perfect the practice of those engineers and others, who require to employ steam-engines, and apply them to various purposes; but who do not possess a complete knowledge of their construction, and therefore require information, which it has hitherto been difficult to attain. The principles of mechanical and chemical action, upon which the operation of steam-engines depends, and the application of those principles to practice, will be also embodied in this part.

" Another part will contain a record and brief explanation of all the speculative projects which have been proposed for the improvement of steam-engines; so as to exhibit to mechanical inventors the various ideas which have been suggested for that object: the instruction that they may obtain from other parts of the work, together with the history of the circumstances from which the great points of invention have originated, may enable them to make some further improvements.

" To attain these objects is not an easy task; for it is only in the course of an active professional practice, that sufficient information is to be obtained; and all competent engineers are too much engaged to find leisure for a literary occupation. The author could not have undertaken such a work, if he had not formed the plan, and collected materials for its execution, at his first entrance into business; and as it became known that he had such an object in view, he has continually received contributions of information from other engineers, who wished to promote the undertaking; and, with very few exceptions, this feeling has been general in the profession. In this way the author became personally acquainted with two great authorities in this branch of mechanics, the late Mr. Watt and Mr. Woolf, and received from them a full knowledge of the origin and progress of their respective inventions, and of the principles which they followed in applying those inventions to useful practice.

"At the commencement of his professional studies in the years 1805 and 1806, the author felt the want of a guide of this kind; and after carefully studying Dr. Robison's article in the *Encyclopædia Britannica*, and M. Prony's *Architecture Hydraulique*, and finding them insufficient for that purpose, he determined to preserve notes of all the observations and investigations, by which he should acquire his own knowledge of the construction and principles of operation in steam-engines and other machines, and their various applications; in the expectation that at some future period a useful publication might be formed from those materials. Professional avocations have long since occupied all the time which might have been devoted to such an object, but it has never been abandoned; and if the sale of the present work should prove sufficient to induce the publishers to undertake others of a similar nature, the author has an accumulation of notes, which in the course of years he may find time to arrange in a corresponding form with the present work.

"In the year 1815, the author drew up a descriptive article on the steam-engine for Dr. Rees' *Cyclopædia*; but the plan of that work, and the limited number of engravings, rendered it necessary to avoid details, which must constitute the great value of a practical treatise. Since the publication of that article in 1816, the want of a correct manual has been still more felt, from the great and increasing extension of the use of steam power, and the author was advised by his friends in the profession to resume his original project; this he undertook to do in 1820, and the bulk of the historical part was written, and most of the plates were engraved by the late Mr. Lowry, in the next year; but the author being obliged to reside at a distance from London, by engagements which left no leisure for this object, the impression has been carried on at intervals, and the publication of the first volume has been unavoidably protracted until the present time.

"To instruct students, it is necessary to state such elementary propositions in mechanics, as have a direct application to the subject of steam-engines; for this purpose a series of definitions are given in an Introduction to the present volume. These definitions have been formed from a full examination of the works of the best writers on the theory of mechanics, viz., Belidor, Emerson, Smeaton, Hutton, Banks, Gregory, Robison, Young, and others. The author has endeavoured to preserve their modes of reasoning, and the mathematical accuracy of their conclusions, without employing the language of geometrical or algebraical analysis; but all quantities are represented in numbers, and their proportions established by the ordinary processes of arithmetic: this plan has been adopted, in order to render the principles very apparent to those who are not accustomed to any other mode of calculus. This part of the work is intended to give practical men an exact knowledge of the true principles upon which their operations ought to be conducted; and other parts, to show the means of applying those principles to their daily practice, in the construction and use of steam-engines.

"To readers who are conversant with mathematical investigations, the mode of stating the propositions will appear to leave them without sufficient demonstration; but the principles which it is intended to explain and define (rather than to demonstrate) have been so well established in mathematical evidence, as to leave no doubt of their truth; and such readers may be referred to the mathematical writers whose principles have been adopted.

"Dr. Gregory's Treatise of Mechanics, in 2 volumes, 8vo., contains the best collection of mathematical investigations; and the fullest and clearest exposition of the principles will be found in Dr. Young's Course of Lectures on Natural Philosophy, in 2 volumes, 4to.; also Dr. Robison's articles on Mechanical Philosophy, in the *Encyclopædia Britannica*, reprinted by Dr. Brewster, in 4 volumes, 8vo. To the young student, a previous mathematical course is strongly recommended: the best guides are, Martin's System of Mathematical Institutions, in 2 volumes, 8vo.; Dr. Hutton's Course, in 3 volumes, 8vo.; and Dr. Gregory's Mathematics for Practical Men.

"One great object of the present work is to furnish practical engineers with a series of rules for calculating all proportions and quantities, which can be required to be known for the construction and use of steam-engines: these rules have been deduced from very numerous observations made upon steam-engines and mills of all kinds and of all magnitudes. In each case the observations have been very carefully compared, and assorted in series, according to the similarity of circumstances, and then such formulæ deduced from them, as would give results corresponding equally well with all parts of the series. The construction of these various formulæ has been a work of great labour, of which very little appears, because only the results of the investigations are retained in the form of an arithmetical rule. The greater part of these rules have been formed by the author for his own use, in professional practice; and having undergone the test of continual application during a course of several years, and received frequent corrections, he is justified in claiming some confidence in their accuracy.

"The principles which regulate the proportions of the different quantities which are to be computed by each rule, are stated in the most concise terms which could be chosen, without using algebraical substitutions; these have been avoided throughout the work, because the methods of algebra and fluxions are only necessary to investigate the formulæ, whereby computations may be performed in numbers, by the processes of ordinary arithmetic; and it is sufficient for practical use, to have rules which will give the required results.

"The method of performing each calculation by the sliding-rule is added, and will tend to facilitate the computations. This valuable instrument was introduced into considerable use amongst engineers by Mr. Watt, and only requires a good collection of formulæ to become of universal application. The author hopes that what he has done will contribute to extend the use of that excellent mode of computation amongst the profession.

"The history of the invention of the steam-engine and its application, is divided into chapters.—The first of which contains an account of the various projects and attempts which were made, during the seventeenth century, to obtain a moving power from fire; and a description of the first working engine which was invented by Mr. Savery for raising water, but it never came into extensive use.

"The second chapter is on the invention, principle, and construction of the fire-engine of Newcomen, which was the first engine brought into important use, and it is still very extensively employed. This subject is treated at length, and rules are laid down for the proportions of its parts.

"The third chapter is on the various applications of Newcomen's invention, which were made during the first half-century after its invention.

"The fourth chapter is on the introduction of cast-iron into the construction of machinery, and the application of the fire-engine to the manufacture of iron.

"The fifth chapter is a history of the origin and progress of Mr. Watt's invention of his first steam-engine for pumping water; with an account of that engine, and of the rules which he established for the proportions of its parts.

"The sixth chapter gives an account of the first application of the steam-engine to give continuous circular motion to mills; with a complete description of the principle, operation, and structure of Mr. Watt's rotative engine for that purpose; and the dimensions of several standard engines made by himself, which have been in use for years, and which perform as well as any modern engines which depend upon the same application of steam.

"The seventh chapter is a treatise on the construction and use of the sliding-rule, and its application to the purposes of calculations relative to steam-engines.

"The eighth chapter is a collection of rules for calculating the proportions and dimensions for all parts of Mr. Watt's rotative steam-engines.

"The ninth chapter is in continuation of the history of the invention of the steam-engine, and describes those modifications in the form of Mr. Watt's engine which were proposed and executed by his cotemporaries.

"The present volume concludes at that part of the history of the invention of the steam-engine, when it had been brought to such a degree of perfection that all its principles of action were fully developed and realized in practice; and, although the art of constructing these machines has been very greatly improved since Mr. Watt's time, and their applications widely extended, no important inventions have been established in practice, except high-pressure engines, and particularly those of Mr. Woolf, which are still used in the same form as he first made them.—The remainder of the subject will consist of technical descriptions of the structure of such steam-engines as are now in use, and as they are made by the best engineers; this, with their applications to various purposes, and the principles which should be followed in making such applications, will form the subject of another volume."

XI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Nov. 15.—DR. KIDD and Dr. Richardson were respectively admitted Fellows of the Society.

The Croonian Lecture, by Sir E. Home, Bart. V.P. was read, entitled, "On the muscles peculiar to organs of sense in particular quadrupeds and fishes."

The author selected for the subjects of this lecture the peculiarities in the muscular structure of the tongue of the Xariffa or *Camelopardalis Giraffa*, an inhabitant of Soudan in Africa; and a muscle belonging to the eye of the *Cobitis anableps*, a fish inhabiting the rivers of Surinam, and called by the natives "the four-eyed fish."

The tongue of the Giraffa, besides being an organ of taste, has many properties of the elephant's proboscis. The latter is incapable of elongation it is true, while the former may be extended to seventeen inches in length. The author observes, that some mechanism must exist by which this elongation can be performed, but that an opportunity of examination after death would be requisite to decide on its nature. The tongue of reindeer offers the same analogy, which however he has not been able to trace for want of time. The chameleon can dart out its tongue to the extent of twelve inches, and, for this purpose, as well as to direct its motion, it has a conical bone inclosed in a muscular tube, the fibres of which are circular, and aid by their pressure to make the bone slide forwards. The Xariffa wants the receptacle for water, which the camel and dromedary possess, it being needless for it as it lives on succulent plants; neither has it the padded hoof to fit it for travelling in sand, but two toes defended by a horny covering,

vering, to enable it to climb rocky ground without stumbling. Its long neck consists of only seven bones, being the same number that occurs in the human skeleton. The tongue is every where smooth and slightly adhesive; it is spotted, but the spots are not raised. Its favourite food is the Acacia tree, of that species now called *Acacia Xariffa*: it has a pleasant flavour both boiled and raw, and its twigs are succulent. The tongue being much exposed to the sun, has a black *rete mucosum*, to prevent blistering. Drawings, by Mr. Cross, exhibit the mode in which it lays hold of the branches of trees. It drinks milk, first rinsing out its mouth with a portion, and rejecting that so employed. It chews the cud, its body being then recumbent and its head and neck erect.

The organ of vision of the *Cobitis anableps* is very remarkable. The specimens examined by the author were furnished by Dr. Muttlebury, of Bath. He first exposes the errors of Artedi and his followers, in their description of this animal; and states, that the cornea being removed, the iris is exposed, having an appearance of two pupils; but on more accurate examination this effect is seen to arise from two lateral projecting portions, folding over each other in the middle, thus dividing the aperture into two. They do not however unite; and in some specimens they leave the pupil entire, only very narrow in the middle, forming an oval, broad above and narrow below; usually, however, they leave two distinct apertures. The crystalline, instead of being spherical, is not even circular, having a small projection at the lower edge, directly behind the smaller aperture. When examined in the microscope, a small bundle of muscular fibres is seen coming from the capsules of the vitreous humour in the lower part, and entering that of the crystalline just at the disc where the smaller curve joins the large one, the action of which is to bring the lower mammary process of the lens downwards and backwards into the centre of the lower apertures in the iris; thus constituting a complete organ for vision at near distances, independent of the part of the lens opposed to that large aperture, which is destined for more distant objects.

The author regards this structure as destined to a similar purpose with that of the *marsupium* in birds, viz. to obviate a difficulty arising from a want of motion in one direction in the eye-ball. He considers that by its means also, the fish, when lying with its eye-ball above the surface of the water, may enjoy distinct vision both in air and water by the motions of the crystalline and eye-ball, combined with the adapting power of the two apertures of the iris to a circular form.

A paper was read, entitled, "Experiments to determine the difference in length of the seconds' pendulum in London and Paris; by Capt. E. Sabine, Roy. Art., F.R.S."

The author commences this paper by a brief statement of the existing state of the determinations of standards of length in the two countries; and he observes that an attempt made by M. Arago in 1817 and 1818, to bring into immediate comparison the standards of the two countries, proved inconclusive, from the rates of the pendulums not having been obtained with sufficient exactness. The author having obtained from His Grace the Master-General of the Ordnance a general leave of absence from his military duties, so long as he could be usefully employed in scientific pursuits, conceived he could in no way better satisfy the condition than in carrying into effect this purpose. Accordingly, being provided with two pendulums,—one made by M. Schumacher, another the property of the Board of Longitude,—he set out for Paris, whither the pendulums were forwarded to him. The comparison was made in Paris at the Royal Observatory, in the Salle de la Meridienne, on the spot in which M. Biot's measurement had been made, and every proper facility and assistance was afforded him. The coincidence clock was compared every twelfth hour by M. Mathieu, with the transit clock of the Observatory. On the 27th of April, the weather having set in mild and steady, the experiments were begun. The results are stated in the form of appended tables, of which a detailed account is given.

Each of the pendulums, when not used in observing coincidences, was employed in determining its rate by a journeyman-clock or counter,—a method used by Messrs. Freycinet and Duperrey; but which the author thinks inferior to that of coincidences, though capable of giving good results. The particulars of these are given in two of the tables. From all the experiments in conjunction, it appears that the numbers of vibrations performed in a mean solar day at Paris (reduced as usual) by the two pendulums, were respectively 85922·06 and 85933·83.

The pendulums and apparatus were reconveyed to London, early in September, by water; and the rates again determined at Mr. Browne's house in Portland-place, by means of that gentleman's excellent clocks and transit observations made by Capt. Sabine. The precautions used are fully detailed; and the observations, which are also appended in a tabular form, the author being assisted by M. Quetelet, of Brussels. They give as a final result 85933·29 and 85945·85 for the numbers of vibrations respectively made by each in a mean solar day, similarly reduced for London.

As a final result of the whole operation, the author regards

12"·00

$12^{\mathrm{s}}{\cdot}00$ as the acceleration of the seconds' pendulum in passing from Paris to London. The same acceleration deduced from a comparison of M. Biot's and Capt. Kater's direct measurements of the seconds' pendulum, in Paris and in London, comes out $11^{\mathrm{s}}{\cdot}76$; or conversely, the length of the seconds' pendulum observed by the former in London transferred to Paris, by an assumed retardation of 12^{s}, gives a length differing from M. Biot's by $0^{\mathrm{in}}{\cdot}00023$. Borda's agrees within $0^{\mathrm{in}}{\cdot}00079$ with M. Biot's; and Capt. Kater's, so transferred, holds very nearly a mean between the two, but approaches rather nearer to Biot's than to Borda's.

Nov. 22.—W. A. Mackinnon, Esq. was admitted a Fellow of the Society.

A paper was read, entitled, "On a peculiarity in the structure of the *ductus communis choledochus* and of the pancreatic duct in man; by John Davy, M.D. F.R.S."

The peculiarity noticed by the author, consists of a valvular apparatus formed by delicate angular processes of the mucous coat of the lower part of the *ductus communis choledochus* and of the pancreatic duct; which he detected by slitting open these tubes under water, and washing off the adhering mucus. The effect of this structure is to prevent any retrograde motion in the fluids conducted by these tubes: and although a very delicate probe may be made to descend through the ducts with facility, its passage in the opposite direction is arrested by the *sacculi* which are formed by the processes of the inner membrane. This structure is not met with either in the sheep or in the ox, in which animals there is no junction of the biliary and pancreatic ducts.

Another paper by Dr. Davy was read, entitled, "Observations on the action of the mineral acids on copper, under different circumstances."

In prosecuting the researches into the slow operation of electro-chemical agency on the alloys of copper, which was the subject of his former paper published in the Philosophical Transactions, the author was induced to examine the action of the mineral acids on copper, under different circumstances. When atmospheric air was excluded, dilute sulphuric acid, into which a bar of polished copper was immersed, had dissolved at the end of three months only a very minute quantity of that metal, and the bar was slightly tarnished with the black oxide of copper. A similar result was obtained with dilute muriatic acid; but dilute nitric acid dissolved a larger portion of the metal, and the bar was encrusted with black oxide. When the vessels in which similar experiments were made, were covered only with glass, so as to retard evaporation but not to exclude the air, at the end of eight months the sulphuric acid was found saturated with copper, and the bar covered with a thin crust of black oxide. With nitric acid there was also a considerable deposition of protoxide of copper on the bar, together with a little crystallized subnitrate, and a very minute quantity of metallic copper. With the muriatic acid, depositions similar to those with the nitric acid took place, the submuriate being very abundant, and crystallizing as in the native specimens of this mineral from Peru.

The author considers the complicated results produced by the presence of atmospheric air as referrible to electro-chemical action, arising from the reaction upon each other of the combinations formed.

A paper was likewise read, entitled, "On the structure of the knee-joint in the *Echidna setosa* and the *Ornithorhynchus paradoxus;* by G. Knox, M.D.F.R.S. E., communicated by Sir James MacGregor, F.R.S."

After a short review of the labours of comparative anatomists on the animals which are the subject of this memoir, the author describes a peculiarity of structure which was discovered by his brother in the knee-joint of the *Echidna,* consisting of an extension of the *ligamentum adiposum,* or re-duplication of the synovial membrane transversely across the whole joint, dividing it into two cavities which have no distinct communication with each other. The articular surfaces of the upper cavity are the patella and the anterior portions of the condyles of the os femoris, while the lower are formed by the inferior and superior surfaces of these condyles, the upper surface of the tibia, and the semi-lunar cartilages. In the *Ornithorhynchus paradoxus,* the double fold of the synovial membrane extends only half-way across the joint, thus constituting an intermediate link of gradation between the *Echidna* and Man, in whom the *ligamentum adiposum* is wholly within the joint.

Nov. 30.—At the Anniversary Meeting of the Royal Society on St. Andrew's day,—after the names had been read of all Members deceased in the preceding year, and before the Medals were delivered, Mr. Davies Gilbert (President,) addressed the Society to the following effect:

Among the names now read, that of His Royal Highness the Duke of York demands our first attention.

We have in common with the whole nation to deplore the loss of an illustrious personage, who has rendered most essential services to his country by discharging the duties of a high military office during a period more arduous than any other in the modern history of Europe. But on topics so remote from our habitual pursuits it would be useless for me to dilate. Justice is done to His Royal Highness by the high station which his memory holds in the opinion and in the estimation of the public.

We have also to lament the loss of two of our Fellows, connected with the Society in the relation of Vice Presidents—the Earl of Morton, and the Bishop of Carlisle.

The first,—in addition to his own merits possessing a strong hereditary claim to our regards, as the descendant of Lord Morton who presided over the Society about sixty years ago,—assisted in our labours; and contributed his aid, with Lord Macklesfield and other distinguished persons, in 1752, to assimilate our Style or Calendar to that used by the continental nations of Europe. To the individual of whom death has now deprived us, we owe much gratitude for his uninterrupted countenance and attention during a long series of years: and in the *Transactions* for 1821 will be found a communication, by Lord Morton, of a curious fact in physiology.

Dr. Samuel Goodenough, late bishop of Carlisle, has ever sustained the character of a sound and elegant scholar. Entrusted with the education of distinguished personages, and having qualified them for the first situations in the state, he fairly and honourably ascended to the summit of ecclesiastical preferment. To classical and theological learning, Dr. Goodenough added a very intimate knowledge of natural history, as is manifested by a communication to the Linnean Society, where his labours have thrown a steady light over an extensive genus of aquatic plants, left by all former botanists in obscurity and confusion. The memory of Dr. Goodenough will long be cherished with affection and with esteem by all who had the honour of his acquaintance either in his public or in his private life.

If it were usual to advert in a particular manner to each Member of whom the Society has been deprived within the last year, there has not been read perhaps the name of a single person, on whom one might not dilate with a melancholy pleasure and satisfaction.—Mr. Canning, pre-eminent throughout the world; The Bishop of Winchester, senior Wrangler of his year, and tutor to Mr. Pitt; Mr. Cline; The Marquis of Hastings; The Duke of Gordon;—all men of literature or science. But as these and other individuals have not been connected in any peculiar relation to the Society, nor shared in its labours, we have only to mention them with regret.

gret.—Colonel Beaufoy may be considered an exception. That gentleman is well known to have devoted his time and attention to practical astronomy; and having confined himself to certain departments, in these he is said to have greatly excelled. His observations of stars occulted by the moon, and of eclipses of Jupiter's satellites, are believed to form the most complete series any where to be found; and such observations are very essential for completing the theories of these respective bodies. —And here it is not irrelevant to mention, that, by the liberality of his son, (professionally engaged in other pursuits and in other countries,) the instruments so well used by Col. Beaufoy are now bestowed on the Astronomical Society.

Two Fellows of the Society demand, however, our special notice—The Rev. Abram Robertson, and The Rev. John Hellins.

Dr. Robertson first appeared in the town of Oxford as a practitioner of medicine: but his abilities and mathematical attainments soon attracted notice. He was induced to become a member of the University; was admitted into orders; received a chaplainship of Christ Church; and became lecturer in geometry, first to the College, and afterwards to the University. In due course Dr. Robertson became professor of mathematics, and finally astronomer at the Radcliffe Observatory. We have in our *Transactions* various proofs of Dr. Robertson's diligence and abilities. Two papers "On a demonstration of the laws given by Sir Isaac Newton for expanding a binomial." This expansion, justly esteemed of the utmost importance as the foundation of every other, and as developing the whole system of fluxions, has received various elaborate demonstrations, the great author having simply contented himself with the annunciation.. Among those of the same date none are more clear or satisfactory than the one given by Dr. Robertson: nor does it detract from his merit, that subsequently others have been devised in the general expansion of functions, more concise, and perhaps more immediately urgent of conviction on the mind. We have a third paper "On the precession of the equinoxes." A fourth, showing "A direct method of computing the excentric from the mean anomaly." And a fifth, demonstrating "A theorem in spherical trigonometry, given by the late Dr. Maskelyne." But the great work of Dr. Robertson is his Treatise on Conic Sections;—following the geometric method of Apollonius among the ancients, and of Hamilton in our own times, by deriving all the properties from the cone itself. As an academic book for the instruction of young men this may well be stated as too extensive, and requiring more time than can now be allotted to any one branch of mathematical science. As a monument to the author's fame, it promises to remain for ages.

The Rev. John Hellins was one of those extraordinary men, who, deprived of early advantages, have elevated themselves by the force of genius and of industry to a level above most persons blessed with regular education. Mr. Hellins at one time computed for the Nautical Almanac. He afterwards assisted at Greenwich. And what is now perhaps almost unknown, he furnished the late Mr. Windham with all the calculations and tables on which that gentleman brought forward his new military system, as minister of war, in 1806. Mr. Hellins applied himself with great industry to some of the most useful branches of pure mathematics. No less than nine communications appear in our *Transactions.* "On the summation of series."—"On the conversion of slowly-converging series into others of swifter convergency."—"On their application to computing of logarithms, and to the rectifying of circular arcs."—"On the roots of equations." And in 1798, "On a method of computing with increased facility the planetary perturbations:" for the last he was honoured with your Copley medal. Retired to a small living in Northamptonshire, Mr. Hellins became a pattern of philosophic calmness and content

> Far from the madding crowd's ignoble strife,
> His sober wishes never learn'd to stray.

He seems to have said

> Curtatis decimis, modicoque beatus agello,
> Vitam secrete in rure quietus agam.

I have known Mr. Hellins above forty years, and I can testify to his virtues. It once happened that, through the late Dr. Maskelyne, I had nearly obtained for him the Observatory at Dublin. The failure cannot however be lamented, since Brinkley was appointed in his stead.

Although death has deprived us of several eminent persons at home; yet undoubtedly the greatest loss to science must be sought for this year in our foreign list. We find there the names of Bode, of Volta, and of La Place.

Professor Bode is known to every one by his magnificent Cælestial Atlas in twenty large plates, containing 17,000 stars laid down and catalogued with a degree of accuracy unknown to former times; and with an elegance and beauty that may never be excelled. This book appeared about thirty years ago; and the author is said to have employed himself up to very recent times in the cultivation of his favourite science, and in giving to the public some books explanatory of the elements, and others more profound; but all in a language little known in this country. He has now departed this life full of years, and with a reputation commensurate with his age.

Professor Volta has enjoyed the rare and enviable felicity of founding a new science. Mr. Galvani had indeed observed the extraordinary effects of peculiarly modified electricity, in exciting the nerves and muscles of frogs: but misled by the physiological hypothesis of a nervous fluid acting intermediately between the sentient principle and the material frame, he hastily concluded that the nervous fluid was now within his reach; and the appearances were denominated Animal Electricity. Nor can we perhaps justly blame Galvani for the generalization that he had formed. Volta himself adopted it for some time, as appears from his paper communicated in 1793: but further experience convinced him that the whole might be explained by electricity chemically produced; and this opinion has been satisfactorily demonstrated by the invention of his pile. Mr. Volta communicated a paper to the Royal Society as early as the year 1782, "On a method of detecting minute quantities of electricity." In 1791 he was elected a Fellow on the Foreign list. And in 1793 Mr. Volta transmitted to the Royal Society, through Mr. Cavallo, the account of Mr. Galvani's discoveries and of his own; which obtained for him the reward of your Copley medal. And in 1800 the *Transactions* were again enriched by a paper from Mr. Volta "On the electricity excited by mere contact."—To pursue the history of Voltaic or chemical electricity any further, would be to detail the successful labours of our own countrymen. Here the pile has been modified into the much more convenient and efficient form of the plates and trough which, in the hands of Sir Humphry Davy, have produced effects equally astonishing and important: and enlarged by the gentleman who now sits on my left hand*, the plates have exhibited such energies as were previously not even contemplated. Galvani has not affixed his name to the science of which he is in a great degree the parent, and which has continued to exercise his genius up to the extremity of a long life: but he has had the satisfaction of witnessing the continually increasing brightness of a flame first kindled by himself, and which he has never ceased to fan.

I now approach La Place. But it cannot be expected that I should give more than a very slight sketch of this extraordinary man.

La Place appears to have commenced his illustrious career in consort with a philosopher distinguished by the sagacity which enabled him to seize the clues leading to all the recesses of modern chemistry, by the indefatigable industry and acumen exerted in exploring every chamber of the labyrinth, and by the unfortunate period in which his lot was cast: a period devoted to an unrestrained action of the worst passions of the human heart,—which could have alone arrested, by a violent death, the progress of Lavoisier. Deprived of his friend and associate in chemical pursuits, La Place appears to have devoted the whole energies of his powerful mind to a science the most abstruse and difficult, but the most sublime of all that are placed within the reach of human intellect. Astronomy, so far as two bodies were alone concerned, had attained absolute perfection by the discoveries of Kepler and by the demonstrations of Newton.

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* Mr. Children.

The elliptic orbits, areas proportional to the times, and the squares of the periods, as the distances cubed, left nothing to be desired. But when the regular motion of a heavenly body round its primary is disturbed by a third, the circumstances are widely different. Sir Isaac Newton had indeed sufficiently shown that the principles of gravity and inertia were adequate to solving the problems of three bodies: but in a field so vast, the exertions of no one man, not even those of Newton, were sufficient for its entire cultivation. The labour of others, —of Bernouilli, of Clairaut, of Euler, of Mayer, of La Place, were required in aid; but still pursuing the system and plan dictated by their great master.

To describe the first important discovery of La Place, it will be necessary to premise some particulars. The problem of three bodies requires, From the momentary direction and intensity of the disturbing force, expressed in the generality applicable to all parts of the orbits, to infer the effects produced (through the medium of integration) in any finite time. It is probable that no effort of the human intellect could ever have attained this object, but for the expedient of imputing to the orbit a physical existence, and consequently a liability to variation in all its parts. The larger axis equal to twice the mean distance, the lesser axis indicative of the excentricity, the line of the apsides, and the inclination of the orbit and the nodes in respect to the orbit of the disturbing body. All these variations are expressed by expansions the most elaborate and complicated. But La Place has the glory of discovering, that after including every term of the expansion which involves the second powers of the excentricities, the larger axes, and consequently the mean distances, remain unchanged. All the larger terms in expanded series indicative of perturbations re-establish themselves in short intervals: but others of less magnitude are extended by the peculiar form of their coefficients through periods extremely long. Those measure what had previously been termed secular equations, supposed to vary as the squares of their distances from some assumed epoch. Since the *Méchanique Celeste* has enlightened the world, empirical equations have disappeared; and others corresponding with the true principles of rotary movements have assumed their place. Among these, as similar in their nature, may be included the two great equations of Jupiter and Saturn, each corresponding to the attractive power of the other planet, as is ascertained by the periods and distances of their satellites.

Descending to the peculiar system of our earth, La Place has deduced from gravity all the complicated inequalities of the lunar movements, and some of these involving the distance of the sun in comparison with that of the moon. The solar parallax has been derived from the lunar, with a degree of accuracy greater than can probably be obtained from observations on Mars, or even from that rare phænomenon, a transit of Venus.

Arrived at the earth itself, this illustrious philosopher has investigated with peculiar care the precession of the equinoxes, —an element of the utmost importance in all astronomical researches, caused partly by a displacement of the ecliptic from planetary attraction; but in a much greater degree by the attraction of the sun and moon on the oblate figure of the earth: the compression entered therefore as a main ingredient into these inquiries, to be considered under every probable variation of internal densities. The theory of compression has been perfected by La Place: but data were still wanting to reconcile all the anomalies caused by depositions or formations dependent on causes, to us, apparently fortuitous. These data, we hope, are either now attained, or at the least brought within our reach, by the ingenuity and perseverance of a gentleman who hears me *, in adapting to practical use the reciprocating property in pendulums between the axis of suspension and the centre of oscillation; a property long known, but never before applied. And by the unhoped-for accuracy recently attained in geodetical operations, by the use of measures possessing within themselves the power of adjustment for heat and cold, under the care of that distinguished individual who is at this moment conducting the trigonometrical survey of Ireland †.

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* Capt. Kater.

† Col. Colby.

The attention of our great philosopher was by a natural progress carried on to a consideration of the tides, which exhibit phænomena in apparent direct opposition to their cause; and which remained utterly incapable of solution till the discoveries of Newton disclosed the powers of centrifugal and centripetal forces. La Place, in the investigation of this most interesting, curious, and important problem, has not only taken into account the declinations and parallaxes of both the great luminaries, but also the oscillations of fluids in transmitting motion as connected with their depths. And hence he has been enabled to form a probable conjecture respecting the depths of our great oceans. From investigating the laws of Nature as displayed by the heavenly bodies, La Place descended to a subject not less difficult or intricate,—the action of particles on each other in capillary attraction, and in the transmission of sound. And in a separate work he has exhausted, by the most ingenious expedients, a subject scarcely less profound than either of the former,—the doctrine of probabilities. I do not notice separate memoirs and communications to Societies, most of which are embodied in his subsequent works. He is said to have contemplated a rigorous mathematical survey of the atomic theory, as developed by definite proportions. But the fate common to humanity has interposed. La Place has lived sufficiently long for his own fame: but no extension of his life would have satisfied the expectation or the demands of science. The time is not arrived, nor can I presume to assign to this extraordinary man his rank among those that are no more. Yet, speaking from this place and on this occasion, I will say, that Newton holds, and ever must hold, the highest station in the fane of philosophical renown. That to him

> Non viget quicquam simile aut secundum.

But although the second niche must remain unoccupied, yet one approximating to that of Newton will hereafter become the elevated station of La Place.

On delivering the Royal Medal for Sir HUMPHRY DAVY.

It is with feelings the most gratifying to myself, that I now approach to the award of a Royal medal to Sir Humphry Davy; and I esteem it a most fortunate occurrence, that this award should have taken place during the short period of my having to discharge the duties attached to the office of President; having witnessed the whole progress of Sir Humphry Davy's advancement in science and in reputation, from his first attempts in his native town, to vary some of Dr. Priestley's experiments on the extrication of oxygen from marine vegetables, to the point of eminence which we all know him to have reached.

It is not necessary for me more than to advert to his discovery of nitrous oxide; to his investigation of the action of light on gases; on the nature of heat; to his successful discrimination of proximate vegetable elements; nor to his most scientific, ingenious, and useful invention, the safety-lamp,—an invention reasoned out from its principles, with all the accuracy and precision of mathematical deduction.

The particular series of discoveries for which the Royal medal has been awarded, are those which develop the relation between electricity and chemistry.

Soon after Sir Humphry Davy had been seated at the Royal Institution by an invitation from Count Rumford, an invitation founded on his first production,—A paper on the nature of heat,—our late President began his experiments and investigations on electric chemistry: a most powerful Voltaic apparatus was fortunately placed at his disposal; and in his hands electric chemistry soon became the most important branch of practical science:. important from its immediate energies and powers; but much more so from the general laws of nature, which it has laid open to our view.

A new acidifying principle, or supporter of combustion, was discovered, possessing the same negative electric properties as oxygen. Muriatic acid disclosed its real composition. The oxymuriates were transferred to their proper class. The alkalies were reduced into metals; and the earths were proved to be similar oxides. But in the progress of these experiments a discovery was made, surpassing all the wonders attributed to alchemy. Three basins were arranged in a straight line, each containing water, and to the middle basin some neutral salt was added. The three were connected by moistened syphons of asbestos: the opposite piles of a Voltaic battery were then applied to the extreme vessels; and in a short time the neutral salt disappeared from the middle basin, and its constituent parts were found separated; the acid attracted to the positive pile of the battery, the alkali to the negative. This astonishing result, followed up by other experiments, led to the conclusion that chemical energies may be increased, diminished, or even inverted, by the superinduction of electric powers homogeneous with or dissimilar from their own. This metastasis in the hands of physiological inquirers promises to conduct them to discoveries of the utmost importance in the functions of life. I flatter myself that it is now actually in such hands.

The principle of varying or modifying chemical energies by those of electricity has been applied by the invention, in a manner the most philosophical, and on a scale the most extensive.

The copper sheathing of ships and vessels had been found to corrode in the short period of a single voyage, being converted into an oxide through the medium of some acid, or at least of a decompounded substance, occupying the negative extremity of the electric scale. The copper must therefore be positive in respect to tbe body decomposed or attracted. A reference was made by the Government to the Royal Society, with the hope of discovering some remedy for this most serious evil. Grounded on a perfect knowledge of chemical and of electric powers, it immediately occurred to the illustrious discoverer of their relations one to the other, that if a substance more positive than copper, and in contact with it, could be exposed to the corroding action, that the copper would, by induction, be rendered less positive, and therefore indisposed to combine with any other negative body.

Experiments the most satisfactory were then made on a small scale; and in consequence of their success, plates of zinc, and afterwards of iron, were applied to ships' bows; and the copper has been fully and completely protected. The theory and the experiments have been confirmed in the most ample manner. A defect has indeed occurred in practice, from the over success of protection. The induction of negative powers to the copper has gone too far; they have caused it to act on the compounds in an opposite direction, by attracting to itself the earths and alkalies, thus affording attachments to the marine vegetables which the copper was intended to prevent. This appears to me, however, susceptible of a cure. I am sufficiently advanced in years to remember the American-revolution war. Ships were then first sheathed with copper: they were preserved clean from weeds, nor was the copper corroded: but the ships were fastened together by iron bolts, and these, to the utter astonishment of every one, decayed; and the ships became unable to sustain the ordinary straining in gales of wind. For some time the effect could not be traced to its cause, for galvanism was then unknown; but at last bolts made of bronze were substituted for those of iron, and immediately the copper failed. When the theory has therefore been modified by experience on the principle of these empiric trials during the American war, I cannot hesitate in predicting complete practical success; with full glory to the illustrious individual who deduced the practice from theory, and with ample advantage to all those who may then bring the practice into beneficial use.

Sir Humphry Davy having last year communicated a paper to the Society in continuation of his former inductions and generalization on chemical and electric energies, there cannot be a doubt but that the only obstacle against his then receiving a Royal medal, on the first occasion that the Society had it to bestow, was his occupying this chair. That obstacle, unhappily for science, no longer exists; and the Royal Society take this earliest opportunity of testifying their high estimation of these talents and of these labours which all Europe admires. We trust and hope, although our late President has been induced by medical advice to retire from the agitation of active public stations, that his most valuable life will be long spared; and that energies of mind may still be displayed to this Society and to the civilized world, equal to those which have heretofore rendered immortal the name of Davy.

On delivering the Medal for M. Struve.

In no science has progressive dilatation and expansion been equally manifested as in astronomy. Limited at first to observing the phases of the moon; to conjecturing the return of eclipses after certain periods; to arranging religious festivals, or the times for agricultural labour by the heliacal rising and setting of stars,—it has ultimately extended to these stars themselves.

A most eminent individual of our own times may be considered as the parent of sidereal astronomy. The late Sir William Herschel, having doubted the extent of the solar system, leapt boldly beyond its comparatively narrow bounds, and laid open to our view suns mutually revolving round each other at distances from whence the orbit of the earth can subtend no more than a physical point. The same piercing eye distinguished between clustered stars, and spaces shining from collections of nebulous matter, destined perhaps in the course of ages to condense into a more solid form. These sublime pursuits are now actively continued, I am most happy to say, in our own country by a son worthy of such a father, and by a gentleman who, uniting energy, ability, and perseverance, allows no one to run before him in whatever he undertakes*.

The same subject is also investigating on the continent of Europe, with the attention and assiduity that it so well deserves,

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* Mr. South.

serves, especially by M. Struve, director of the Observatory at Dorpat. Having obtained the use of a powerful refracting telescope, this indefatigable astronomer has, in the short space of four years, alone and unassisted, produced this work,—A Catalogue of Double and Multiple Stars, to the amount of 3063 stars, "*Catalogus Novus Stellarum Duplicium et Multiplicium,*"—all laid down with an accuracy commensurate to the great labour and attention bestowed on them. The Royal Society have not hesitated in marking with their highest approbation such labour, such attention, and such ability, displayed by an individual who has thus established strong claims for gratitude on the astronomical world, and from a continued exertion of whose energies much more may with confidence be expected.

On delivering the Copley Medal to Dr. WILLIAM PROUT.

The science of chemistry, like that of astronomy, may reckon its different periods and distinct elevations. The decomposition of neutral salts; the discovery of gases; the decomposition of water; the application of voltaic energies, by the skilful hands of our late President; the atomic theory, with definite proportions. But nature is inexhaustible, and much more remains to be done. Although the ultimate elements of animal and vegetable substances are known to be few; yet their proximate elements, produced by unions of the former in different proportions and in different manners, continue indefinite. Besides the amylaceous, the saccharine, and twenty other principles widely diffused through the vegetable kingdom, there remain the essential oils and specific secretions; each resolvable into the few ultimate elements, and so readily changing the Proteus-like appearance of their proximate forms, as to baffle all but the most delicate and refined attempts to investigate their real properties and discriminating qualities, —qualities induced through the agency of life, and perhaps involving substances not subservient to the laws of gravity and inertia; the essential attributes of ordinary matter.

Much progress has indeed been made in separating particular substances from their combinations with others, to the great improvement of pharmacy and medicine.

But the Royal Society have viewed with peculiar satisfaction a new and accurate mode of analysis described by Dr. Prout, and founded on the most evident and simple principles; promising not merely to disentangle any one particular combination, but to afford an insight into all the products created by living chemistry. They have hastened, therefore, to stamp with the highest mark of their approbation, as well the mode of analysis itself, as the specimens of what, in the hands of Dr. Prout, it has already performed: and not doubting, but that by the exertion of such talents, such ingenuity, and such labour, their satisfaction will from year to year be continually increased.

On delivering the Copley Medal to Lieutenant HENRY FOSTER.

Of all the accidental discoveries ever made by man, the most unexpected and extraordinary, as well as most useful in its consequences, appears to have been the magnetic needle. No one could have thought it within the range of possibility to devise any plan, by means of which a ship in the midst of a wide ocean, surrounded with perfect darkness, and tossed by the winds and by the waves, might yet be able to ascertain its course with the same certainty as in open day, and under circumstances the most favourable. Yet the simple experiment of a child floating a magnetized needle on a cork, directly led to this important discovery. The variation must, without doubt, have been immediately observed. Columbus is said to have first noticed, and with astonishment and dismay, that the variation increased as he proceeded on his great voyage of discovery towards the West. Magnetism, a subject at once so curious and so useful, was attended to with such care, that the secular change of variation in the same place did not long escape notice. The dip was early known; and more than a century ago observations were made on the daily change of direction, and on a supposed relation which it bore to the appearance of northern lights, and to other natural phænomena. Nor were theories wanting:—some utterly gratuitous, as that conjectured by the celebrated Dr. Halley, who supposed arbitrary points of attraction, and an internal earth or revolving nucleus. Other theories, although given by less eminent men, appear to be more conformable to the true principles of generalizing by induction; as that quoted by Mr. Foster, from Derham in his Physico-theology. But the accuracy of modern experiment was wanting. The method of counting vibrations to ascertain intensity had not then occurred; nor were instruments, in all probability, to be procured that were accurately made, or of much delicacy of motion. In recent times, and by a member of this Society *, we have seen the local attraction of ships compensated on the most scientific principles; terrestrial direction neutralized; and the line of action, at least of diurnal variation, ascertained.

And we have seen phænomena little less astonishing than the one displayed by the original discovery of magnetism itself, in the connection that it bears to electricity and in the induction from rotary motion. It was important, however, that experiments should be repeated in different places, and especially in those which are most difficult of access, but situated near to the magnetic pole.

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* Mr. Barlow.

Lieutenant Henry Foster, well known to this Society by the cooperation he afforded to Captain Basil Hall in determining the number of vibrations made by an invariable pendulum near the equator, and at several other stations; having shared in the dangers of Captain Parry's second voyage, eagerly seized the opportunity afforded by a winter residence at Port Bowen, on the eastern side of Prince Regent's Inlet, in lat. 73° 14′, to ascertain the rate of an invariable pendulum, to conduct an elaborate course of experiments on magnetism; and in addition to these, observations on refraction.

One is utterly astonished at the magnitude of these labours, and at the accuracy and care with which they were conducted, (as is manifest from internal evidence,) in a situation where comfort and ease were unattainable, and where peculiar difficulties presented themselves at every step. It is impossible for me to give an abstract of Mr. Foster's most ample and detailed communication: I must refer every scientific inquirer to the paper itself. Among its important contents are:—The amount and times of daily variation attributable of course to the sun, but including in one series the action apparently caused by the moon.—The line on which a needle being directed the daily variation ceases.—A refutation of the supposed connection between tremors of the needle and aurora borealis.—The amount and times of daily variation in the dip; with a deduction from thence, according to the known law of the cosines, to the periodical change in horizontal intensity.—And a legitimate conclusion from all these facts, that the magnetic axis of the earth may probably describe a small curve, compounded of circles attributable to the sun and moon, of two or three minutes' radius round its mean place, which will solve the change of dip, of diurnal variation and of horizontal intensity; and may account for the secular variation in a manner similar to that which explains the precession of the equinox.

The Royal Society are of opinion that they do no more than strict justice, in awarding their Copley medal to the author of these observations and deductions; and not without a hope that by so doing, public attention may be more strongly drawn towards an officer possessing such abilities, energies, and preseverance *.

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* Never were expectations more speedily or more amply gratified; for on

After the delivery of the Medals, the Society proceeded to the election of a Council and Officers for the year ensuing; when on examining the lists, the following was found to be the state of the ballot:

Members of the old Council to continue.—Davies Gilbert, Esq. M.P. Capt. Francis Beaufort, R.N.; John George Children, Esq.; Sir Humphry Davy, Bart.; John F. W. Herschel, Esq. M.A.; Sir Everard Home, Bart. V.P.; Capt. Henry Kater, V.P.; John Pond, Esq. A.R.; William Prout, M.D.; William Hyde Wollaston, M.D. V.P.; Thomas Young, M.D. Foreign Sec.

Members of the Society chosen into the Council.—Francis Baily, Esq.; The Rev. William Buckland, D.D.; Charles Lord Colchester; John Wilson Croker, Esq.; William Henry Fitton, M.D.; The Rev. Edmund Goodenough, D.D.; John Guillemard, Esq.; John Ayrton Paris, M.D.; Peter Mark Roget, M.D.; Capt. Edward Sabine, Roy. Art.

Officers for the ensuing year:

President: Davies Gilbert, Esq. M.P.—*Treasurer:* Capt. Henry Kater.—*Secretaries:* Peter Mark Roget, M.D.; Capt. Edw. Sabine.

ASTRONOMICAL SOCIETY.

Nov. 9.—Mr. Baily presented a paper "On the right ascension of γ *Cassiopeæ.*" As this paper is a short one, and of an interesting nature we shall give it nearly in the words of the author:

"On comparing the Catalogue of Stars, recently published by this Society, with the Catalogue of 100 principal fixed stars given by Mr. Pond, at the end of the Nautical Almanac for 1829, I was struck (he says) with the considerable difference which appears in the Æ of γ *Cassiopeæ:* Mr. Pond making the Æ of that star upwards of one second (in time) more than the Catalogue printed by this Society. At first I imagined that some error might have crept into the calculations of the Society's Catalogue, notwithstanding they were made by two computers, independent of each other, and afterwards revised by our indefatigable secretary, Mr. Stratford. I therefore, for my own satisfaction, went through the whole computations myself, and was pleased to find that there was not the slightest difference in the results. I next reduced the whole of the observations of that star made by Mr. Pond at the Royal Observatory at Greenwich, and found them to agree very nearly with the result, deduced by Mr. Taylor, who makes the mean of 10 observations to be $= 0^{h}\,46^{m}\,13^{s},23$ reduced to Jan^{y}. 1, 1825: whereas the Society's Catalogue gives only $0^{h}\,46^{m}\,12^{s},13$ on January 1, 1825; being a difference, as already stated, of $1^{s},1$. Bradley has

on the very day that the medal was delivered, His Royal Highness the Lord High Admiral was graciously pleased to advance Mr. Foster to the situation of a commander, and to assign him a ship for a voyage of scientific investigations in the Southern hemisphere.

5 observations of this star, and Piazzi has 46. It is scarcely possible that Bradley should be in error one second of time: and still less probable that the mean of 46 observations by Piazzi should be erroneous to such an amount. The result of Bradley's observations (after allowing for the effect of precession) differs 8″,5 in space from those made by Piazzi: which quantity, divided by 45 (or the interval of years between the two observers), will make the annual proper motion of the star, if it really have any, about 0″,19 in space. But, this is not confirmed by the recent observations of Mr. Pond; and we must look elsewhere for a solution of the difficulty. That the position of a star of the third magnitude should be so undecided at the present day that its Æ cannot be satisfactorily depended on, to a second of time, does not speak much in favour of modern astronomy; and shows us that a great deal still remains to be done towards establishing the *fundamental* parts of the science. * * * Amongst other suggestions I have imagined that it might arise from a typographical error; and, in fact, if we suppose a misprint of 10″ in the Æ of γ *Cassiopeæ* in Piazzi's Catalogue, that is, if we read Æ=11° 11′ 17″,6 instead of 11° 11′ 7″,6, the whole difficulty will vanish, and the results of the observations of Bradley, Piazzi, and Pond, will agree to the greatest exactness. But, we are scarcely warranted in making such an alteration without a reference to the original observations. Some suspicion however is excited that the printing is not strictly correct, from the circumstance that Piazzi considers the annual proper motion in Æ, as deduced from Bradley's observations, to be = 0; which would agree with the amended reading as here suggested: but which does not accord at all with the present reading in the Catalogue, since the annual proper motion is, as I have already observed, in such case = – 0″,19.

"Before I close these remarks, I would observe that there are also differences in the Æ of three other principal stars (besides that of β *Scorpii*, which is acknowledged to be an error in the Greenwich Catalogue) to which I am desirous of calling the attention of the Society; and for which I can, by no means, account. These are ζ *Ursæ Majoris* and β *Cephei* (both stars of the third magnitude) and *c Draconis*, a star of the fifth magnitude. The two last differ, as in the case of γ *Cassiopeæ*, above a second in time from the Catalogue of Mr. Pond: but ζ *Ursæ Majoris* differs as much as 1^s,4. The case of this last star is the more remarkable, since the observations of Bradley and Piazzi correspond with wonderful exactness; there being a difference of only one second, in *space*, between them, after a lapse of 45 years: whereas from the time of Piazzi to the year 1825, a period of only 25 years, there appears, from Mr. Pond's observations, to be a difference of upwards of 20″.

"The whole of the computations relative to the positions of these stars, I have frequently repeated, and can assure the Society that there is no error in the results as printed in their Catalogue. Time only, and further observations, can clear up these apparent difficulties."

A paper was read "On double object-glasses; by M. Littrow."

The first part of this communication is devoted to the derivation of the equations expressing the conditions proposed by Mr. Herschel for the destruction of the aberration of sphericity in a thin double object-glass, by the ordinary processes in use among the German geometricians for such investigations. The author states himself to have entered on this investigation with a view to disseminate a knowledge of the theory alluded to in his own country; but being induced thereby to resume some former investigations of his own, he takes the opportunity to communicate to the Society his own principal results.

Taking for granted the well-known expression of Euler for the aberration of a lens of two surfaces, and developing it in descending powers of the distance of the radiant point from the lens, he obtains expressions, from which, by proper management, and substitution of similar quantities for a second lens, he derives the two final equations (A) and (z) demonstrated by Mr. Herschel. He then proceeds to the main object of his paper. This may be briefly stated to be the embodying of the relations expressive of the refraction of a ray through any four spherical surfaces, however situated, (provided they have a common axis), in trigonometrical equations, in which no quantity is regarded as small, and of course nothing neglected. These equations are in themselves sufficiently simple when undeveloped, and in that state may very readily be applied to determine whether any proposed construction of an object-glass really does satisfy the essential geometrical conditions of a perfect telescope, by producing a rigorous union of different coloured rays, and rays incident on different parts of the object-glass. Accordingly the author instances their application to a construction recently proposed by a German optician, as of peculiar excellence. In this construction the indices of refraction of the crown and flint lenses being respectively 1·53 and 1·60, and their dispersive ratio 0·25, the thickness of the crown lens 0·01, that of the flint 0, and the lenses being supposed in contact, the radii of the surfaces are

For the crown......	1st surface	(convex)	0·69281
	2nd surface	(convex)	2·255319
For the flint........	1st surface	(concave)	1·543030
	2nd surface	(concave)	5·768005

Substituting these data in his equations he finds that they satisfy sufficiently well the conditions of achromaticity, but that they are very far from being entitled to the same encomium when the spherical aberration is considered.

But when the question is inverted, and the problem is, not to try whether a proposed construction be good or not, but, *à priori*, to determine what is best, the equations in question, though simple enough in their trigonometrical form, become complicated by the algebraic developments their direct analytical resolution necessitates. Now the essence of M. Littrow's proposed method is to do away with all this development, so far as it tends to produce complication; and after preparing the equations in the most convenient manner the case will allow, to substitute for their direct algebraical, an *indirect numerical,* solution, by the well known and ready method of trial and error, a method perfectly correct in theory as well as easy in practice.

M. Littrow exemplifies this theory by applying it to that particular case when the indeterminate problem is limited by the condition of Klügel, viz. that the refractions at the first two surfaces shall be as small as possible, in which case the first or crown lens is in fact wholly given, and it only remains to determine the radii of the flint. He takes into consideration the thickness of the former, but not the latter, lens, and neglects also their distance *inter se.* As a numerical instance, he supposes the indices of refraction 1·53 and 1·58, the dispersive ratio $\frac{2}{3}$, and the thickness of the crown lens one hundredth part of its focal length, when, by applying the process described, he finds the following radii:

Crown lens. .	1st surface (convex)	0·186823
	2nd surface (convex)	0·608170
Flint lens . .	1st surface (concave)	0·407996
	2nd surface (concave)	0·445808

Focal length of the compound lens = 1 ; aperture 0·06495

M. Littrow, however, is by no means of opinion that the condition of minimum refraction here assumed is the best, or in any way essential to a good object-glass; only it facilitates calculation. He regards it as more advisable to aim at increase of aperture, and for this purpose to assume the first or crown lens equiconvex, which he affirms to be the condition requisite for the attainment of that end. He therefore explains his method of proceeding in this case, and applies it to the same numerical data with those last mentioned, which give for the final results:

Crown lens (equiconvex)	Radius of each surface. . . .	1·06
Flint lens (biconcave)	Radius of 1st surface	1·04394
	of 2nd surface. . . .	3·296512

Focal length 3·702231
Aperture. . . . 0·09973 × focal length

or nearly $\frac{1}{10}$ of the focal length, being a much larger proportional aperture than it has been usual hitherto to give to achromatic telescopes.

The author concludes by stating the reason of his entering into these investigations: viz. an application made to him by an artist of Vienna.

A paper was also read from M. Slawinsky, containing the following observations, made at Wilna. (Longitude $1^h\ 41^m\ 12^s$ East of Greenwich).

1. *Eclipses of Jupiter's Satellites.*

					Sidereal time.	
1825.	Jan. 17	Im.	1	..	$4^h\ 0^m 23^s,7$	good observation.
	Mar. 22	Em.	2	..	8 51 51 ,9	very good.
	Apr. 23	Em.	1	..	10 44 25	good enough.
	May 5	Em.	1	..	12 29 38 ,7	very good.
1826.	May 8	Em.	1	..	15 31 56 ,1	middling.
	June 2	Em.	2	..	16 46 0 ,1	middling.
1827.	Mar. 18	Im.	1	..	16 1 14 ,9	a little doubtful.
	Apr. 18	Im.	1	..	16 46 15 ,2	passable.

2. *Occultations.*

			Sidereal time.	
1825. Apr. 1	*e* Leonis....	Im.	$10^h\ 8^m\ 55^s,2$	very precise.
		Em.	11 15 14,2	a little doubtful.
23	Star (mag. 8·9)	Im.	11 22 29	exact.
1826. Feb. 15	208 Tauri	Im.	6 55 58,8	good.
16	Saturn			
	1st ansa	Im.	3 11 58,8	good.
	1st limb		3 12 8,8	
	2nd ansa		3 14 48,3	
1827. Mar. 18	λ Libræ......	Im.	17 4 55,9	good enough.

ZOOLOGICAL SOCIETY.

We extract from an Address delivered by Mr. Children at the Anniversary Meeting of the Zoological Club of the Linnæan Society, on the 29th November last, the following outline of the progress and present prospects of this new, but flourishing, Society.

"From this short sketch of what has passed under our immediate observation within these walls in the course of the last twelve months, I turn to what has been doing in another quarter, to which we all look with an interest and anxiety commensurate with the importance attached to the growth and progress of that young but promising child of British energy and science, the Zoological Society. It is a glorious feature in the philosophical character of Great Britain, that whilst in foreign countries, Science owes most of her success to the fostering care of Royal patronage, or the protection of executive power,—*here*, with faint exceptions, 'few and far between,' she relies on her own resources; and, unlike the creeping parasite, raises her head in independent dignity by the individual exertions of her disinterested cultivators, who, loving her for herself, seek only to accelerate her progress, and establish her empire in the human mind on the firm basis of immutable truth. To such an origin the Zoological Society may proudly assert its claim;—not one shilling has been drawn from the public purse for its support: and could it condescend to ask such aid, I for one would raise my voice against the humiliating petition—*Absiste precando, viribus indubitare tuis*. But it has not so forgot its dignity: it has relied solely on the liberal ardour of an enlightened people, and it will still rely on it;—nor will it rely in vain. The spirit of its immortal Founder has gone forth, and will not fail to light up in every heart capable of exalted feelings, some portion of that fire which animated his own; some wish, some sacred hope of treading, with however unequal steps, in the path he has so zealously marked out for them. In saying that not one shilling has been drawn from the public purse for the support of the Zoological Society, I must not be understood as meaning to imply that therefore its welfare is a subject of indifference to the gracious Monarch who wields the sceptre of these kingdoms, or the enlightened individuals whom, in his wisdom, he has summoned to his councils. That the very reverse is the fact, has already been confirmed by the exertion of Royal munificence in favour of the Society, and by its having at its head one of His Majesty's principal officers of state—a man, whose qualities of head and heart have rarely been equalled, never surpassed; and of whom both the Society and the British nation may honestly be proud. Such a Monarch and such a Minister will never be backward to further the interests of Science, when paramount claims shall happily cease to divert the current of national treasure into other channels, and when increasing prosperity shall relax the strict bands of public œconomy, by which their natural impulses are at present checked and circumscribed. If proof be wanting to support this assertion, we need only turn our eyes a short space northward, for indisputable evidence of the inclination of His Majesty's Government to further the views of the Zoological Society: and it is peculiarly gratifying to me to inform you, that in addition to the ground already allotted for the gardens and *vivaria*, final arrangements have been very recently completed, for the grant of the lake and its islands in the Regent's Park, for the purposes of breeding, rearing, and preserving water-fowl and other aquatic animals; and for a plot of ground for the erection of suitable offices and farm-yards, for breeding and domesticating poultry, &c. The right of *entrée* has also been granted to the Members of the Zoological Society, to the walks and ornamental grounds on the West side of the Regent's Park next to the lake;—all, privileges of essential importance to the Society, and gratifying proofs of the interest that His Majesty's Government takes in its welfare.

" As an accurate and sufficiently minute account of the valuable additions that have lately been made to the Society's Menagerie and Museum appears in the last Number of the " Zoological Journal," it would be superfluous to dwell on them in this place. I shall therefere merely state, that among the latter, stands conspicuous the extensive collection of its lamented founder, the late Sir Thomas Stamford Raffles, particularly rich in those rare animals, only lately known to science, from the eastern islands; as the male and female Proboscis Monkey (*Simia nasica*, Linn.)—a new species nearly allied to it,—the Malay Bear (*Helarctos Malayanus*, Horsf.); different species of *Tupaia*, and of the other new genera *Mydaus, Ictides, Gymnura*, &c. The Birds include most of the splendid species of Sumatra, particularly the gallinaceous fowls. Various new and interesting species are also found among the Fishes, Reptiles, Insects, and Zoophytes. Various other valuable animals have been added by the members and friends of the Society: but the most conspicuous of the late acquisitions is a fine specimen of the Ostrich, graciously presented by His Majesty. In the menagerie and gardens nearly two hundred living animals are exhibited in suitable paddocks, dens, and aviaries; as two beautiful *Llamas*, from the Duke of Bedford and Mr. Robert Barclay; a *Leopard*, the gift of Lord Auckland; *Kangaroos*, a *Russian Bear*, *Ratel*, *Ichneumons*, &c. &c.; besides a pair of *Emus*, *Eagles*, *Cranes*, *Gulls*, *Gannets*, *Corvorants*; various Gallinaceous Birds, and many others.

" The number of Members, whose names are inscribed in the books of the Zoological Society, amounts this day to 685."

XII. *Intelligence and Miscellaneous Articles.*

ISOPYRE,—A NEW MINERAL SPECIES.

THE following account of this mineral by W. Haidinger, Esq. F.R.S. E. &c. as well as the four following notices, we copy from the last number of Professor Jameson's Journal.

1. *Description.*—Regular forms not observed. Very pure masses of considerable size, often nearly two inches in every direction, occur imbedded in granite.

Cleavage none. Fracture conchoidal; highly perfect, where the mineral is pure; of lower degrees of perfection, where there are foreign admixtures in it.

Lustre vitreous, often considerable. Colour grayish-black and velvet-black, occasionally dotted with red, as in the heliotrope. Streak pale greenish-gray.

Opake, or very faintly translucent on the thinnest edges, with a dark liver-brown tint.

Brittle. Slight action on the magnetic needle.

Hardness =5·5. . .6·0. Specific gravity =2·912.

2. *Observations.*—Several specimens of the species of isopyre are preserved in the cabinet of Mr. Allan. Some of them are quite pure, and have no rock attached to them; others are imbedded in a kind of granite, chiefly consisting of quartz, crystals of which often penetrate the dark-coloured mass of the isopyre. Some of the specimens were procured by Mr. Allan three years ago, on a journey through Cornwall, in which I had the pleasure of accompanying him, from a miner in St. Just; others were given to Mr. Allan by Mr. Joseph Carne of Penzance, whose collection of minerals is particularly rich in the products of the western districts of Cornwall. The west of Cornwall is certainly the native country of the isopyre, but I am unable at present more accurately to indicate its locality, as I then considered the substance actually to be, what it was called, *black opal*, and, as such, much less interesting than it proved on more attentive examination, and omitted to take a note of the exact locality.

The resemblance of the isopyre to obsidian, or to what might be supposed to be the appearance of opal, when of a black colour, is very considerable; only the lustre of isopyre is less bright and glassy than that of obsidian. It is also very much like certain varieties of iron slag, and in fact it would be difficult to suspect the mineral not to be a product of the same kind of fusion which we are capable of producing in our own furnaces, if it were not associated with crystals of quartz, or did not contain, as in one of Mr. Allan's specimens, small imbedded crystals of tin-ore and of tourmaline. In allusion to this appearance, and also on account of the perfect similarity of a globule melted before the blowpipe, with the fragment employed in the experiment, I propose the trivial name of *Isopyre*, for designating the mineral, from ισος (equal) and πυρ (fire). The similarity of properties is even preserved in regard to magnetism,

netism, the globule obtained by exposing a fragment of the mineral to the blast of the blowpipe being magnetic, as well as the fragment itself, and even in a higher degree.

From the description given * of the Tachylite of Breithaupt, this mineral should much resemble the isopyre. Its specific gravity is much lower, being only 2·5...2·54, so as to preclude the possibility of their belonging to the same species. It occurs in basalt and wacke at Sæsebuehl, near Gœttingen, likewise only massive.

Dr. Turner has analysed the isopyre, and finds its composition to be

Silica	47·09
Alumina	13·91
Peroxide of iron	20·07
Lime	15·43
Peroxide of copper	1·94
	98·44

OSMELITE,—A NEW MINERAL SPECIES.

Professor Breithaupt, of Freyberg, gives the following account of this substance:

The name of this mineral is derived from οσμη (smell) and λιθος (stone). Its characters are as follows: Colour grayish-white, which passes into a tint between smoke and yellowish-gray. Planes, which have been exposed to the weather, have their colour changed into dark hair-brown. It consists of thin prismatic concretions, either scopiformly or stellularly arranged, and these again collected into coarse granular concretions, forming massive portions. Cleavage visible only in one direction, owing to the thinness of the prismatic concretions, which indeed pass into fibrous. Its form is conjectured to be rhomboidal; is strongly translucent: it feels rather greasy: its hardness, owing to the fibrous structure, is difficult to determine. It appears, however, from some trials on the file, to be intermediate between that of fluor-spar and apatite. Specific gravity = 2·792 to 2·833.

It gives out, in the common temperature of a room, a distinct clayey smell, which is increased by breathing on it, or when brought from a warm to a cold place. In the mouth it tastes like clay, and appears as if it would dissolve like clay, although no change takes place.

This species is distinguished from the zeolites by its greater specific gravity. It approaches to tabular spar in hardness and specific gravity, but in no other characters.

It occurs superimposed on calcareous spar, mixed with datolite,—in veins in trachyte, in a hill at Niederkirchen, near Wolfstein, on the Rhine.

HYDROSILICITE,—A NEW MINERAL SPECIES.

Dr. Kuh, in his inaugural discourse, entitled "*De Hydrosilicite, nova fossilium specie*, Berlin, 1826," informs us, that he found, in the serpentine of Frankenberg in Silesia, along with chrysoprase, opal, and pimelite, a mineral which he names Hydrosilicite. It is white, without lustre, feels greasy, translucent, fracture even, soft, does not adhere to the tongue, amorphous, and appears to be almost entirely composed of pure silica and water.

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* Leonhard, 2d edit. p. 781.

RUSSIAN PLATINA-SAND.

Professor Breithaupt has given the annexed mineralogical examination of this substance.

I was favoured by M. Schwetzau with a quantity of the platina-sand, washed out of the sand of Nijnotaguilsk, in the government of Perme, in Siberia. Of this Siberian sand there are two kinds: the one is ferriferous, and contains platina; the other, which is purer and more quartzy, afforded principally remarkably fine wash-gold.

The platina-sand, even at first glance, appears composed of grains of different kinds. I separated, by the eye, the following minerals: 1. *Platina.* 2. *Gold* 3. *Irid-osmine.* 4. *Silver-white flat grains.* 5. *Iserine, or magnetic iron-sand.*

The grains, from their appearance, could not have rolled far, and must have been found at no great distance from the place of their origin, for many of them are very sharp-edged, or even bristled with points.

1. *Platina-grains.*—I attempted to separate these from the iserine grains, by means of the magnet; but was surprised to find that not only the iserine, but also many of the platina-grains, adhered to it. I found that some of the platina-grains were magnetic, others not: hence these two kinds are probably varieties of two distinct species.

First species: *Common Platina.*—It is the same with the platina brought from America by Humboldt, and possesses the following characters:

Colour *platina-gray*, which is different from steel-gray. On concave places there is observed a yellowish appearance.—The grains are angular and bristled, seldom blunt-edged; the crystals are hexahedral, and grouped, as in silver-glance. Hardness =70·—8·5*. Is perfectly malleable. Specific gravity 17·001 – 17·608. A large American specimen in the Wernerian cabinet was 16·914. It is well known that the native platina is always lighter than that prepared by chemical means.

Second species: *Ferruginous Platina.*—The colour is *platina-gray*, but darker than in the preceding species. In hollows in the specimens, the surface is tarnished, from dark brown to black, as in meteoric iron. The grains and crystals have the same forms as in the former species.—Hardness =8·0—8·5. Malleable, but not so completely so as in the first species. Specific gravity 14·666 —15·790. It is magnetic, and in some grains not only repels, but also attracts. It is distinguished from the former species by lower specific gravity, less perfect malleability, and its affording, by chemical trials, a considerable portion of iron.

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* Scale of hardness here used is that of Breithaupt, in his Mineralogy, 7 = that of glassy actynolite, 8 = that of adularia, 9 = quartz.

2. *Gold.*—I found few grains of gold in the platina-sand: these were partly *gold-yellow*, partly *grayish-yellow*. Is Werner's grayish-yellow gold, gold combined with platina?

3. *Irid-osmin.*—This species, which is a compound of iridium and osmium, presents the following characters:

The colour is not steel-gray, as is generally believed, but a middle colour between whitish lead-gray and common lead-gray. It occurs crystallized in low hexagonal prisms, which have an axotomous cleavage. Hardness =8·0—8·75. Is imperfectly malleable. Specific gravity =17·969—18·571.

It would be desirable to have iridium and osmium again examined. Iridium will probably be found to possess a higher specific gravity than platina, and probably belong to the tessular system. The osmium, on the contrary, appears to belong to the electro-negative metals, which possess a hexagonal crystallization, such as arsenic, tellurium, and antimony.

4. *Silver-white flat grains.*—They appear to be palladium.

Concluding Remark.—In the portion of platina-sand I examined, the large half was ferruginous platina, the smaller common or true platina. The remaining grains composed about $\frac{1}{100}$th part of the whole.

In the Phil. Mag. and Annals for November, we gave a notice of Professor Ossann's having discovered various new metals in the Uralean platina. From the following account, by the same Professor, it appears that there are several varieties of it.

The platina, from ore of the Urals, is more varied in character than that found in America.—I have already been enabled to distinguish four different sorts, and I am told there are still more. One of the kinds, that which is most abundant, is sold at the mint in Petersburg. It consists of grains of different descriptions. Small grains can be separated by means of the magnet, resembling the magnetic grains in the platina of Brazil. The other grains are partly of a lighter and darker lead-gray colour, and about a line in diameter,—partly of a gold-yellow colour; and some are small, flattish, and shining metallic. In the following analysis I used the bluish-gray coloured grains. The following results were obtained in soluble matter:

Palladium	1·64
Rhodium	11·07
Platina	80·87
Copper	2·05
Iron	2·30
Sulphur	0·79
Trace of Iridium.	
Residuum	0·11
	98·83

Poggendorf's Journal.

NEW PHÆNOMENA OF VAPOUR OBSERVED BY CLEMENT DESORMES.

This philosopher communicated, on the 4th of December, to the Royal Academy of Sciences, some singular results relative to steam. "When compressed in a boiler, and issuing in a violent and hissing jet, through an orifice made in a pretty large plate, if a flat disk of metal be presented to it, at a little distance from the orifice, the disk is strongly repelled; but if it be brought near and placed against the plate, as if to close the orifice, although the steam issues on all sides like artificial fire-works, and presses against the disk more than before, not only is the disk not driven away, but it adheres to the plate even when the jet is directed downwards. It remains suspended in opposition to its gravity, and can be detached only by force. The same result takes place in an experiment with the wind which issues from the large bellows of a furnace."

NOTICE OF A FIRE-BALL:—BY THE REV. S. E. DWIGHT.

This meteor appeared on Saturday evening, March 21, 1813, a little before ten o'clock. The sky was extensively overcast, yet the covering was every where thin; and in the North where the meteor appeared, in various tracts of considerable extent, the stars were in full view. I was standing on a platform on the north side of the house, where I could survey the whole tract of sky over which the meteor passed. When the light first broke upon me, I was looking eastward, and for a moment supposed it to be a flash of very vivid lightning; but from its continuance was led almost instantly to look to the luminary whence it proceeded. The following are the observations which I made at the time with regard to it.

1. The meteor, when I first saw it, was about 35° above the horizon; and from the course of the fence near which I stood, I judged its direction, at that time, to be about N. 20° E.

2. Its figure was nearly that of an ellipse, with the ends in a slight degree sharpened or angular.

3. The length of the transverse diameter appeared to be about equal to the apparent diameter of the moon when on the meridian; and that of the conjugate, about three-fourths of the transverse.

4. The colour of the body resembled that of the moon, but was evidently more yellow.

5. A trail of light was formed behind it of considerable length, perhaps of ten or twelve degrees. It was broadest near the body, and decreased in breadth very slowly for about two-fifths of its length; after which it was an uniform stripe of light, about as wide as the apparent diameter of the planet Venus. The direction of the tail was coincident with that of the transverse diameter.

6. The ball was much more luminous than the tail, so that the end of the ball connected with the tail was scarcely less distinct in its form than the opposite end.

7. The illumination was so powerful, that all the objects around

me cast distinct shadows, though less strongly marked than when the moon is at the full.

8. Numerous sparks, of the apparent size of the smaller stars, but much more brilliant, were continually issuing from the ball of the meteor, and after descending a little distance, soon disappeared.

9. The length of time, in which the body was visible, was about eight, or possibly ten seconds.

10. A short time before its disappearance,—say one or two seconds,—three much larger sparks, or luminous fragments, were thrown from the body at the same moment. Two of these were apparently as large as the planet Venus; the third was still larger. These three were the last pieces which I saw leave the body. Their paths were at first nearly parallel with that of the meteor, yet beneath it. From this direction, however, they all deviated constantly and rapidly in parabolic curves, until they seemed falling perpendicularly towards the earth. Each fragment became less and less distinct, until it disappeared. The largest of the three continued visible until it was within about 20 degrees of the horizon.

11. The meteor itself disappeared as suddenly as if, in one indivisible moment, it had passed into a medium absolutely opaque, or as if, at a given moment, it had left the atmosphere; but a few moments afterwards there was a distinct and somewhat extensive illumination over that part of the sky for about a second, as if the light of the departing luminary had been reflected from some unknown surface to the earth.

12. When the meteor disappeared, it was about 30° above the horizon, and, as I judged from the course of the fence, in the direction of N. 45° E., or 25° eastward of the place where I first saw it. I concluded that the direction of its path was probably from W. by S. to E. by N. It was obviously going from me; its path making an angle with the optic axis of about 60°.

13. Not less than eight minutes, nor more than ten, after the disappearance of the meteor, there was a report very loud and heavy, accompanied with a very sensible jar. Though mistaken for thunder by those who did not see the meteor, it did not much resemble either thunder or the report of a cannon; but was louder, shorter and sharper than either, and was followed by no perceptible echo.

14. A friend of mine, who was in Berlin at the time, about 23 miles due N. of New Haven, saw the meteor distinctly, but made no particular observations concerning it. His estimate of it accorded generally with mine, but it appeared to him larger, more elevated, and somewhat more to the East in its apparent place.—I could not learn that the fragments which fell from it were discovered.—*Silliman's Journal*, vol. xiii. p. 35.

ON THE AURORA BOREALIS OF 26TH SEPT.: BY DR. FORSTER.

Boreham, Nov. 25, 1827.

Having seen the remarkable aurora which occurred on the 26th of September last, slightly mentioned in several of the journals, but no where accurately described, I send you the following brief notes on this remarkable phænomenon, which I made during its continuance through the night of the 25th, as it was seen by me and by others from the neighbourhood of Chelmsford in Essex.—I was passing the night at Boreham, and about nine o'clock P.M. I first noticed a very remarkable light in the North, the sky became intensely red in the N.N.E. and N., and afterwards very light all over the northern hemisphere. Clouds, however, soon obscured it for a time, and I returned to the house. At midnight on looking out of window, towards New Hall, I noticed the sky to be remarkably luminous, and on going out perceived every object as clearly as on a bright moonlight night. In the North the most brilliant streams of light shot up into the zenith from the horizon, generally in irregular diverging radii, whose centre appeared to be at nearly N.N.W. by N.; but other streamers of white light in other directions becoming alternately brilliant and faint appeared, while in E.N.E. and N.E. the whole sky seemed of a bright yellowish light: the red colour which I perceived at nine o'clock had quite disappeared, and the white light continued to prevail till it was lost in the day-light of the following morning. It is recorded that on two former occasions, one about twenty years since, and the other early in the eighteenth century, bright auroras happened on the same day; namely, 25th of September. I should like to have authenticated particulars of this fact, as it would establish a most remarkable coincidence of dates with the occurrence of this rare and beautiful phænomenon.

With regard to the aurora of the last 25th of September above described, I may observe, that I never saw distant objects so clearly on the brightest moonlight night as I did on that occasion at midnight; but its light more resembled that of the break of day than of the moon, and hence the name of Aurora has not unaptly been applied to this meteor. T. FORSTER.

LIST OF NEW PATENTS.

To R. Wheeler, of High Wycomb, for improvements on refrigerators for cooling fluids.—Dated the 22nd of November 1827.—6 months allowed to enrol specification.

To W. J. Dowding, of Poulshot, Wiltshire, for improvements in machinery for rollering wool from the carding-engine.—22nd of November.—2 months.

To J. Roberts, of Wood-street, Cheapside, and G. Upton, of Queen-street, Cheapside, for improvements on Argand and other lamps.—24th of November.—6 months.

To J. A. Fulton, of Lawrence Pountney-lane, Cannon-street, London, for a process of preparing or bleaching pepper.—26th of November.—6 months.

To J. Apsey, of John-street, Waterloo-road, Lambeth, for an improvement in machinery to be used as a substitute for the crank.—27th of November.—2 months.

To J. Jenour, jun., of Brighton-street, Pancras, for his cartridge or case, and method of more advantageously inclosing therein shot or other missiles for loading fire-arms.—28th of November.—6 months.

To T. Bonnor, of Monkwearmouth Shore, Durham, merchant, for improvements on safety-lamps.—4th of December.—6 months.

To W. Fawcett, of Liverpool, and M. Clarke, of Jamaica, for improved apparatus for the better manufacture of sugar from the canes. —4th of December.—6 months.

SCIENTIFIC BOOKS, &c.

A small work, entitled "The Circle of the Seasons," has just been published by Mr. T. Hookham, of Bond-street, in which the average day of flowering of most of our common garden plants throughout the year is noticed, under its respective day; and a short account of other phænomena is added, with some popular observations on the weather, &c. &c. The work is in one small volume 12mo., and intended for a daily companion to the popular botanist out of doors.

Polariscope,—an instrument for observing some of the most interesting phænomena of polarized light. Made and sold by W. Cary, Strand. The principle of this instrument was, we believe, first discovered and described by Biot, and its form we observe is nearly the same as one which has been described by Herschel. We conceive that Mr. Cary has rendered an acceptable service to science by making these instruments for sale, as it will place the important and interesting subject of polarized light within the reach of a greater number of inquirers than would otherwise have had an opportunity of studying it.

A Tabular View of Volcanic Phænomena, comprising a list of the burning mountains that have been noticed at any time since the commencement of historical records, &c. &c. By Charles Daubeny, M.D.F.R.S. Professor of Chemistry in the University of Oxford. This is an extremely useful accompaniment to the author's work on Volcanos. It consists of three parts: 1st, A list of the countries in which volcanos occur; 2ndly, A chronological list of volcanic phænomena; 3rdly, A view of the comparative heights of volcanic mountains. The just estimation in which Dr. Daubeny's work is held, is of itself a sufficient recommendation of the present tabular view, which in addition to the information already mentioned, contains an account of the geological nature of the various volcanic mountains.

A new weekly Medical Journal, under the title of "The London Medical Gazette," being a journal of medicine and collateral sciences, was published on Saturday, December 8.

A short series of Popular Lectures on the Steam-engine, by Dr. Lardner, Professor of Natural Philosophy in the new University, is announced for publication in a few days. The author professes to have stripped the subject of all its technicalities, and to have presented it in such a form that readers totally unacquainted with mechanical science may readily comprehend the construction and operation of the steam-engine, as well as the most interesting circumstances connected with the history of its invention and progressive improvement.

METEOROLOGICAL OBSERVATIONS FOR NOVEMBER 1827.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·40 Nov. 5. Wind NW.—Min. 29·28. Nov. 29. Wind N.W. Range of the mercury 1·12.

Mean barometrical pressure for the month . . . . 29·964
——— for the lunar period ending the 18th instant . . . . . 29·872
——— for 14 days with the moon in North declination . . . 30·057
——— for 15 days with the moon in South declination . . . . 29·687
Spaces described by the rising and falling of the mercury . . . 6·790
Greatest variation in 24 hours 0·730.—Number of changes 19.
Therm. Max. 62° Nov. 13. Wind N.—Min. 27° Nov. 22. Wind N.
Range 35°.—Mean temp. of exter. air 48°·42. For 30 days with ⊙ in ♏ 50·60
Max. var. in 24 hours 19°·00—Mean temp. of spring water at 8 A.M. 54°·82

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air on five different days . . . . 100°
Greatest dryness of the air in the afternoon of the 2nd 51
Range of the index 49
Mean at 2 P.M. 70°·0—Mean at 8 A.M. 79°·2—Mean at 8 P.M. 81·2
—— of three observations each day at 8, 2, and 8 o'clock . . 76·8
Evaporation for the month 0·70 inch.
Rain near ground 1·835 inch.—Rain 23 feet high 1·690 inch.
Prevailing Wind N.W.

Summary of the Weather.

A clear sky, 1½; fine, with various modifications of clouds, 9; an overcast sky without rain, 14; foggy 1½; rain, 4.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
15	10	27	1	15	19	12

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
6	3½	3	2	4	1	2½	8	30

General Observations.—This month has been calm and dry, excepting four or five days, and very mild for the season till the 20th; afterward it was mostly cold, foggy, and an extremely damp air.

At 20 minutes part 8 P.M. on the 4th instant, two coloured *paraselenæ* appeared, one on each side of, and 22° 39′ distant from, the moon's centre, as determined by the star marked ι (Iota) in the left heel of the constellation Auriga, which star was conspicuous through, and apparently centered in the paraselena on the northern side of the moon. These mock-moons were formed in a turbid-looking broad band of *cirrostratus* as it slowly passed the moon's horizontal rays.

In the afternoon of the 9th there was a curious appearance of nascent *cumuli* in a long connected range between Gosport and Portsdown Hill; they had the appearance of whitish columns of steam moving eastward near the surface of the earth with a gentle wind from N.W.

On the 11th, 18th, 19th, 25th, and 26th, the index of the hygrometer advanced to the extreme moisture point, or 100 degrees, the whalebone on these days being expanded to its utmost extent, by the deposition of dew and thick haze thereon. The dew deposited in the rain-gauge in the nights of the 17th and 18th amounted to *four hundredth* of an inch in depth.

From 11 till 12 P.M. on the 18th, a faint aurora borealis appeared in the horizon between the N.W. by N. and N. by E. points. A mild lemoncoloured light, and the segment which it described above the horizon, indicated the phænomenon, whose altitude did not exceed five degrees; nor was it accompanied by any perceptible coruscations during the hour of observation.

In the mornings of the 22nd and 23rd the ice lay pretty thick on the ground, and icy efflorescences appeared on the inside of the glass windows (being the first time this autumn), which were succeeded by a frosty haze on the latter day, and a light shower of snow in the evening. This was the earliest fall of snow observed here during the last twelve years. Several showers of snow also fell in Northumberland on the 21st, and much more fell between London and Manchester on the 22nd and 23rd.

The mean temperature of the atmosphere for November varies considerably in different years, sometimes to ten or twelve degrees. Sudden changes from a temperate air to several degrees below the freezing point, as was the case the latter part of this month, are found so powerful on some delicate constitutions, as to cause a general languor and a painful sensation of the flesh, which are mostly experienced at the setting in of piercing northerly and easterly winds immediately after mild currents of air from the opposite points of the compass. By some attention to this subject, it may be ascertained that colds and their calamitous train of consequences at the close of autumn and beginning of winter are more frequently produced by sitting in wet clothes and shoes, or in a brisk current of air when warm; or by going out of a hot room for any length of time in the dewy or foggy night air,—circumstances which seldom fail in obstructing the insensible perspiration of the body, than by a uniform decrease of temperature, however low, with the precaution of wearing suitable winter clothing.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are two *paraselenæ*, three lunar halos, an aurora borealis, two meteors, lightning in the evening of the 8th, and three gales of wind, two from the South, and one from the West.

REMARKS.

London.—Nov. 1. Fine day: some rain in the evening. 2, 3. Fine. 4. Foggy morning: fine day. 5. Cloudy. 6. Fine. 7, 8. Cloudy. 9. Drizzly. 10. Cloudy. 11, 12. Fine. 13—15. Cloudy. 16. Rainy. 17. Cloudy. 18. Foggy morning. 19. Cloudy. 20. Gloomy: cloudy. 21. An extraordinarily high tide this morning about 4 o'clock, doing very considerable damage: day fine. 22. Fine: a fall of snow about 4 P.M. covering the ground generally. 23. Cloudy: clear night. 24. A considerable fall of snow during the night. 25. Gloomy: calm: a gradual thaw. 26. Gloomy. 27. Fine. 28. White frost: fine, having rain in the night. 29. Fine. 30. Cloudy.

Penzance.—Nov. 1, 2. Fair. 3—5. Fair: showers. 6—9. Misty. 10. Fair. 11. Fair: rain. 12. Fair. 13, 14. Misty. 15—17. Rain. 18. Fair. 19, 20. Clear. 21. Fair: hail-showers. 22. Hail-showers. 23. Rain. 24. Showers. 25. Misty. 26. Fair. 27, 28. Rain. 29. Fair. 30. Misty. —Rain-gauge ground level.

Boston.—Nov. 1. Fine. 2. Stormy. 3, 4. Cloudy. 5. Fine. 6. Rain. 7. Cloudy: rain, A.M. 8, 9. Cloudy. 10—12. Fine. 13, 14. Cloudy. 15. Foggy. 16, 17. Cloudy. 18. Cloudy: rain A.M. 19, 20. Cloudy. 21, 22. Fine: plenty of ice for the first time this season. 23, 24. Fine: snow, P.M. 25, 26. Cloudy. 27. Foggy. 28. Fine. 29. Rain. 30. Cloudy.

RESULTS.

Winds, N. 2: NE. 1: E. 1: SE. 3: SW. 2: W. 3: NW. 13: var. 1.

London.—Barometer: Mean height for the month	30·154 inch.
Thermometer: Mean height for the month	43°
Evaporation	·83 inch.
Rain	1·32.

Meteorological Observations by Mr. Howard near London, Mr. Giddy at Penzance, Dr. Burney at Gosport, and Mr. Veall at Boston.

Days of Month, 1827.	Barometer.							Thermometer.							Wind.				Evapor.		Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost. 8½ A.M.	Lond.	Penz.	Gosp.	Bost.	Lond.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.											
Nov. 1	30·01	29·99	30·00	30·00	30·03	30·01	29·57	52	45	54	46	49	44	38	NW.	NW.	N.	NW.	...	...	·03	...	...	...
2	30·29	30·01	30·00	29·98	30·10	29·83	29·40	51	34	55	46	55	38	47	NW.	NW.	NW.	NW.	...	...	...	...	...	...
○ 3	30·36	30·29	30·10	30·06	30·10	30·10	29·70	53	43	54	50	57	47	43·5	W.	W.	W.	calm	...	0·10	...	...	...	...
4	30·50	30·36	30·18	30·15	30·25	30·17	29·72	54	45	55	49	58	46	48·5	W.	W.	W.	calm	...	...	...	...	...	...
5	30·50	30·46	30·25	30·20	30·40	30·32	29·77	56	43	55	50	60	51	52	W.	W.	NW.	W.	...	...	·04	...	...	...
6	30·46	30·36	30·30	30·30	30·34	30·26	29·75	53	48	56	50	58	47	47	NW.	W.	NW.	calm	...	·10	...	...	...	0·12
7	30·36	30·27	30·16	30·04	30·18	30·13	29·77	53	42	55	49	60	49	46	NE.	W.	NW.	E.	...	...	·03	...	...	·17
8	30·27	30·07	29·92	29·86	30·07	30·06	29·70	48	44	54	52	55	49	43	N.	S.	NW.	calm	...	...	...	...	0·015	...
9	30·10	30·07	29·76	29·72	29·86	29·78	29·46	49	43	55	52	59	48	46	SE.	SE.	NW.	calm	...	·10	·02	...	·025	...
10	30·10	30·09	30·02	30·00	30·02	29·90	29·50	53	47	55	51	57	49	48·5	NW.	NW.	NW.	N.	...	...	...	...	...	...
11	30·15	30·09	30·10	30·08	30·00	29·95	29·45	57	32	55	49	58	42	53	NW.	W.	NW.	calm	...	...	...	...	...	...
12	30·31	30·15	30·16	30·12	30·17	30·11	29·75	58	43	55	53	54	46	39	NW.	NW.	N.	NW.	...	·04	...	0·540	...	...
13	30·31	30·27	30·20	30·16	30·12	30·11	29·74	60	48	55	54	62	51	54	NW.	NW.	N.	W.	...	...	·14	...	·025	...
14	30·27	29·98	30·04	30·00	30·06	29·94	29·66	57	33	54	51	53	47	48	E.	NW.	E.	SE.	...	...	·18	...	·300	·11
15	29·98	29·67	29·92	29·54	29·74	29·51	29·36	48	36	53	47	52	49	42·5	SW.	W.	N.	calm	...	·06	·03	...	·400	·25
16	29·99	29·67	29·46	29·40	29·62	29·34	29·13	47	33	49	42	54	46	46	SE.	SE.	S.	E.	...	...	·12	0·085	·085	...
17	30·24	28·99	29·70	29·56	29·84	29·70	29 46	53	35	55	45	56	48	40·5	SE.	SE.	SE.	calm	...	...	...	0·350	...	...
18	30·40	30·24	30·00	29·90	30·10	29·97	29·70	50	46	56	47	57	48	46	N.	SE.	E.	calm	...	·10	...	...	...	·15
19	30·40	30·35	30·08	30·04	30·17	30·16	29·86	47	45	56	54	54	47	47·5	SE.	E.	NE.	calm	·47	...	...	...	...	...
20	30·37	30·35	30·10	30·10	30·14	30·05	29·70	47	40	54	50	51	44	46·5	SW.	E.	N.	calm	...	...	...	...	...	...
21	30·37	30·10	30·08	30·06	30·12	30·07	29·80	41	24	49	49	44	29	35	NW.	NE.	N.	NW.	...	·05	...	...	...	...
22	30·10	29·92	30·00	29·96	29·92	29·73	29·54	36	19	45	33	42	27	28·5	NW.	NE.	N.	NW.	...	...	...	...	...	...
23	30·30	29·84	29·74	29·54	29·68	29 56	29·55	31	20	50	38	40	30	28	var.	NW.	N.	NW.	...	...	...	...	...	...
24	30·20	29·84	29·90	29·86	29·97	29·79	29·55	34	24	46	40	38	30	28·5	NW.	NW.	NE.	NW.	...	·05	...	...	·200	...
25	30·48	30·20	30·04	30·04	30·02	29·92	29·70	37	36	50	40	46	42	34	S.	NW.	NE.	calm	...	...	...	0·760	...	...
26	30·55	30·48	30·22	30·20	30·32	30·20	29·96	48	25	50	46	48	39	34·5	NW.	NW.	NE.	calm	...	...	...	...	...	...
27	30·55	30·25	30·22	30·12	30·30	30·18	29·96	41	28	53	47	51	42	40·5	NW.	S.	S.	calm	...	·05	...	...	...	...
28	30·25	29·59	30·00	29·50	30·01	29·65	29·67	41	36	54	48	51	46	35	S.	SW.	S.	E.	...	...	·60	...	·610	...
29	29·69	29·59	29·54	29·50	29·50	29·28	29·10	50	36	55	48	53	47	41·5	NW.	W.	NW.	SE.	...	...	...	1·430	...	·59
30	29·59	29 27	29·52	29·44	29·48	29·44	29·20	54	48	56	49	56	49	41·5	S.	SW.	SE.	calm	·36	·05	·13	...	·175	...
Aver. :	30·55	29·27	30·30	29·40	30·40	29·28	29·60	60	19	56	33	62	27	42·3					·83	0·70	1·32	3·165	1·835	1·39

Fig. 2.

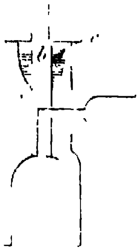


Fig. 3.

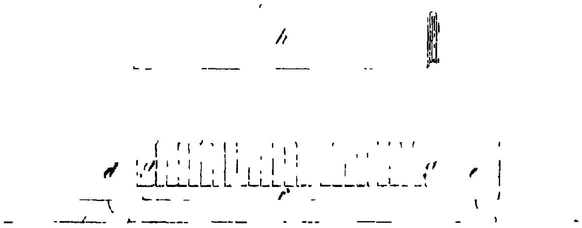


Fig. 5.

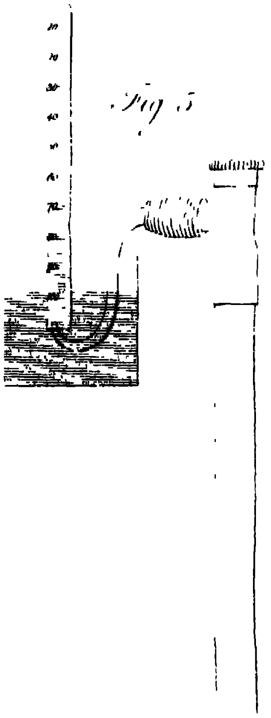
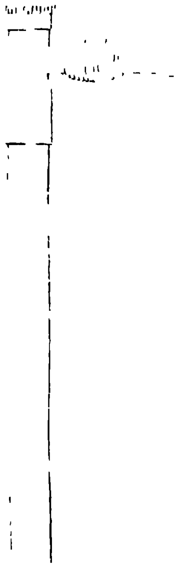


Fig. 4.



J. Porter sc.

THE
PHILOSOPHICAL MAGAZINE
AND
ANNALS OF PHILOSOPHY.

[NEW SERIES.]

FEBRUARY 1828.

XIII. *On the mutual Decomposition of Sulphate of Zinc and Chromate of Potash. By* T. Thomson, *M.D. F.R.S. L. & E. Professor of Chemistry in the University of Glasgow.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN the last Number of your Journal (for December 1827) I observed a paper by Mr. Henry Stokes "On some new double chromates." The three compound salts which he describes as double chromates, in fact contain merely a small quantity of chromate of potash, or rather of chromic acid in a state of mechanical mixture. They are the three salts which I have described in my First Principles of Chemistry, vol. ii. under the names of Potash-sulphate of zinc, p. 435,
Potash-sulphate of nickel, p. 434,
Potash-sulphate of copper, p. 436,
as any one may satisfy himself by comparing my descriptions and analyses of these salts with those of Mr. Stokes.

I shall take this opportunity of rectifying the account which I have given of chromate of zinc in my First Principles of Chemistry, vol. ii. p. 357. The salt which I describe as a chromate of zinc is in reality a dichromate of zinc, for it contains exactly twice as much oxide of zinc as I have assigned it. This mistake I discovered soon after the publication of my *First Principles*. And it induced me to examine carefully what takes place when solutions of sulphate of zinc and chromate of potash are mixed in the atomic proportions. The result is curious, and will be sufficiently understood by the following statement. If we mix together solutions containing

16 atoms chromate of potash ...... = 200
16 atoms sulphate of zinc .... = 164——364.

There will be deposited
6 atoms dichromate of zinc......... = 102
By carefully concentrating the mother liquor we
obtain 1st, 5 atoms bichromate of potash... = 95
2nd, 4 atoms potash-sulphate of zinc = 85
3rd, 6 atoms sulphate of potash...... = 66
4th, 1 atom bisulphate of potash..... = 16
Total.... 364

All these salts are conceived to be anhydrous, merely to simplify the numbers.

The practical chemist will easily understand how I proceeded. I evaporated the liquid as carefully as possible, picking out the crystals as they formed, and examining them. I easily recognized bichromate of potash, potash-sulphate of zinc, and sulphate of potash, by their forms. I did not perceive any crystals of bisulphate of potash, I merely inferred the formation of this salt from the impossibility of accounting in any other way for the formation of the other four salts from the atoms which I had mixed, unless the formation of that salt also be admitted. The reader will not suppose that I succeeded in separating the whole of these four salts from each other, I merely endeavoured to determine the nature of all the salts formed. The rest was the result of an obvious calculation.

From Mr. Stokes's experiments it would appear that similar salts are formed when sulphate of nickel or sulphate of copper is mixed with chromate of potash in the atomic proportions. This I have not hitherto had occasion to investigate. But it will probably turn out that my chromate of nickel and chromate of copper are in fact, dichromates. This I shall endeavour to ascertain as soon as I have leisure.

I am, &c.
Dec. 14, 1827. THOMAS THOMSON.

XIV. *On the Measurement by Trigonometry of the Heights of the principal Hills in the Vicinity of Dent, Hawes, and Sedbergh, in Yorkshire.* By JOHN NIXON, *Esq.*[*]

THREE years ago I commenced, and in the course of the present summer I succeeded in completing, the trigonometrical measurement of the heights of a group of hills of the *Penine* chain. Different observers have generally furnished from similar operations results so extremely discordant, that it becomes necessary, in order to inspire confidence, to give every needful particular respecting the bases, signals, instruments, and methods of calculation.

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[*] Communicated by the Author.

Bases.—From the third volume of the Trigonometrical Survey of England and Wales, are extracted the following distances between stations, &c. placed on hills forming part of the above group.

Ingleborough to the	Calf	77,615 feet.
Ditto	Shunnor Fell . .	82,397
Ditto	Pen-y-gent Rock	32,124
Ditto	Whernside . . .	22,435
Shunnor Fell to the	Calf	59,487

On the Calf were found the stakes which had supported the theodolite used in the Ordnance Survey. Shunnor Fell was not visited, but the signal now on its summit has most probably been erected over the proper station-mark. In the centre of the pile of stones yet remaining on the highest point of Whernside, there stood some years ago a signal-staff known to have been erected by Colonel Mudge's party. The rock on Pen-y-gent now cut up, and forming part of the boundary wall of Horton, was not situated exactly on the summit of the fell. The station on Ingleborough could not be satisfactorily identified, but the highest point of the mountain corresponds very nearly with it in its bearing and distance from the old hut, as given in the account of the survey. Assuming as a base for the calculations the distance of the *summit* of Ingleborough to that of Pen-y-gent, which we shall call 32,160 feet, the errors of such of the distances as are comparable with those of the Ordnance Survey will be;

Ingleborough to the	Calf	+32 feet.
Ditto	Shunnor Fell . .	−22
Ditto	Whernside . . .	+ 6
Shunnor Fell to the	Calf	+18

Signals.—Where stones were at hand, small towers five to eight feet high with bases six to nine feet in diameter were erected. When the summit of the hill was destitute of rocks, the (pyramidal) signals were formed of large sods. These signals were invariably placed on the loftiest point of the hill, determined in doubtful cases by resorting to a well-adjusted twelve-inch telescopic level.

Description of the Theodolite.—With the exception of a few observations made by a common four-inch theodolite, the horizontal angles were measured by a six-inch theodolite, constructed in the autumn of 1826, by Mr. P. Lealand, London.

The

The excellent twenty-inch telescope of this instrument is furnished with cylindrical rings, a delicate spirit-level, and very fine adjustable (steel?) cross-wires.—It is supported in the Ys of a cradle (common to theodolites of this description) having a horizontal axis four inches in length. A clamp and tangent screw, fitted to one of the supports of this axis, serve to give a slow vertical motion to the telescope*. The lower of the six-inch horizontal circles is divided (on silver) into thirds of a degree, capable of being read off to half-minutes by means of the two opposite verniers of the upper circle and a pair of double magnifiers. The latter are very conveniently fixed to a ring moveable about the rim of the under circle. The upper circle is also provided with a clamp and tangent screw for slow horizontal motion, and has a couple of spirit-levels placed at right-angles to each other, which with the four screws of the parallel plates enable the observer to render the plane of contact of the two circles parallel to the horizon. On the upper parallel plate is fixed a third clamp and tangent screw, intended, no doubt, to bring the cross-wires of the telescope, the circles being clamped together, with some particular degree coincident with the zero of one of the verniers, exactly on the observed object.

When the ground at the station proved impenetrable, heavy stones were piled against the feet of the tripod (set up with its legs at an angle of 45° to the horizon), in such a manner as to render it fixed and inflexible. The summit of the hill being swampy, the legs (set up more erect) were forced below the surface of the turf to some depth, and large stones were heaped on the ground immediately under the centre of the instrument†.

Either of the levels of the upper circle being brought exactly over any two of the screws of the parallel plates, the latter served to place its bubble at the mark. The circle being then moved half round, one half of the deviation of the bubble from its mark was corrected by the two screws, and the other by the adjusting nuts of the level. The level at right angles having undergone a similar verification and correction, the plane of contact of the two circles would consequently be rendered parallel to the horizon‡. The intersection of the cross-wires of the telescope (or line of collimation) was known to be parallel to the cylindrical rings encircling its tube, when the telescope could be made to revolve in its Ys without causing the point of intersection to deviate from the distant well-defined object which it had bisected. After this adjustment, the line of collimation, provided the artist had made the axis of the cradle parallel to the plane of contact of the two circles, and the Ys at right angles to that axis, would necessarily move in a vertical plane. As the remote signals could not be distinctly seen on interposing the horizontal wire of the telescope, the following method of observation with the vertical wire *only* was resorted to. The point of intersection of the wires being made to accurately bisect an object at a moderate distance, one wire was rendered perfectly vertical by turning the telescope to the right or left within its Ys, until either extremity of the wire, on elevating or depressing the telescope, would also pass through the middle of the object observed. Thus adjusted, it would evidently be superfluous to bisect *constantly* with any particular point of the vertical wire. To guard, however, against the possibility of the wire not being perfectly straight, a point, visually estimated to be equi-distant, from some particular particle of dust, &c. adhering to it, and the point of intersection by the other wire, was invariably brought on the signal.

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* The instrument has no vertical arch.

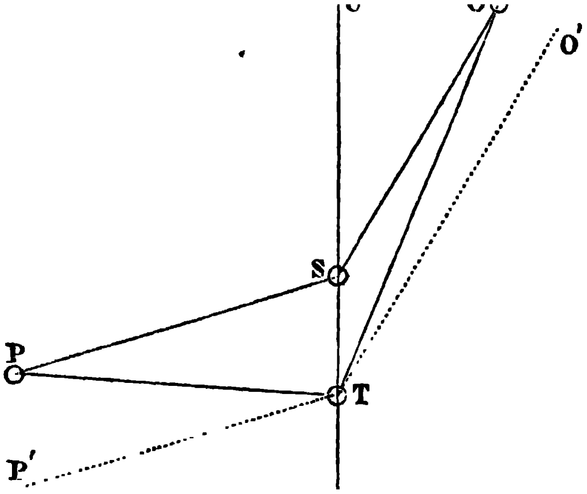
† Noughtberry Hill excepted, the ground at the theodolite-stations proved hard and firm.

‡ The correctness of this adjustment was confirmed when the upper circle could be moved quite round without displacing the bubble of the level of the telescope, previously brought to its mark.

The graduations being numbered from 0° to 360°, it follows, that the readings from the one vernier should differ exactly 180° from those given by the opposite one; or it will prove either that the vertical axis does not coincide with the centre of the divided circle, or that the divisions of the latter are incorrect. As both sources of error may exist, it will be proper to register for calculation the mean of the two readings. The order of numeration of the graduations being from left to right, the angle between any two of the observed signals is immediately found from a similar register, being equal, either to the difference of their respective readings, or to 360° minus that difference, according as it falls short of, or exceeds 180°.

The description of the instrument and its adjustments has been so ample, that the mode of observing with it will be too evident to require pointing out. It may, however, be needful to remark, that notwithstanding the length of the vertical axis, which is nearly four inches, it was found that a variation in the degree of force with which the two circles were clamped, caused a sensible deviation in the direction of the line of collimation; a source of error which it was endeavoured to obviate by always applying the *utmost* force to the clamp. Having completed single observations of the whole of the signals, it would, without doubt, have materially increased their accuracy to have repeated them (circumstances permitting) with the telescope reversed in position; but such was the inconvenient situation of the tangent screw of the upper circle, as to render the method impracticable*. To make a second set of observations with the telescope in its original position was the only alternative.

As *data* for the reduction of the observations to the centre of the base of the signal, the telescope was pointed so as to bisect the latter, and the reading of the verniers in addition to the distance of the centre of the circles to that of the signal registered in the usual manner. In order to comprehend the nature of the reduction, let S in the annexed figure represent the centre of the signal, T that of the divided circle, U the prolongation of the straight line passing through S and T, and O the object observed. Make TO′ parallel to SO; then, as the angle USO equals that of UTO′, the angle USO must exceed the observed angle UTO by the angle OTO′, equal to SOT. It is also clear that the observed angle, whether acute or obtuse, to the left or right of the line UST, will always be in defect. To calculate the angle SOT we have given the angle STO, the side ST, and (approximatively) that of SO. Then, as the graduations of the circle are numbered from left to right, the angle at T between U (or S) and an object O to the right, will be equal to the reading of O *minus* that of U; and we must add the angle of correction SOT to O, in order to make the registered readings coincide with those of another theodolite placed over S, the reading for U being alike by the two instruments. When the object (P) is to the left of the line UST, it will form with it at T an angle



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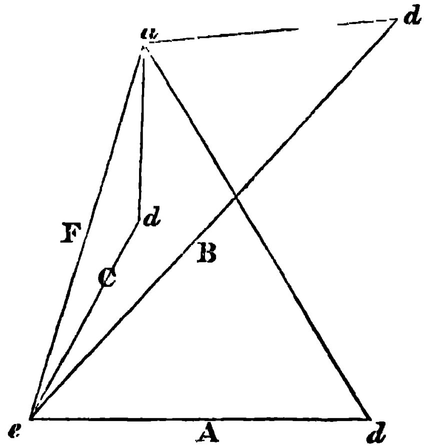
* I have since discovered that, by unfixing the clamp on the upper parallel plate, the circles might have been moved half-round in azimuth.

equal to U minus P, and the correction must be applied to P with the *negative* sign, in order to make USP greater than UTP by the angle of correction SPT. These corrections being applied to the readings for the different signals, we form a new register, whence we can immediately find by subtraction (as before) the angular distance of any two of the signals as observed from S in lieu of T.

On referring to the distances of some of the more remote signals from the theodolite-stations as measured from different bases, discrepancies, amounting in some instances to upwards of 100 feet, will be remarked. These differences were occasioned in a great measure by the signals having a diameter too inconsiderable for the distance, so that the vertical wire in lieu of bisecting, appeared to efface them*. The formula adopted for obtaining a proper mean of such of the different measurements of a distance as were remarkably discordant, requires to be explained.

In order to find the distance between the point *e* and the inaccessible object *a*, measure from *e* in different directions



any number of bases, as ABC, and observe at the two extremities of every base the inclination of that base to a line passing through the point of observation in the direction of the inaccessible object. With these *data* we shall be able to calculate from each base an independent measure of the required distance;—but supposing the observations to have been subject to a small uncertainty, limited, for instance, to one minute, required the probable error of each of the respective *calculated* distances?

By trigonometry, 180° *minus* the sum of the angles taken at the ends of any one of the bases will be equal to the subtense of that base at *a*. Then if we restrict ourselves in our calculations to the use of logarithms, treating them in the notation as natural numbers, F (or the distance required) will be equal to the sum of the base and sine of the angle opposite F, *minus* sine of the angle subtended at a by the base.—Here we may observe that the value of the angle at e affects the calculation merely as it serves to determine that of the unobserved angle a.

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* At a distance of 60,000 feet a tower 6 feet in diameter appears under an angle of only 21″.

Admitting that *one* only, as well as *both* of the angles $d\,e$, may have been inaccurately observed, the subjoined list will include every possible arrangement of the angular errors together with the consequent alteration of the correct value of the unobserved angle a:

I. Make the error of $d + 1'$, and $e + 1'$; whence $a = -2'$;
II. $+1$ 0 -1;
III. 0 $+1$ -1;
IV. $+1$ -1 0.

As the log. sines increase from 0° to 90°, and diminish from 90° to 180°, it will be necessary to divide our investigations into two classes.

Class I.—The angles at a and d being both acute. Call a' the difference of the log. sine of a and the log. sine of $a + 1'$ (or of $a - 1'$); and d' the corresponding difference of d and $d + 1'$ (or of d and $d - 1'$). Then as the correct length of F has been shown to be equal to $\mathrm{A} + \sin d - \sin a$, its value with an arrangement of the angular errors as given in No. I. will become

$= \mathrm{A} + \overline{\sin d + d'} - \overline{\sin a - 2a'}$; or simply

$= \mathrm{F} + d' + 2a'$; whence x or the lineal error required will be $= d' + 2a'$. For the whole class of the angular errors the values of x will be as follows:

I. $x \ldots = d' + 2a'$
II. ...... $d' + a'$
III. ...... a'
IV. ...... d'.

Hence the probable error of calculation will be equal to the mean of the above four quantities, or to $a' + \frac{3d'}{4}$.

Class II.—When a is obtuse. Selecting No. I. of the arrangement of the possible errors of observation, the calculated length of F becomes

$\mathrm{B} + \overline{\sin d + d'} - \overline{\sin a + 2a'}$; whence x will be equal to $d' \backsim 2a'$, and its value for the complete arrangement of errors,

I. .. $x = d' \backsim 2a'$
II. ...... $d' \backsim a'$
III. ...... a'
IV. ...... d':

Consequently when either a or d are obtuse, the mean error of calculation will be

$a' - \frac{d'}{4}$, when a' is greater than d';

$\frac{3d'-2a'}{4}$, when d' is greater than twice a';

$\frac{2a'+d'}{4}$, when d' is greater than a' but less than $2a'$.

x as thus expressed is the logarithm of the natural number by which we must multiply the correct distance, in order to obtain its value as affected by the mean of the possible errors of observation arranged conformable to our list. Consequently if we call 1 the correct distance, then will the error of measurement be equal to this natural number *minus* 1. Now if the distance as derived from one base A shall be liable to an error of 10 feet, and the same distance, as deduced from another base B, be uncertain to 20 feet, then must the claim to accuracy of A exceed that of B in the reciprocal ratio of 10 to 20 or as 10 to 5.

When we have given F′, F″, F‴ or the required distance as calculated from the same number of bases, together with the *reciprocals* of $x - 1$ corresponding to the respective bases, which call x', x'', x''', the correct value of F may be considered as equal to

$$\frac{\overline{F' \times x'} + \overline{F'' \times x''} + \overline{F''' \times x'''}}{x' + x'' + x'''}.$$

In practice it will be most convenient to take out the log. difference for 1′ of the sines of the angles d and e, noting them + or − according as the angle is acute or obtuse. A type of calculation is subjoined.

Base A; Noughtberry Hill to Ingleborough . . 44106 feet.
B Ditto Dod Fell 19347

A. Noughtberry 62° 19′ 8″
Ingleborough 55 26 53 log. diff. of 1′ = +0·0000870 = d',
Berkin (62 13 59) . . . . . . +0·0000665 = a'.

B. Noughtberry 145° 40′ 35″
Dod Fell 23 30 40 log. diff. of 1′ = +0·0002904 = d',
Berkin (10 48 45) . . . . . . +0·0006610 = a'.

Berkin to Noughtberry Hill by base A . . 41053 feet.
Ditto Ditto B . . 41142

Arithmetical mean . . . . . 41097·5

The value of x for the base A will be $a' + \frac{3d'}{4}$, $= 0{\cdot}0001317$, of which the natural number *minus* 1 is ·000303; and the reciprocal of the latter 330033.

The value of x for the base B will be $a' + \frac{3d'}{4}$, $=0{\cdot}0008788$, of which the natural number *minus* 1 is ·002026, and the reciprocal of the latter 49358.

The corrected distance will therefore be equal to

$$\frac{\overline{41053 \times 330033} + \overline{41142 \times 49358}}{330033 + 49358} = 41064{\cdot}5 \text{ feet.}$$

We might now proceed to investigate what proportion of the difference of 180°, and the sum of the three observed angles of a triangle should be assigned to each angle, were not the differences in the present survey, as appears from the following list, too trivial to require such nicety of correction.

No. 1. −0′ 40″
2. +0 19
3. −0 17
4. −0 35
No. 5. −2′ 35″
6. +1 18
7. −1 37
8. −0 12
No. 9. +1′ 54″
10. −0 6

Mean difference 57″ or 19″ per angle.

In the succeeding register of the observations, the second column contains the reading from the nearest vernier; the third column that from the opposite one, suppressing the degrees. The two next columns give the minutes and seconds of the corresponding readings for the second set of observations.

At Dod Fell.

					Reduction to Centre.
Lovely Seat	265° 5′ 30″	6′ 0″	5′ 0″	5′ 45″	−0′ 34″
Shunnor Fell	248 46 30	47 0	46 0	46 30	−0 28
Noughtberry Hill	182 42 0	42 0	42 0	41 30	−0 10
Berkin	159 11 15	11 0	11 0	10 30	+0 6
Bow Fell	191 1 30	1 30	1 0	1 0	−0 9
Pen-y-gent	66 41 0	40 30	41 0	40 30	+0 32
Whernside	138 51 30	51 0	51 0	50 45	+0 20
Colm	151 36 30	36 0	36 30	36 0	+0 10
Blea Moor	139 58 0	57 15	58 15	57 15	+0 30
Ingleborough	110 40 45	40 0	41 0	40 30	+0 24
Wildboar Fell	215 20 0	20 30	20 0	20 30	−0 15
Cam Fell	116 37 30	36 30	....	....	+1 42′
Cotter Fell*	232 20 0	20 0	....	....	−0 19
The Signal	354 0 0	6 feet distant.			

At Whernside.

Colm	241° 15′ 30″	15′ 30″	....	....	0′ 0″
The Calf	277 42 30	42 30	....	....	+0 10

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* There was no signal on this hill.

					Reduction to Centre.
Bow Fell	303° 26′ 30″	26′ 30″	......	......	+0′ 25″
Shunnor Fell	336 56 15	55 30	......	......	+0 14
Noughtberry	343 17 30	17 0	......	......	+0 36
Lovely Seat	348 2 30	2 0	......	......	+0 14
Whaw Fell	358 40 15	40 0	......	......	+0 41
Dod Fell	15 39 15	38 0	......	......	+0 19
Wildboar Fell	308 30 30	31 0	......	......	+0 15
Pen-y-gent	70 57 30	56 30	......	......	−0 4
Ingleborough	119 53 0	52 30	......	......	−0 36
Graygreth	189 56 0	55 30	......	......	−0 41
The Signal	241 0 0	4·5 feet distant.			

At Pen-y-gent.

Whernside	85° 49′ 30″	49′ 0″	51′ 30″*	50′ 30″	0′ 0″
Ingleborough	54 5 30	5 0	6 30	6 30	+0 14
Colm	83 22 0	21 30	23 30	22 30	+0 1
Dod Fell	138 21 30	21 30	23 0	22 30	−0 18
Bow Fell	108 53 30	53 0	55 0	54 0	−0 5
Noughtberry	117 9 0	8 30	10 30	10 0	−0 9
Wildboar Fell	119 28 0	27 0	29 0	29 0	−0 5
Blea Moor	101 38 0	37 30	39 0	37 30	−0 6
Shunnor Fell	139 28 30	28 30	29 30	27 30	−0 9
Lovely Seat	147 38 0	37 30	38 30	38 30	−0 10
The Signal	265 49 0	4 feet distant.			

At Ingleborough Hill.

Berkin	3° 52′ 0″	52′ 0″	51′ 30″	51′ 30″	+0′ 31″
Colm	14 9 30	10 0	9 30	9 30	+0 47
The Calf	20 27 0	27 0	.....	.....	+0 12
Whernside	36 23 0	22 30	23 0	22 30	+0 21
Pen-y-gent	135 44 0	44 0	44 0	44 30	−0 57½
Dod Fell	83 59 30	60 0	58 30	59 0	−0 23
Noughtberry	59 19 0	19 30	19 0	19 30	−0 6
Blea Moor	60 21 30	21 30	22 0	21 0	−0 11
Graygreth	350 32 30	32 30	......	......	+1 9
Bow Fell	38 30 30	30 30	......	......	+0 7
Lovely Seat	72 36 30	36 30	......	......	−0 9
Shunnor Fell	63 59 30	60 0	(Fell misty)		−0 5
The Signal	230 30 0	9 feet distant.			

At Noughtberry† Hill.

Dod Fell	224° 17′ 0″	16′ 30″	......	......	+0′ 29″
Pen-y-gent	267 5 0	5 30	......	......	+0 24
Ingleborough	307 38 30	38 0	......	......	+0 26

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* On the point of making the second set of observations one of the feet of the tripod was disturbed.

† *Rubus Chamæmorus.*

					Reduction to Centre.
Blea Moor	305°47′ 30″	47′ 0″	....	....	+1′ 14″
Whernside	328 5 0	5 15	....	....	+0 38
Colm	354 36 30	36 0	....	....	+0 15
Berkin	9 58 0	57 30	....	....	+0 4
The Calf	56 12 15	11 45	....	....	−0 15
Bow Fell	60 7 30	7 0	....	....	−0 42
Wildboar Fell	92 14 30	13 30	....	....	−0 30
Cotter Fell	112 29 0	28 30	....	....	−0 27
Shunnor Fell	137 37 30	36 30	....	....	−0 28
Lovely Seat	155 53 0	53 0	....	....	−0 21
Whaw Fell	282 38 0	37 30	....	....	+2 43
The Signal	197 30 0	6 feet distant.			

The angles at the following stations were measured by a four-inch theodolite (much out of repair) reading off to one minute. The graduations were numbered precisely as those of the six-inch theodolite.

At Whaw Fell.

Dod Fell	291° 30′ 0″
Noughtberry	192 42 0
Blea Moor	54 32 0
Whernside	73 33 0
Cam Fell	328 40 0
Bow Fell	160 58 0
Ryssell	135 30 0
Snays Fell	291 58 0
Bear's Head	274 20 0
Ten End	265 14 0

At Blea Moor.

Dod Fell	26° 6′ 0″
Cam Fell	42 28 0
Snays Fell	14 28 0
Whaw Fell	349 0 0

At Colm.

Whernside	235° 52′ 0″
Ingleborough	272 17 0
Graygreth	313 19 0

At Ryssell.

Blea Moor	305° 56′ 0″
Bow Fell	200 24 0
Whaw Fell	285 17 0

At Bear's Head.

Dod Fell	167° 49′ 30″
Noughtberry	205 33 30
Whaw Fell	189 56 30

At Ten End.

Bear's Head	267° 16′ 0″
Cam Fell	22 30 0
Blea Moor	47 58 0
Whaw Fell	57 41 0
Noughtberry	80 8 0

Calculation of the Distances.

		Feet.
Ingleborough to Pen-y-gent		32160

Ingleborough	99° 20′ 18″	22441
Pen-y-gent	31 44 16	42096
Whernside	48 55 26	

		Feet.
Ingleborough	76° 23′ 55″	44098
Pen-y-gent	63 3 9	48082
Noughtberry	40 32 56	
Ingleborough	22 56 55	44111
Whernside	136 35 9	25026
Noughtberry	20 27 56	

Pen-y-gent

		Feet.
Pen-y-gent	31° 19′ 46″	48086
Whernside	87 39 35	25023
Noughtberry	61 0 39	

Ingleborough	51 44 24	46068
Pen-y-gent	84 15 49	36354
Dod	43 59 47	

Ingleborough	47 33 58	46068
Whernside	104 13 24	35095
Dod	28 10 38	

Whernside	55 18 4	35096
Pen-y-gent	52 31 45	36356
Dod	72 10 11	

Mean Distances.

Ingleborough to Noughtberry...44105
Ingleborough to Dod Fell 46068
Pen-y-gent to Noughtberry......48084
Pen-y-gent to Dod Fell 36355
Whernside to Noughtberry......25024
Whernside to Dod Fell 35095

Pen-y-gent	21° 12′ 2″	48091
Dod	115 59 59	19350
Noughtberry	42 47 59	

Ingleborough	24 39 5	44110
Dod	72 0 6	19345
Noughtberry	83 20 49	

Whernside	32 21 7	25031
Dod	43 50 20	19339
Noughtberry	103 48 33	

Mean distance of Dod to Noughtberry...... 19345

Ingleborough	38 51 57	77647
Noughtberry	108 33 4	51393
Calf	32 34 59	

Whernside	65 35 11	56420
Noughtberry	88 6 0	51404
Calf	26 18 49	

Mean distance of the Calf to Noughtberry 51398
The Calf to Pen-y-gent 95894

Ingleborough	121° 32′ 46″	32250
Pen-y-gent	29 16 18	56210
Colm	29 10 56	

Ingleborough	22 12 42	32283
Whernside	121 23 21	14296
Colm	36 23 57	

		Feet.
Ingleborough	69° 48′ 28″	32267
Dod	40 55 27	46230
Colm	69 16 5	

Ingleborough	45 8 45	32260
Noughtberry	46 57 49	31288
Colm	87 53 26	

Pen-y-gent	54 59 25	56230
Dod	84 55 8	46237
Colm	40 5 27	

Pen-y-gent	33 46 57	56222
Noughtberry	87 30 51	31291
Colm	58 42 12	

Whernside	102 2 21	14284
Noughtberry	26 30 45	31294
Colm	51 26 54	

Whernside	134 23 26	14276
Dod	12 45 1	46221
Colm	32 51 33	

Dod	31 5 17	46268
Noughtberry	130 19 16	31335
Colm	18 35 27	

Mean Distances.

Ingleborough to Colm 32265
Pen-y-gent 56221
Whernside 14285
Noughtberry 31302
Dod 46239

Ingleborough	45° 49′ 27″	23444
Whernside	70 2 55	17888
Graygreth	64 7 38	

Ingleborough	23 36 45	23439
Colm	41 2 0	14301
Graygreth	115 21 15	

Whernside	51 20 26	17890
Colm	77 27 0	14311
Graygreth	51 12 34	

Mean Distances.

Ingleborough to Graygreth......23442
Whernside 17889

Ingleborough	55° 26′ 53″	44141
Noughtberry	62 19 8	41053
Berkin	62 13 59	

Ingleborough	80 6 36	44157
Dod	48 30 4	58080
Berkin	51 23 20	

Dod

		Feet.
Dod	23°30′40″	58182
Noughtberry	145 40 35	41142
Berkin	10 48 45	

Corrected mean Distances.

Ingleborough to Berkin	44149
Dod	58092
Noughtberry	41065

Ingleborough	97°12′33″	55981
Pen-y-gent	54 47 42	67971
Bow Fell	27 59 45	
Ingleborough	20 48 32	55981
Noughtberry	112 27 52	21520
Bow Fell	46 43 36	
Ingleborough	45 28 15	55998
Dod	80 20 8	40495
Bow Fell	54 11 37	
Pen-y-gent	23 3 40	67951
Whernside	127 30 1	33550
Bow Fell	29 26 19	
Pen-y-gent	29 28 1	67993
Dod	124 19 49	40503
Bow Fell	26 12 10	
Whernside	39 50 56	33580
Noughtberry	92 0 48	21530
Bow Fell	48 8 16	
Whernside	72 12 1	33576
Dod	52 9 42	40480
Bow Fell	55 38 17	

Mean Distances.

Ingleborough to Bow Fell	55987
Pen-y-gent	67972
Whernside	33568
Noughtberry	21525
Dod Fell	40493

Pen-y-gent	33°38′ 2″	87582
Whernside	122 25 56	57475
Wildboar Fell	23 56 2	
Pen-y-gent	18 53 39	87672
Dod	148 38 43	54561
Wildboar Fell	12 27 38	
Whernside	67 7 56	57516
Dod	76 28 40	54506
Wildboar Fell	36 23 24	
Whernside	34 46 51	57566
Noughtberry	124 7 45	39670
Wildboar Fell	21 5 24	

		Feet.
Dod	32°38′18″	54436
Noughtberry	132 3 44	39545
Wildboar Fell	15 17 58	

Corrected mean Distances.

Whernside to Wildboar Fell	57522
Dod	54494
Pen-y-gent	87613
Noughtberry	39616

Ingleborough	71°43′30″	82363
Pen-y-gent	85 22 15	78465
Shunnor Fell	22 54 15	
Ingleborough	27 36 34	82326
Whernside	142 56 2	63300
Shunnor	9 27 24	
Ingleborough	19 59 12	82407
Dod	138 5 4	42163
Shunnor	21 55 44	
Pen-y-gent	53 38 13	78480
Whernside	94 0 49	63352
Shunnor	32 20 58	
Pen-y-gent	22 19 0	78541
Noughtberry	129 29 7	38643
Shunnor	28 11 53	
Whernside	38 42 49	63377
Dod	109 54 38	42158
Shunnor	31 22 33	
Noughtberry	86 40 32	38619
Dod	66 4 20	42180
Shunnor	27 15 8	

Corrected mean Distances.

Ingleborough to Shunnor	82375
Pen-y-gent	78500
Whernside	63359
Noughtberry	38628
Dod	42168

Ingleborough	63° 6′49″	80969
Pen-y-gent	93 31 51	72354
Lovely Seat	23 21 20	
Ingleborough	13 17 12	80899
Noughtberry	151 46 2	39303
Lovely Seat	14 56 46	
Ingleborough	11 22 31	81008
Dod Fell	154 24 2	36978
Lovely Seat	14 13 27	

Pen-y-gent

		Feet.
Pen-y-gent	61° 47′ 49″	72300
Whernside	82 54 27	64213
Lovely Seat	35 17 44	

Pen-y-gent	30 28 36	72316
Noughtberry	111 13 0	39345
Lovely Seat	38 18 24	

Whernside	27 36 27	64188
Dod Fell	126 13 6	36870
Lovely Seat	26 10 27	

Dod Fell	82 23 18	36873
Noughtberry	68 24 35	39305
Lovely Seat	29 12 7	

Corrected mean Distances.

Ingleborough to Lovely Seat	80959
Whernside	64200
Pen-y-gent	72321
Noughtberry	39320
Dod	36887

Noughtberry	111° 48′ 56″	46333
Dod	49 38 0	56456
Cotter Fell	18 33 4	

Pen-y-gent to Cotter Fell 92115

Ingleborough	75° 21′ 51″	28252
Pen-y-gent	47 31 48	37058
Blea Moor	57 6 21	

Ingleborough	23 37 33	28250
Dod	29 17 13	23144
Blea Moor	127 5 14	

Pen-y-gent	15 31 27	37063
Noughtberry	38 42 50	15860
Blea Moor	125 45 43	

Pen-y-gent	36 43 55	37056
Dod	73 16 54	23140
Blea Moor	69 59 11	

Dod	42 43 31	23149
Noughtberry	81 31 15	15880
Blea Moor	55 45 14	

Mean Distances.

Ingleborough to Blea Moor	28251
Pen-y-gent	37059
Dod	23144
Noughtberry	15870

Whernside	15° 22′ 57″	20418
Noughtberry	45 25 17	7604
Whaw Fell	119 11 46	

Noughtberry	58 23 14	7593
Dod	22 49 16	16671
Whaw Fell	98 47 30	

		Feet.
Whernside	16° 58′ 8″	20434
Dod	20 58 52	16655
Whaw Fell	142 3 0	

Mean Distances.

Noughtberry to Whaw Fell	7598
Whernside	20426
Dod	16668
Shunnor Fell	45068
Lovely Seat	44290
Bow Fell	27603
Ingleborough	37356
Blea Moor	9371
Berkin	41407
Colm	29836
Pen-y-gent	40818

Blea Moor	16° 22′ 0″	14348
Dod	23 19 29	10211
Cam Fell	140 18 31	

Blea Moor	53 28 0	14343
Whaw Fell	85 52 0	11555
Cam Fell	40 40 0	

Whaw Fell	122 36 0	7618
Blea Moor	25 28 0	14926
Snays Fell	31 56 0	

Whaw Fell	72 33 0	19821
Noughtberry	85 0 0	18981
Ten End	22 27 0	

Whaw Fell	63 24 0	19829
Cam Fell	81 25 0	17931
Ten End	35 11 0	

Whaw Fell	81 39 0	27998
Noughtberry	82 44 0	27925
Bear's Head	15 37 0	

Whaw Fell	17 8 0	28011
Dod Fell	140 45 0	13042
Bear's Head	22 7 0	

Mean Distances.

Ten End to Whaw Fell	19825
Bear's Head	28004

Whaw Fell	80° 58′ 0″	26028
Blea Moor	78 23 0	26243
Ryssell	20 39 0	

Whaw Fell	25 28 0	25984
Bow Fell	69 39 0	11916
Ryssell	84 53 0	

Mean distance of Whaw Fell to Ryssell......... } 26006

[To be continued.]

XV. *On*

XV. *On* Mr. Herapath's *Method for the Integration of Linear Equations with constant Coefficients.*

THE integration of linear equations with constant coefficients engaged the attention of analysts very early in the progress of the Integral Calculus. The subject has been treated of so often and so ably, that we can hardly expect any addition to our knowledge of it either new in principle, or very interesting in the details. It would not therefore have been necessary to notice particularly the communications of Mr. Herapath published in the last two Numbers of this Journal, if he had not fallen into an inadvertence respecting Lagrange's investigation, of which it seems proper to advise the public.

In pp. 22, 23, of the last Number of this Journal, Mr. Herapath applies Lagrange's process to the equation,

$$\frac{d^3y}{dx^3} + A\frac{d^2y}{dx^2} + B\frac{dy}{dx} + Cy = X.$$

The calculation is given at length in Lacroix's Treatise on the Integral Calculus, 2nd edit. vol. ii. pp. 326, 327. If r, r_1, r_2 stand for the roots of the equation

$$r^3 + Ar^2 + Br + C = 0,$$

the complete integral is as follows, viz.

$$y = \frac{e^{rx}\left(c + \int Xe^{-rx}\,dx\right)}{r - r_1\,.\,r - r_2}$$

$$+ \frac{e^{r_1x}\left(c_1 + \int Xe^{-r_1x}\,dx\right)}{r_1 - r\,.\,r_1 - r_2}$$

$$+ \frac{e^{r_2x}\left(c_2 + \int Xe^{-r_2x}\,dx\right)}{r_2 - r\,.\,r_2 - r_1}.$$

Of these three parts Mr. Herapath has calculated only the first, which he says must separately satisfy the proposed equation. Now in this assertion, of which no proof is given, lies his mistake. If every part had contained only one root, it might have been rightly inferred that it must separately satisfy the differential equation; but, as the case stands, there is no ground for the assertion. If Mr. Herapath will substitute the two remaining parts of the integral in the differential equation, in like manner as he has substituted the first part, he will find, on collecting the three results, that there is no failure in Lagrange's method.

α β.

XVI. *On the Error imputed by* Mr. Herapath *to* Lagrange *in his Method of integrating Linear Differential Equations.* *By* A CORRESPONDENT.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

YOUR Correspondent Mr. Herapath has endeavoured to prove that Lagrange has fallen into an error in his integration of linear differential equations, and expresses his surprise that mathematicians have for upwards of half a century suffered this to pass unobserved. Skilful mathematicians will not take Mr. Herapath's word for his bold assertion without examination, and will then easily detect the fallacy of his reasoning: but as your readers are not all of them mathematicians of competent knowledge, it will perhaps not be useless to point out Mr. Herapath's error.

Mr. H. has deduced the following equation (page 24.)

$$(r^2 + Ar + B)\,aX + (r + A)\,a\,dX + a\,d^2X = X$$

a being $\dfrac{1}{r^2 - r\,r_2 - r\,r_1 + r_1 r_2} = \dfrac{1}{(r - r_1)(r - r_2)}$

This equation is quite incomplete. Mr. H. having employed that part of y which depends on the root r, only; all the terms arising from those parts of y which depend on r_1 and r_2 are left out. A and B involve the three roots equally, and their values may be substituted in the above terms on the left-hand side of the equation as Mr. H. does, and we obtain for them the following

$$r_1 r_2 aX - (r_1 + r_2)\,a\,dX + a\,d^2X$$

Call a', a'' the values corresponding to a for the two other roots r_1, r_2, so that $a' = \dfrac{1}{(r_1 - r)(r_1 - r_2)}$ and $a'' = \dfrac{1}{(r_2 - r)(r_2 - r_1)}$.

The terms which are, consequently, to be added to make Mr. H.'s equation complete will be

$$r\,r_2 a' X - (r + r_2)\,a'\,dX + a'\,d^2X$$

$$r\,r_1 a'' X - (r + r_1)\,a''\,dX + a''\,d^2X,$$ and the aggregate of all terms will give this equation:

$$(r_1 r_2 a + r\,r_2 a' + r\,r_1 a'')X - [(r_1 + r_2)a + (r + r_2)a' + (r + r_1)a'']dX + (a + a' + a'')\,d^2X = X.$$

It will now be easily seen that $a + a' + a'' = 0$,

$(r_1 + r_2)\,a + (r + r_2)\,a' + (r + r_1)\,a'' = 0$ and

$r_1 r_2 a + r\,r_2 a' + r\,r_1 a'' = 1$ and that, consequently, the equation becomes $X = X$, as it ought to be. This is the same proof of the correctness of Lagrange's solution that Mr. Herapath employs to establish the truth of his own.

I am, Gentlemen,
Your obedient servant,
4, Pancras-Lane. F. R. S.

XVII. *On the ultimate Composition of simple Alimentary Substances; with some preliminary Remarks on the Analysis of organized Bodies in general. By* William Prout, *M.D. F.R.S.*

[Concluded from page 40.]

Of the Saccharine principle.

IN the following observations, the word *Sugar*, is used in its ordinary acceptation; but the extended sense in which the term *saccharine principle* is employed, requires a few remarks.

Messrs. Gay Lussac and Thenard were induced to conclude, from their experiments on organized products, that when the hydrogen and oxygen of a substance exist in it in the proportions in which they form water, the substance is neither acid nor alkaline, as in sugar, starch, gum, &c.; that when the oxygen exceeds this proportion, the substance possesses acid properties; that when it is less, an oily or resinous character*. These conclusions are true to a certain extent, but by no means universally so, as will be shown hereafter. I shall however adopt this general distribution of organized substances so far, as to confine my attention at present to those substances in which the first peculiarity above mentioned exists; and as sugar, on account of its crystalline form, appears to constitute the most perfect and definite of these substances, I have thought it best entitled to give a name to the whole class, or family, and hence have included, under the term *saccharine principle*, all those substances, whatever their sensible properties may be, into the composition of which the hydrogen and oxygen enter in the proportions in which they form water. Now it will be found, that the substances thus constituted may generally be employed as aliments; and as they are chiefly derived from the vegetable kingdom, they may be considered as representing vegetable aliments, properly so called; hence, *saccharine principle* and *vegetable aliment* may be regarded as synonymous terms, and they will be so employed throughout the present inquiry.

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* *Récherches Physico-chimiques*, ii. 321.

As a subject of general interest to chemists, as well as of considerable importance in the present inquiry, I shall also attempt to investigate the composition of a few of the compounds of the saccharine principle with oxygen, or what are usually denominated the *vegetable acids.*

Of Sugars.

Many analyses of sugar have been published by different chemists, no two of which agree with each other. These discrepances have doubtless arisen from various causes, though one cause has probably been some real or accidental difference in the composition of the sugars employed*. How many distinct varieties of sugar exist I do not pretend to know, but there are at least two, (independently of the sugar of milk, manna, &c. which belong to another series,) and probably there are several others; and it is to the mixture or combination of these in different proportions, and the frequent presence of foreign bodies, that a good deal of the confusion respecting the composition of sugar has undoubtedly risen.

Cane Sugar.—The strongest and most perfect sugar that I am acquainted with, is sugar candy carefully prepared from cane sugar. This, purified by repeated crystallizations from water and alcohol, and deprived of the little hygrometric moisture

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* Some years ago I published an analysis of sugar, in which the proportions of carbon to water were stated to be to one another as 40 : 60. I was not aware at that time of the differences existing among sugars, and the results given were founded on the analysis of a specimen of remarkably fine looking sugar candy, a quantity of which I had purchased and kept by me for several years for the purposes of experiment[a]. At length my stock became exhausted, and I was surprised to find on analysing other specimens, that they in general contained upwards of one per cent. more of carbon than what I had before examined. This induced me to recur to the notes of my former experiments, but I could detect no material error in them; and though I readily admit that the apparatus I then employed was much less susceptible of accuracy than what I now use, I cannot help thinking that the candy itself was partly in fault, and that it was prepared from an imperfect sugar, probably from the East Indies.

There was also another circumstance which contributed to mislead me, not only in this but in all my other results, viz. an inaccuracy in the weight usually assigned to atmospheric air, at least as regarded *my weights.* I have long suspected the perfect accuracy of this datum as settled fifty years ago by Sir G. Shuckburgh, and have been accustomed for some time past to make an allowance for it; but I was not aware till recently of its exact amount, when I was induced to undertake a series of experiments on the subject, which I hope shortly to lay before the public.

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[a] See Annals of Philosophy, iv. 424 (N.S.) I do not distinctly remember whether, at the time this paper was published, some of the original sugar candy existed or not, but I had then only made one or two experiments on the sugar of commerce.

moisture that usually adheres to it by exposure for some time to a temperature of 212°, was found to be composed of

Carbon	42·85
Water........	57·15

Now, all the finest and purest specimens of loaf sugar of commerce that I have yet examined, give, when similarly treated, precisely the same results. They may therefore be considered as identical in their composition with sugar candy*. Cane sugar appears to undergo no change whatever at the temperature of boiling water; but at about 300° it begins to melt, and assume the form of a dark-brown liquid. In one experiment, after exposure to this temperature for seven hours, it lost ·6 per cent. only of its weight, but its properties seem to have been permanently injured. Berzelius however has shown that, on being combined with lead, sugar parts with about 5·3 per cent. of water without undergoing decomposition; for he has likewise shown that it may be obtained again from the lead in its original state. This saccharate of lead I have several times formed, and once by accident I obtained it in the state of beautiful crystals.

Sugar of Honey.—The *lowest*† well-defined sugar that I have yet examined, was first obtained from Narbonne honey, by means of a process formerly pointed out by me for obtaining diabetic sugar in a state of purity‡. This, deprived of its hygrometric moisture by being placed under a receiver with sulphuric acid for several days, was found to consist of

Carbon.......	36·36
Water........	63·63

This sugar in the ordinary state of the atmosphere usually contains more water than indicated by this analysis; that is to say, generally about 64·7 per cent. On the other hand, on exposure to a temperature considerably below that of boiling water, it rapidly loses about 3 per cent. of water, and begins to assume the fluid form: kept at the temperature of boiling water for 30 hours, it lost in one experiment upwards of 10 per cent. of its original weight, became of a deep brown colour, and seemed to be partially decomposed *.

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* Dr. Ure states that he has found sugar to contain upwards of 43 per cent. of carbon; but no such specimen has occurred to me, though I by no means deny its existence. Indeed I have hitherto met with no sugar as it occurs in commerce, yielding more than 42·5 per cent. of carbon, and frequently it contains considerably less than this.

† In commerce, these imperfect sugars are denominated *weak* or *low* sugars, which last epithet is here employed in this sense.

‡ Med. Chirurg. Trans. viii. 537. I have little doubt that honey contains a still lower sugar, and which is incapable in this country (at least during a great part of the year), of assuming the solid form. This is probably the liquid sugar of Proust.

Sugar prepared from *starch* evidently belongs to this variety, as is sufficiently indicated both by its sensible properties and composition. The same is true in general of *diabetic sugar*, and probably also of the *sugar of grapes, figs, &c.* When pure, all the varieties of this sugar are beautifully white, crystallize in spherules, and are permanent under the ordinary circumstances of the atmosphere.

Between these two extremes, sugars occur of almost every grade, as the following table will show.

	Carbon.	Water.
Pure sugar candy	42·85	57·15
Impure† sugar candy . . .	41·5 to 42·5	58·5 to 57·5
East India sugar candy (*v*)	41·9	58·1
English refined sugar . . .	41·5 to 42·5	58·5 to 57·5
East India refined sugar (*v*)	42·2	57·8
Maple sugar (*v*)	42·1	57·9
Beet-root sugar (*v*)	42·1	57·9
East India moist sugar (*v*)	40·88	59·12
Sugar of diabetic urine . .	36 to 40?	64 to 60?
Sugar of Narbonne honey	36·36	63·63
Sugar from starch	36·2	63·8

On some of these it may be necessary to make a few remarks. The *sugar candies* of the shops frequently contain minute quantities of foreign fixed bodies, such as lime, &c., as well as others of a destructible character. Both the specimens of *India sugar candy* I examined were obviously impure to the eye, being of a brown colour and deliquescent; they contained, among other things, traces of potash. The *East India refined sugar* was perfectly white, but rather soft and friable, and it did not possess the fine and brilliant grain of the best refined sugars of commerce. For a specimen of the *maple* sugar I was indebted to Mr. Faraday; this, when I received it, was very impure and deliquescent, but by treating it by the process above alluded to, a portion was separated that differed but little in its appearance from cane sugar. The *beet-root sugar* was made and refined in France; it was perfectly white, but rather soft and fine in the grain. The *East India moist sugar* was of a very low kind, and known in commerce by the name of *Burdwan sugar;* it was deprived of its hygrometric moisture before analysis by exposure to sulphuric acid under a receiver. The *diabetic sugar* was prepared as above; the results given were obtained many years ago, and I have had no opportunity of repeating the analysis with the present apparatus; I believe however that diabetic sugars in general belong to the honey variety. The *sugar of starch* was prepared by myself in the usual manner.

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* I observed that after this sugar had been cautiously melted it might be preserved in the state of a transparent fluid, if placed in a perfectly dry atmosphere, as under the receiver of an air-pump with sulphuric acid; but that in a few hours after exposure to the air, it began to grow opaque and assume the crystalline form, by attracting moisture. Is not this precisely analogous to the deterioration which is known to take place in the sugars of commerce? See Mr. Daniell on this subject in the Royal Institution Journal, vol. xxxii. Dr. Ure supposes that this deterioration depends on the absorption of oxygen; but I have hitherto met with no sugar containing an excess of oxygen.

† In these results *fixed* bodies only have been allowed for, and those marked (*v*), as occurring in commerce, are probably subject to slight variations in their composition.

Of Amylaceous Principles.

Before we proceed to consider the analysis of amylaceous bodies, a few remarks on the nature of these and similar substances may not be deemed improper. It has been known from the very infancy of chemistry, that all organized bodies, besides the elements of which they are essentially composed, contain minute quantities of different foreign bodies, such as the earthy and alkaline salts, iron, &c. These have been usually considered as mere mechanical mixtures accidentally present; but I can by no means subscribe to this opinion. Indeed, much attention to this subject for many years past has satisfied me that they perform the most important functions: in short, that organization cannot take place without them. This point will be more fully investigated hereafter: at present it is sufficient merely to observe, that many of those remarkable changes which crystallized bodies undergo on becoming organized, are more apparent than real; that is to say, their chemical composition frequently remains essentially the same; and the only points of difference that can be traced, is the presence of a little more or less of water, or the intimate mixture of a minute portion of some foreign fixed body. There is no term at present employed which expresses this condition of bodies, and hence, to avoid circumlocution, I have provisionally adopted the term *merorganized**, (μέρος *pars* vel *partim*,) meaning to imply by it that bodies on passing into this state become partly, or to a certain extent, organized. Thus starch I consider as *merorganized* sugar, the two substances having, as we shall see presently, the same essential composition, but the starch differing from the sugar by containing minute portions of other matters, which, we may presume, prevent its constituent particles from arranging themselves in the crystalline form, and thus cause it to assume totally different sensible properties*.

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* I am indebted to my friend Mr. Lunn for this term.

Wheat Starch.—The most perfect form of the amylaceous principle is undoubtedly that derived from wheat. This has been analysed by different chemists with very different results. MM. Gay Lussac and Thenard state that they found it to contain as much as 43·55 per cent. of carbon; while Dr. Ure informs us that he only found 38·55 per cent. The following observations will sufficiently explain these differences.

A very fine specimen of wheat starch, which had been prepared expressly at my desire without the addition of the colouring matter commonly added to the starch of commerce, and which had been kept in a dry situation for many months, was found, in the ordinary columnar form in which it usually occurs, (abstracting foreign matters,) to consist of

Carbon......	37·5
Water	62·5

One hundred parts of the same specimen reduced to a state of fine powder, and subjected to a temperature between 200° and 212°, for the space of twenty hours†, lost, in a mean of two experiments, 12·5 parts, and on being analysed in this state gave

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* When this subject first occupied my attention many years ago, I was at a loss to form any notion of the *modus operandi* of these minute admixtures of foreign bodies, except the mechanical one mentioned in the text, viz. that they operated by being interposed, as it were, among the essential elements of bodies, and thus by weakening or modifying their natural affinities. But the admirable paper, published by Mr. Herschel, in the Philosophical Transactions for 1824, " On certain motions produced in fluid conductors when transmitting the electric current," appeared to throw an entire new light on the subject. The facts brought forward in this paper are of the most important kind, and seem to me to be evidently connected with a principle of a more general character, which when completely developed, will lead to the most unexpected results. " That such minute proportions of extraneous matter," says Mr. H. " should be found capable of communicating sensible mechanical motions and properties of a definite character to the body they are mixed with, is perhaps one of the most extraordinary facts that has yet appeared in chemistry. When we see energies so intense exerted by the ordinary forms of matter, we may reasonably ask what evidence we have for the imponderability of any of the powerful agents to which so large a part of the activity of material bodies seem to belong?"

Any substance may be supposed capable of performing the part of a mer-organizing body; but, in a certain point of view, *water* appears to constitute the *first* and *chief*, at least in organized substances.

† I have reason to believe from other experiments that six or eight hours, or even less, of steady exposure to the boiling temperature, will sometimes reduce both starch and arrow root, and even gum, to this state of desiccation.

Carbon	42·8
Water.......	57·2

which very nearly coincides with what by calculation it ought to have given, on the supposition that the loss of weight was owing to the escape of water, a circumstance indeed of which there could have been little doubt. Starch however in this state still retains water, a portion of which may be separated by subjecting it to higher temperatures. Thus, after having been exposed as above for twenty-four hours to the temperature of 212°, on being further submitted to a temperature between 300° and 350° for six hours longer, it lost 2·3 per cent. more, and analysed in this state gave very nearly

Carbon	44
Water.......	56

It had now acquired a slight yellow colour, and seemed to have suffered some change in its properties; hence, this is probably nearly the utmost quantity of water that starch is capable of parting with, short of decomposition.

Arrow root.—This is another variety of the amylaceous principle, of which, like sugar, there seems to be a great variety. The specimen on which the following experiments were made was remarkably fine, and free from adventitious matters. It had been kept in the same drawer with the starch before mentioned, and under precisely similar circumstances of the atmosphere was found to consist of (abstracting foreign matters)

Carbon......	36·4
Water	63·6.

One hundred parts, in the above state, exposed for twenty hours to a temperature between 200° and 212°, lost fifteen parts. Hence its composition, when thus dried, was very nearly the same as that of wheat starch similarly exsiccated; or it consisted of

Carbon	42·8
Water	57·2.

On being subjected to the full temperature of 212° for six hours longer it lost 3·2 per cent. more, and was then reduced to a state similar to that of starch dried between 300° and 350°, or it consisted very nearly of

Carbon......	44·4
Water.......	55·6

When subjected to the temperature of 300° and 350° for six hours longer, it lost 1·38 per cent. more of its weight, but became of a deeper yellow colour than starch similarly exposed, and consequently showed greater marks of decomposition. Hence, this form of the amylaceous principle, like the sugar of honey before mentioned, seems to part with the whole of the water not essential to its composition at the temperature of 212°, or even perhaps below this point if exposed for a period sufficiently long.

It may not be deemed superfluous to notice here very briefly two or three circumstances resulting from the above analyses, which, though their importance may not be seen at present, should be constantly borne in mind, as they will enable us hereafter to throw light on many points connected with organization, which otherwise would be inexplicable.

In the first place, the identity of composition between the sugar of honey and arrow root, under the ordinary circumstances of the atmosphere, seems to show that the differences among the varieties of the amylaceous principles are precisely analogous to those existing among sugars, or in other words, that there are *low* starches as well as *low* sugars. Whether arrow root be the lowest that exists, I am unable to say; but I have met with none lower; and have reason to believe that the greater portion of the other varieties of the amylaceous principle known to exist, like the varieties of sugars above given, are intermediate in their composition between arrow root and wheat starch. The same remarks apply to other merorganized principles.

In the second place, the identity of composition between wheat starch and cane sugar, and between the sugar of honey and arrow root above mentioned, seems to show that, though merorganized bodies are not actually capable of assuming the crystalline form, yet that the original tendency among their essential elements to combine in certain proportions (and perhaps to assume certain forms) still continues to operate, though in a mitigated degree, and thus to exert, as it were, a feeble *nisus*, or endeavour toward the maintenance of certain definite modes of existence.

Thirdly, and lastly, crystallized bodies usually part with their water of crystallation with difficulty, and when they do, it is commonly *per saltum*, or in definite quantities. Merorganized bodies, on the other hand, retain water so feebly at all points, that within certain limits this fluid may be readily separated, or made to combine with them in every proportion. And this appears to be true, not only with respect to water, but with other substances capable of combining with merorganized bodies. It may be remarked also in general, that *low* varieties of principles resemble merorganized bodies in these and some other respects; thus, they usually part readily with all the water not essential to their composition at the temperature of 212°, or even less (provided they be submitted to it long enough,) above which point they rapidly undergo decomposition, &c.

Lignin, or the Woody Fibre.

Messrs. Gay Lussac and Thenard first showed that the hydrogen and oxygen in this principle exist in it in the proportions in which they form water, a result fully confirmed by my experiments. The variety of forms in which lignin occurs in different woods is so great, that an examination of them all would be quite out of the question; I therefore selected two, viz. the woods of the *Box* and *Willow*, which appeared to present the greatest contrast; the one being among the densest, the other the lightest of the woods. These were both treated exactly in the same manner; that is to say, they were first reduced to the form of a coarse powder by rasping, then well pulverized in a Wedgwood mortar, and afterwards sifted. Being by these means reduced to the form of impalpable powders, they were boiled in repeated portions of distilled water till that fluid came off unchanged: a tedious process, requiring several days to accomplish perfectly. After this they were similarly treated with alcohol, and finally again with distilled water. They were now exposed to the atmosphere, when in a dry and favourable state; and when they ceased to lose weight were submitted to analysis, and found to consist of (abstracting foreign matters)

	Box.	Willow.
Carbon	42·7	42·6
Water	57·3	57·4

A known weight of each was then exposed for twenty-four hours to a temperature of 212°, and afterwards for six hours longer (by means of an oil bath) to a temperature between 300° and 350°; and at the end of this time they were found to have lost, per cent.

Box.	Willow.
14·6	14·4

Analysed in this state of desiccation, they were found to consist of

Carbon......	50·0	49·8
Water	50·0	50·2

showing that the loss of weight arose from the escape of water. These latter results nearly agree with those of MM. Gay Lussac and Thenard, as obtained from the analyses of the woods of the *Oak* and *Beech*, and seem to show beyond a doubt, that the composition of all of them is similar, or that they consist of equal weights of carbon and water; to which simple analogy this important principle probably owes its stability.

Lignin undoubtedly exists in many other forms besides the woody fibre; indeed it appears to constitute the skeleton or groundwork on which most organic processes in the vegetable kingdom are carried on. To illustrate its properties as an *aliment*, the only point of view in which we have to consider it here, I shall briefly quote the experiments of Professor Autenrieth, of Tubingen, who showed some years ago, that by proper management this principle might be rendered capable of forming bread. The following was the method he employed for this purpose. In the first place, every thing that was soluble in water was removed by frequent maceration and boiling. The wood was then reduced to a minute state of division; that is to say, not merely into fine fibres, but actual powder; and after being repeatedly subjected to the heat of an oven, was ground in the usual manner of corn. Wood thus prepared, according to the author, acquires the smell and taste of corn flour. It is however never quite white, but always of a yellowish colour. It also agrees with corn flour in this respect, that it does not ferment without the addition of leaven, and in this case sour leaven of corn flour is found to answer best. With this it makes a perfectly uniform and spongy bread; and when it is thoroughly baked, and has much crust, it has a much better taste of bread than what in times of scarcity is prepared from the bran and husks of corn. Wood flour also, boiled in water, forms a thick tough trembling jelly, like that of wheat starch, and which is very nutritious[*].

It may be remarked that all the preceding principles are capable of being converted into oxalic acid by the action of the nitric acid, and into sugar by the action of dilute sulphuric acid.

Acetic Acid, or Vinegar.

This principle seems to have been more or less used as an aliment, either by accident or design, in every age and country. There have been various analyses of it published, by different chemists; but it is singular, that although some of them have given its exact composition, no one seems to have been struck with the most remarkable peculiarity of its composition[†], viz.

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[*] See the Edinburgh Magazine for November 1817, p. 313, where an account is also given of the Lapland mode of making bread from the bark of trees, as described by Von Buch. It is not improbable that during the above processes the lignin combines with water, and forms an artificial starch.

[†] Berzelius, in his paper On the definite proportions, in which the elements of organic nature are combined, assigns to vinegar this composition. See Annals of Philosophy, v. 174 (O.S.) Dr. Thomson also, in the last edition of his Chemistry, gives the same composition; though in his more recent work he has assigned to it another proportion of hydrogen.

that the hydrogen and oxygen exist in it in the proportions in which they form water. Some experiments which I made many years ago appeared to render this probable; but from the difficulties attending the analysis of this acid, and the uncertainty arising from the properties of the oxide of copper formerly stated, I was unable to satisfy myself completely on the subject. On repeatedly burning, however, a very fine specimen of the acetate of copper, in a given bulk of oxygen gas, with the apparatus described at the commencement of this paper, it was found that the volume of the gas underwent no change, and hence, that the above opinion was correct.

Acetic acid, freed from non-essential water, I find to be composed of

Carbon	47·05
Water	52·95

results which almost exactly agree with those of other chemists.

Sugar of Milk.

The sugar of milk employed in these experiments was prepared by myself in the usual manner, and rendered as pure as possible by repeated crystallizations. It was then freed from its hygrometric moisture by confinement under a receiver with sulphuric acid, and was found to consist of

Carbon	40
Water	60

results almost exactly agreeing with those of Berzelius.

Manna Sugar.—The saccharine principle existing in manna has been long known to possess peculiar properties. That employed in the following analysis was separated by means of alcohol in the manner commonly described in chemical books, and was obtained in a state of perfect purity by repeated crystallizations from that fluid. It was then dried at 212°, and in this state was found to consist of

Carbon	38·7
Water	61·3

results very different from those of M. Theodore de Saussure*. This sugar seems to part with hygrometric water only at the temperature of boiling water; but a few degrees above this point it begins to suffer decomposition, and at 250° it assumes, without melting, the form of a brown powder, and acquires a strong empyreumatic odour.

Gum Arabic.—A very fine specimen of gum arabic reduced to powder, and analysed as it existed under the ordinary circumstances

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* See *Bibliothéque Britannique*, 1814; also Annals of Philosophy, vi. 424.

cumstances of the atmosphere, was found (abstracting foreign matters) to consist of

Carbon	36·3
Water	63·7

One hundred parts of the same gum, exposed to a temperature between 200° and 212°, for upwards of twenty hours, lost 12·4 parts. Hence its composition thus dried would be nearly

Carbon	41·4
Water	58.6

results confirmed almost exactly by actual analysis.

The same gum, further exposed to a temperature between 300° and 350° for six hours longer, assumed a deep brown colour, and seemed to have suffered decomposition, though it lost in weight only 2·6 per cent. more. Hence, gum probably parts with the whole of the water not essential to its composition at the temperature of 212°, provided it be exposed for a sufficient time to this degree of heat.

Substances belonging to this series appear in general to be of a weak or *low* kind, though they are probably very numerous. They may be readily distinguished by being converted into saclactic acid by the action of nitric acid.

The vegetable Acids.

Oxalic Acid.—Many years ago I ascertained that this acid in the crystallized state consists of

Carbon	19·04
Water	42·85
Oxygen	38·11

a composition assigned to it long since by other chemists, and now I believe generally admitted, except by Dr. Thomson, who informs us that he has met with a specimen containing as much as half its weight of water[*]. I have examined a great many specimens with the view of verifying this result, but hitherto have not been successful.

Citric Acid.—This and all the following acids, except the *malic*, were analysed at the same period as the oxalic acid above mentioned, and the results have been recently verified. I find the crystals of citric acid to consist of

Carbon	34·28
Water	42·85
Oxygen	22·87

This composition has been approached very nearly by several chemists; but no one, so far as I know, has given it exactly.

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[*] Attempt to establish the first principles of chemistry by experiment, ii. 103.

Tartaric Acid in crystals is composed of

Carbon	32·0
Water	36·0
Oxygen	32·0

a composition assigned to it by Dr. Thomson in his work just quoted.

Malic Acid.—I am not acquainted with any analysis of malic acid except that of M. Vauquelin*, which has not, I believe, obtained much confidence among chemists, chiefly on account of the large proportion of hydrogen which he assigns to it. The acid I employed was obtained from the berries of the mountain ash by a process very similar to that of Mr. Donovan. It was not analysed *per se*, but in combination with lead, with lime, and with copper, and was found, abstracting water not essential to its composition, to consist of

Carbon	40·68
Water	45·76
Oxygen	13·56

This acid, in many points of view, may be regarded as one of the most interesting and important of all the vegetable acids.

Saclactic Acid.—The unexpected composition of this acid induced me to investigate its properties more fully than I had otherwise intended. What I first employed was obtained from the sugar of milk, and hence was tolerably pure, though not perhaps completely so. Latterly, I have preferred that prepared from gum, which, though exceedingly impure as first obtained, may be easily and completely purified by the following simple process.

Add ammonia in slight excess to the impure acid, and afterwards as much boiling distilled water as will dissolve the saclactate formed. Filter the solution while boiling hot, and then evaporate it very slowly nearly to dryness. The saclactate of ammonia will be separated in the form of crystals, which are to be washed with cold distilled water till they become quite white and pure. They are now to be again dissolved in distilled water, and the boiling saturated solution permitted to drop from a filter into cold diluted nitric acid. This latter of course decomposes the saclactate, and precipitates the saclactic acid in a state of perfect purity. Thus obtained, this acid was found to consist of

Carbon	33·33
Water	44·44
Oxygen	22·22

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* *Ann. de Chimie et de Physique*, tom. vi. 337.

results differing a little from those of other chemists, who probably did not take the necessary pains to obtain this acid in a perfectly pure state.

In conclusion, I wish to observe, that I purposely abstain at present from making any further observations on the preceding results than those already given. I do this for several reasons: in the first place, such observations will appear with greater effect, when the whole of the facts in my possession are laid before the public; and secondly, I consider that data which lead to such important conclusions as these appear to do, cannot be too firmly established; I therefore, in the mean time, earnestly invite chemists in general to repeat them, and thus either to confirm them, or point out their errors; and for the sake of those who may be inclined to take this trouble, I shall close this part of the subject with the following remarks: 1. The multiples of hydrogen, carbon, and oxygen, are assumed in the preceding calculations as 1 : 6 : 8. 2. The results given are, on all essential points, the means of many experiments, the differences among which are either inappreciable, or at most vary from ·01 to ·03 of a cubic inch in from 5 to 8 cubic inches of carbonic acid or oxygen gas; the greatest differences in general being, for obvious reasons, found among merorganized bodies; and hence the analyses of these are usually stated to the first decimal figure only. 3. As rules to be observed, I would say, that a single result should never be registered, nor a single calculation made, till the operator has made himself complete master of his apparatus, and carefully studied the nature of the substance to be analysed; for different substances often require very different management: that two or three results should never be relied on; the minute quantities here sought can be only obtained, like those of astronomy, by repeated observations: and lastly, the utmost care should be taken that the substances operated on be *pure*, a point of greater importance, and frequently of more difficult accomplishment than any other, and one that has caused me more trouble than all the rest put together.

XVIII. *On the Means of ascertaining the Purity of Sulphate of Quina.* By R. Phillips, *F.R.S. L. & E. &c.*

THE great demand which has arisen for this important medicine, and the high price at which it is necessarily sold, have excited some, who are careless as to the means by which they acquire gain, to sophisticate it in a vast number of ways, and by every means which talent misapplied could suggest.

Having repeatedly of late been requested to examine various samples of sulphate of quina, I thought it might be useful to state the several modes which may be employed for that purpose: and I make the present communication with the greater confidence, because I have received the very able assistance of my friend Mr. John T. Barry, of Lombard-street, to whose chemical skill, and the opportunity of frequently applying it, I am indebted for the greater number of hints and facts detailed in this paper.

Pure sulphate of quina has the form of minute fibrous crystals, it is inodorous, and its taste is bitter. If certain vegetable products, such as starch or sugar, be mechanically mixed with it, they may possibly be observed by merely inspecting the preparation with a glass.

1st. If the sulphate of quina be mixed with a considerable proportion of foreign matter, it may probably be detected by dissolving the salt in question in about three hundred times its weight of water,—say one grain in about five fluid drams of boiling distilled water. On cooling, pure sulphate of quina will be deposited in feathery crystals in twenty-four hours, if there be no adulteration.

2dly. As indirect, but as good collateral evidence, the taste of sulphate of quina of known good quality may be compared with that of another sample. Thus when pure, a grain of sulphate of quina will render nearly a pound and a half of water, or 10,500 grains, sensibly bitter.

3rdly. The alkalies either pure or their carbonates, if but slightly in excess, always occasion precipitation at ordinary temperatures in a solution of sulphate of quina containing only 1-1000dth of its weight, or less than one grain in two fluid ounces of water.

4thly. A solution of tannin occasions a very sensible precipitate in an aqueous solution of sulphate of quina, containing only 1-10,000dth of its weight of the salt, provided there be no acid in excess. Kino is that form of tannin which best answers the purpose. It is however to be observed, that the salts of morphia, cinchonia, strychnia, &c. are similarly affected by tannin; but they are not likely to be mixed with sulphate of quina.

5thly. Sulphate of quina suspected to contain sugar, gum, or other substances soluble in cold water, may be tried by digesting the same portion of the salt in small and successive portions of water to saturation. If the sulphate of quina be pure, and the solutions all properly saturated, they will have the same taste and specific gravity; and similar portions will yield by evaporation equal quantities of solid residuum.

6thly. A repetition of the above process, substituting alcohol for water, answers for extracting resin and some other substances, because sulphate of quina is soluble in alcohol to only a limited extent.

7thly. If a white substance insoluble in cold water be found in the sulphate of quina, heat the mixture to about 170° of Fahrenheit. This will render starch soluble, and its presence may be determined by the addition of an aqueous solution of iodine, which will immediately occasion a blue colour, and eventually a blue precipitate. The iodine should be added in very small quantity.

8thly. Sulphate of quina has been adulterated with ammoniacal salts. These are rendered obvious by adding a little of the suspected salt to a solution of potash. If any ammoniacal salt be present, ammoniacal gas will be readily detected, either by the smell, or by holding over the mixture a piece of turmeric paper, or a bit of glass moistened with acetic acid.

9thly. To ascertain whether sulphate of quina contains any earthy salts, such as sulphate of magnesia or sulphate of lime; burn a portion of it in a silver or platina crucible, or even in a clean tobacco-pipe. Any earthy salt, or any matter indestructible by heat, will of course remain in the vessel.

10thly. To ascertain that the sulphate of quina contains the proper quantity of sulphuric acid and quina, dissolve a little in pure muriatic or nitric acid, and add a solution of muriate or nitrate of barytes: 60 parts should give about 17·3 to 17·4 of sulphate of barytes; or the method may be varied without the trouble of drying the precipitate. Dissolve 60 grains of sulphate of quina in water slightly acidulated with muriatic or nitric acid; add a solution of 18 grains of nitrate of barytes, and separate the precipitated sulphate of barytes by filtering. If nitrate of barytes be now added to the clear solution, it should still occasion slight precipitation, for 60 of sulphate of quina contain 5·8 gr. of sulphuric acid, equivalent to 19·1 of nitrate of barytes.

This test is only to determine that there is no crystallized vegetable matter uncombined with sulphuric acid in the sulphate of quina; the detection of earthy or alkaline sulphates has already been provided for.

11thly. Sulphate of quina should lose not more than from 8 to 10 per cent of water by being heated till deprived of its water of crystallization. Mr. Barry informs me that he once examined a sample which contained more than 40 per cent of water in excess diffused through it.

XIX. *On the Phænomena of Water-spouts.* *By Mr.* JAMES MAIN*.

IN sailing from Madras to China, the usual course is through the Straits of Malacca and Sincapore. On the passage from Pulo-penang (*i. e.* the Island of areca-nut trees) to Malacca, the prospect from the ship was interesting: to the right, the thinly inhabited island of Sumatra was distinctly seen covered by impervious jungle, with lofty and thickly growing woods, even to the water's edge. To the left, the kingdoms of Quida, Malay, and Siam, on the continent of Asia, presented the same aspect; but, unlike many parts of the torrid zone, beautifully verdant and refreshing to the eye which had just left the sultry atmosphere and comparatively thinly planted coast of Coromandel. During the passage through those scenes, the navigation was tedious, and the weather uncommonly unsettled; the heat of the air was generally from 75 to 85 by Fahrenheit's thermometer. Occasional and insufferably hot gleams of sunshine, raised from the sea; and especially from the wood-covered and damp surface of the adjacent lands, a most copious evaporation; surcharging the air, and suddenly forming vast accumulations of clouds, all highly charged with electricity. In all directions lightning descended, followed by heavy and impetuous rain. The surrounding air and clouds converging to the spaces thus left unoccupied, generated various and contrary currents of air, wheeling the clouds in violent commotion; partial tornados were consequently created: these by their vertiginous course affected the adjacent and surrounding vapours, drawing them into the vortices; the grosser parts of this whirling body of vapour naturally inclined to the centre of the tornado, and there coalescing, formed the aqueous column called a water-spout.

The first appearance of this phænomenon is the lower end of the column impending from the base of a dark cloud in the shape of an inverted cone, in a somewhat waved direction, descending gradually to the sea or earth. When it happens to descend on the former, it is plainly discernible that a considerable body of air below and around the spout partakes of its dinetical motion; because the water is violently agitated therewith for a considerable time before the point of the cone reaches the surface; and during its approach, a cylindrical body of thick spray of much greater diameter than the spout, is seen raised from the waves, and appears to meet it in its descent; and when in collision, the agitation is extreme. The contact continues for ten or twenty minutes, according to the size of the spout;

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* Communicated by the Author.

and when exhausted, the lower end becomes broken, less defined, and shrinking as it were upwards, disappears as it began.

Some travellers have reported, that water-spouts rise out of the sea; having observed them, perhaps, only when the spout had descended and was in contact with the surface; or it might happen, that in such a case the tornado might be first generated below; and before the clouds were acted on, a column of spray would be seen to rise and approach the clouds, before a column of water is formed in them of sufficient density to descend. Similar appearances are sometimes seen on shore, when columns of dust, or, as the writer has often witnessed, even cocks of hay are carried up into the air by whirlwinds to a great height.

So frequent were these meteorous appearances during this part of the voyage, that on one occasion (when in sight of "The Rabbit and Coney," two small islands in the Straits, well known to navigators), no less than five water-spouts were seen from the ship at the same instant! some of which were falling on the land, and others on the sea;—the nearest to the ship being computed to be at the distance of five miles. The writer, who attentively observed these phænomena, used an excellent perspective glass, and on one occasion had an opportunity of seeing one while the sun shone upon it (about 7 A.M.), which appeared to be tubular; as the sun's rays were reflected in a double parallel stripe along that part of the spout on which they shone, viz. one stripe reflected from the exterior side next the sun, and another less bright from the interior surface (as was supposed) of the opposite side. This, however, is only a surmise; but from similar motions of fluids which may be seen at any time, there may be reason for supposing that the spout is not solid: for instance, if a hole be made in the bottom of any vessel, and plugged up with the end of a stick; let the vessel be filled with water and the plug withdrawn, the water in escaping through the aperture soon assumes a circular or rather a spiral motion, the descending column draws, or more properly speaking, admits after it an inverted cone of air, reaching from the surface of the water to the hole at which it escapes.

The idea of preventing the approach (for these phænomena are seldom stationary) of a water-spout upon a ship (which might produce dreadful consequences), by discharging great guns towards it, is reasonable; and if it did not approach too rapidly, no doubt would be effectual; because the concussion from the gun would counteract and break the sweeping current of the tornado, and thereby dissipate the motion-embodied fluid, and of course prevent its effects.

Many ideas occur to the beholder of such "elemental strife." Moses hath told us, that soon after the creation "there went up a mist from the earth, which watered the whole face of the ground;" and this necessary alternation of the ascent and descent of the element of water has continued ever since. The aspiring agency of heat, both from the earth and from that daily borrowed of the sun, is apparently the cause of the ascent of humidity; yet without the assistance of some other no less powerful agent, it is difficult to account for all the phænomena connected with the ascent and descent thereof. For though most copious between the tropics, and in our summer months, the lowest degree of temperature even within the arctic circle does not materially check evaporation, and the assumption by aqueous vapour of an invisible state in the atmosphere takes place as well in winter as summer. We find when the atmosphere is most fully charged with aqueous vapour, and has its temperature above the dew point, and consequently not a cloud to be seen; when almost all of it is exhaled from the surface of the earth; when vegetables and even animals languish,—the barometer is high: but when this sustaining power becomes diminished or withdrawn, a change takes place, a sensible precipitation of moisture (erroneously called dew) is felt, the air becomes turbid, clouds are formed, lightning is seen, thunder heard, and rain descends. The barometer, eased of its burden, sinks to the point of rain, or snow, or storm; and to this succeeds "a showery time," which continues till the atmosphere is again charged with that power which keeps its vapour in solution, and restores its transparency. Is electricity this agent?

These meteorous changes, so interesting to mankind, always suggest and force upon us the wish to obtain an efficient weather-glass; such an one as would be affected by the existence and motions of those powers which cause the changes in the atmosphere which are out of the reach of our perceptions. Perhaps an electrometer which would indicate the quantity, but especially the character and *direction* of that subtile fluid (if such an instrument could be constructed), would be of the greatest service in foreshowing the changes of the weather.

The above description and remarks are from actual observation. No reference has been made to any book, or other authority on the subject; and whether the writer has been deceived by appearances, and been led to erroneous conclusions as to the cause of them, must be left to the judgement of the reader.

6, Union Row, Chelsea,
April 21, 1827.

XX. *On*

XX. *On the Nursing Pouch or Chamber of the* Chama concamerata *of Gmelin. By* John Edw. Gray, *Esq. F.G.S. &c.*[*]

WALCH in the 12th volume of the *Naturforscher Journal*, of Berlin, described and figured (*t.* 1. *f.* 7.) a very curious shell, which Chemnitz has since named *Chama concamerata*. It has been considered as very rare; but having lately been brought to this country in considerable abundance, —through the kindness of Mr. Pratt, of Bath, I have been enabled to examine several specimens which had the dried animal in them: and as the formation of the cavity, which is itself an anomalous structure in Mollusca, and the use to which it appears to be applied by the animal, has not hitherto been described, I shall proceed briefly to describe them.

The cavity is formed by a folding-in of the middle of each of the valves; and this folding-in does not appear to take place till the shell has arrived at the middle period of its growth. It is marked externally by a groove formed by the coming together of the sides of the fold; the parietes of the cavity are thin, and marked with the same lines of growth as the valves themselves; the cavities or folds of both the valves are exactly opposite, and when the valves are closed, they meet so as to form a nearly closed chamber, which, although in the internal cavity of the shell, is completely external to the animal; and the contents of the cavity, like when the head is in a double nightcap, are on the outside of the shell. Indeed, the manner in which the cavity is formed may be easily understood by folding a piece of paper to the shape.

The chamber appears to be used by the animal as a *nursing pouch* to contain its eggs; for in all the specimens which I have seen, the cavity of each of the valves contained a group of oval, crumpled, pellucid bodies, adhering together, which, when they were soaked in water under the glass of a microscope, dilated, became semi-transparent, regularly oval, and had all the appearance of the eggs of bivalve Mollusca; but they are rather large for the size of the shell.

This is indeed a remarkable anomaly; for most *Conchiferæ*, as was described by Lister, aërate their eggs in their gills, and then emit them into the water to the protection of Nature. Some freshwater species, as the *Cyclades*, indeed, appear to keep them in the concave part of their shell, so as to be viviparous. I do not know of any instance but the above, where a pouch appears to have been formed for their protection; and what is more remarkable, the other species of the genus *Cardita*, to which Lamarck has referred this shell, are undoubtedly destitute of any thing of the kind.

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[*] Communicated by the Author.

XXI. *On an Apparent Violation of the Law of Continuity.*

By P. M. Roget, *M.D. F.R.S.*[*]

IT cannot fail to strike every philosophical observer of nature, that the greater number of changes which take place in the universe are effected only in a gradual manner; that each event is connected by insensible differences, both with the one which immediately precedes, and the one which immediately follows it; and that we may hence infer that the whole series of mutations which constitute any observed change, is perfectly uninterrupted and continuous. With regard to those changes which appear at first sight to be more suddenly effected, a careful analysis of the whole of the phænomena, and a more minute subdivision of the time during which they occur, will enable us to perceive the operations of the same general principle of successive and intermediate gradation of condition necessarily existing between opposite states of being. Thus, the apparently sudden impulse communicated to a ball by the explosion of gunpowder is, in reality, as much the result of a gradual communication of motion, during which the ball passes successively, though with great rapidity, through every intermediate degree of velocity, as in the case of the slowest increase of motion, which it might acquire by the constant operations of the greatest accelerating force. It was by extending these views to a variety of subjects of physical science, that Leibnitz was led to the conclusion, that the same principle pervaded every department of nature, and that *the Law of Continuity*, as he called it, was the law of the universe. *Natura non operatur per saltum* was his motto; implying that every thing that is executed in nature is done by indefinitely small degrees. Boscovich has assumed this principle as the foundation of his ingenious and profound theory of natural philosophy, and deduced from it a variety of important corollaries and conclusions. The law of continuity, says that philosopher, consists in this, that any quantity, whilst passing from one magnitude to another, must pass through all the intermediate magnitudes of the same kind: or, according to the law of continuity, all changes in nature are produced by insensible and infinitely small degrees; so that no body can in any case pass from motion to rest, or from rest to motion, without passing through all possible intermediate degrees of motion.

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[*] From the Scientific Gazette, Nos. 19 and 20, for November 5 and 12, 1825.—As this work has long since been discontinued, and was of confined circulation, we think Dr. Roget's interesting paper will be acceptable to the readers of the Philosophical Magazine.—Edit.

The simpler illustrations of this law are obvious, and must readily occur to those who seek for them; but it is important to familiarize the mind to them, with a view of preparing ourselves for the inquiry to what extent the principle admits of being carried, and whether it be really, as it is pretended, a universal law. It appears to obtain without exception in all those physical changes of situations, qualities, and conditions, which are connected with one another by their mathematical relations. All the forces and powers of nature vary by a continuous gradation from one period of time to another, or undergo differences of degree at different parts of space, without suffering any abrupt transition at any one point. Continuity is an attribute common to the law of gravitation, which acts at a distance, as well as to the laws of the corpuscular forces, of which the sphere of operation is limited to insensible distances, and which give rise to the phænomena of cohesion, of elasticity, and of all chemical combinations and decompositions. It belongs equally to the forces concerned in the phænomena of electricity and of magnetism. As the law of each of these forces may be expressed by some mathematical function of the distance at which they operate, so the same continuity must continue to pervade and characterize all the effects which can result from them, either when exerted singly, or in combination. The motions of bodies may in every case be regarded as the aggregate of the motions of every one of their particles; and the motions of any single particle, as well as of an aggregate of particles, produced by a continuous force, must be itself continuous, not only with regard to augmentations or diminutions of velocity, momentum, mechanical force, &c., but also with regard to the spaces they traverse, and the lines they describe in space. No deflection from a perfectly rectilineous course can take place but by infinitely small degrees; or, to express it in geometrical language, the line of motion cannot pass into any curve, of which that line is not the tangent at the point where the change of direction begins; far less, therefore, can a motion in one right line, suddenly pass into a motion in another right line, which forms any angle whatsoever with it. All angular motions are therefore excluded by the law of continuity, according to which abrupt transitions, either of velocity or direction, are physical impossibilities. The apparent violations of this law in the effects of the collision of elastic bodies, or their reflection after impinging on hard surfaces, and even the phænomena of the refraction and reflection of the rays of light, are shown by Boscovich to be really all in perfect conformity with the law of continuity.

Not content with establishing the universality of this law by induction, Boscovich has endeavoured to prove it by other arguments, of a still more positive nature, derived from abstract considerations. This universality, he says, arises necessarily from the very nature of continuity. The limit which joins the precedent and the consequent of any thing is common to both, and is therefore indivisible. Thus, a superficies separating two solids has no thickness, and is that in which a transition from the one to the other occurs; a line, dividing two portions of a continued superficies, has no breadth; a point, discriminating two segments of a continued line, has no dimension of any kind. So it is with regard to time: for the limit of two consecutive portions is common to both and indivisible; and as every change of a variable quantity from one magnitude to another must be made in time, so every change must participate in the continuity of time. But to every moment of time, a certain magnitude of the variable quantity corresponds, and the limit of two moments of time is common and indivisible; therefore the limits of two magnitudes, corresponding to these two moments, must, in like manner, be common and indivisible. Moreover, it is impossible for any quantity to have two magnitudes at the same time, and when continually varying, that it shall have the same magnitude at different times; much more impossible, therefore, that in the limit of two moments of time it shall have two magnitudes, the one course pending to the precedent, and the other to the consequent moment; or shall not have gone through the intermediate magnitudes in the intermediate moments of time. For the same reason, a body cannot have two velocities at the same time, and therefore cannot have two velocities in the limit common to two moments of time; and when continually changing its velocity, cannot have the same velocity in different moments of time, but must go through all the intermediate velocities in the intermediate moments of time. Hence, then, in passing from the magnitude 8 to the magnitude 12, the variable quantity passes through the magnitudes 9, 10, 11. The increase or diminution of temperature, for example, goes on gradually; the mercury in the thermometer rises or falls progressively, passing through every intermediate degree from one point of the scale to another. Now as this reasoning is unaffected by any considerations of the hardness, softness, elasticity, or other physical property of bodies, the universality of the law, as resulting from the nature of continuity, is a truth independent of such considerations. From these arguments, which I have given as stated by his able expositor Professor Robison, Boscovich has concluded that the law of continuity is essentially universal, and that a breach of it is metaphysically, as well as physically, impossible.

The severest test to which the validity of this mode of reasoning can be put, is to apply it to the pure mathematical relations of space and quantity, to which, if the law of continuity have any necessary existence in the nature of things, it ought to apply with the greatest rigour. We find, accordingly, that all the changes of magnitude in those quantities of which the value is dependent on that of certain other quantities, accompany corresponding changes in these latter quantities, in a manner strictly conformable with the law of continuity.

[To be continued.]

XXII. *On the Titaniferous Iron-slag of Königshütte in Upper Silesia, and on the Probability of its containing Tantalium. By Prof.* Hünefeld, *of Greifswalde**.

ALTHOUGH the investigations of Wollaston, Walchner, Rose, Du Ménil, Cordier, Vauquelin, Peschier, Berzelius, Zinken, Schrader, Karsten, and others, have shown that titanium is widely distributed, it has been nowhere found in great quantities. Wollaston found it in 1822 in the slag of the great iron-works at Merthyr Tydvil in Wales, in regular pale copper-coloured cubes†; and Walchner found the same in the slag of the pea-iron-ore of the High-furnace of Kandern in Baden‡; and Karsten, before this, in those of the Königshütte§. Karsten's observation has been but rarely mentioned, and has been especially neglected by Berzelius. It seems to me that it is particularly calculated to extend our knowledge of titanium (at least for the German chemists); and this has induced me to give an account of the titaniferous slag of the Königshütte, which I examined in 1824 at Breslau. I must however premise, that I had then no opportunity of combining the investigation, as to quantity, with that as to the nature of the substances.

The slag containing titanium, which was given to me for examination by my friend M. Müller, was thickly covered and filled with pale copper-coloured cubes of titanium; and I found that Dr. Wollaston's description was applicable to them in all respects. Peschier, it is well known, has declared it to be titanate of iron, of which, however, too little proof has been offered for it to be admitted as a fact ||.

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* From Schweigger's *Jahrbuch*, N. S. Band xx. p. 332.

† See Philosophical Magazine, vol. lxii. p. 18. and lxiii. p. 15.

‡ Ibid. vol. lxvi. p. 124.

§ Karsten's *Archiv*. iii. 524.

|| Walchner has declared himself decidedly against this opinion, and

A portion of the slag was digested in aqua regia: the greater part of it was dissolved, with evolution of sulphuretted hydrogen gas, whilst a considerable number of the small cubes of titanium remained undissolved, having a perfect metallic lustre. But besides these, there remained in the residuum a blackish powder, containing grains and minute laminæ, nearly of the colour of silver, and having a metallic lustre, to which I shall return in the sequel. Another portion of the slag was ignited with nitre, in order to oxidate the titanium. The melted mass dissolved in water gave a beautiful green solution, which, when exposed to the air, soon assumed first a deep red, and then a dark violet colour, and at last lost all colour *, whilst portions of the protoxides of manganese and iron were deposited. Subsequently, this deposit (marked *a*) and the sides of the vessel became covered with fine crystalline iridescent laminæ and needles, probably titanate of potash precipitated by the substances having attracted carbonic acid from the air.

The residuum of the dissolved mass in the porcelain crucible was washed and dried, by which it assumed the colour of iron-rust. It was partly dissolved in muriatic acid, and thereby gave a solution of iron, and left a blackish powder (*b*). The filtered strongly alkaline re-acting liquid was evaporated, and mixed with nitric acid (*c*). After standing somewhat longer, it gave crystals of nitre, and then others of an indistinct form. They were mostly white, somewhat opalescent, not easily soluble grains of salt, cracking and decrepitating between the teeth, and which, with salt of phosphorus, were melted before the blowpipe into a clear bead, which within the flame received no colour, either by itself or mixed with tin. The solution was rendered somewhat turbid by caustic ammonia, and white flakes were deposited. Boiled and dissolved with potash, and acidulated with muriatic acid, infusion of galls precipitated it of a dark yellow; but hydrosulphuret of potash and ferroprussiate of iron gave no precipitate; which leads me to suppose that those grains of salt were *tantalate* of potash.

The fluid *c* had the following properties. With an alcoholic infusion of galls, a yellowish-orange precipitate was formed, which was dissolved on being heated; and when it was concentrated, it gave a brown liquid, which remained clear on cooling.

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shown that the iron between the laminæ of the cubes of pure titanium had been mechanically interposed; whence (like Wollaston) he also explains its magnetism.

* The cracks of the crucible contained a beautiful orange-red efflorescence in needles, which were not examined. Were they manganesate of potash, or sulphuret of titanium with sulphuret of potassium? (Vid. Berz. *Jahresber.* v. 134.)

cooling. Sulpho-cyanate of potash dissolved in alcohol produced a red colour; with a greater addition, a dark rose colour; but with impure sulpho-cyanate of potash (still containing ferroprussiate of potash), a greenish white precipitate; with the ferroprussiate of potash itself a dark grass green precipitate, which was in part dissolved in the remaining liquid, giving it the same colour; and it was also dissolved by nitric acid. This last solution turned gradually brownish green *. Another solution made with muriatic acid, gave, by contact with a cylinder of zinc, the usual indications of titanium.

The black powder *b* was digested in aqua regia, which extracted the iron and manganese, leaving however a considerable proportion of a black powder, which under the burnisher gave an iron-gray streak of a metallic lustre, became inflamed even before it arrived at a red heat, and yielded a whitish substance, which, with a few deviations, bore the characters of *tantalic acid.* These deviations were probably produced by a small portion of manganese, which was indicated in the trial by the blowpipe.

I subsequently obtained, through the friend above mentioned, a larger quantity of slag containing titanium. It had a still greater quantity of large and small regular cubes of titanium, which were affixed on the outside as well as to the inner cavities of the slag. They had nearly all the same regular form and the rose copper colour, except a few very small groups, which tended somewhat to an orange tint, and had but a faint brilliancy (sulphuret of titanium?). But besides these visible crystallizations of titanium, the slag had some other interesting substances upon it.

1. Granules of metal melted into it of the shape of beans or spheres, weighing from two to thirty grains.
2. Cavities of different sizes, the sides of which, when filed, had a fine lustre as of steel.
3. Several grains of metal as it were sprinkled in the slag, of a globular or oblong shape, with a metallic lustre, and a colour between that of silver and that of tin. In their chemical properties they resembled the small lustrous metallic leaves which have been mentioned above; they scratched glass, gave a shining powder, and seemed therefore to be *tantalium,* such as we have found it hitherto described.
4. Melted grains of metal, partly globular, with a tint verging upon the colour of brass.
5. Portions of melted slag, of a dark rose red nearly. They

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* See Pfaff's *Neue Versuche über das Verhalten der Titansäure gegen verschiedene reagentien* (New Experiments on the Habitudes of Titanic Acid with different re-agents); *Jahrb.* b. xv. p. 372; and Pfaff's *Analyt. Chem.* b. ii. p. 523.

all showed, on being filed, a fine metallic lustre, were very hard and tough, but not malleable, some scratching glass distinctly, others but feebly.

No. 1.—The metallic granule was attracted by the magnet, showed, when filed, a bright steel-lustre, which continued in the air; it was very hard, not ductile, but after many blows with a hammer, flew into several pieces, and showed a fine-grained fracture. These fragments could not be melted even in the most intense flame of the blow-pipe; they only became slightly diminished in lustre. Treated with muriatic acid, they yielded odorous hydrogen; the muriatic acid dissolved iron, the granule turned black, and after a longer digestion crumbled into a black dust. In aqua regia, a little more was dissolved; about five-sixths remained, which, when washed and polished with a glass stick, gave a streak of a metallic lustre similar to molybdena. When moistened, it gave a smell of hydrogen gas, such as is given by manganese. Thus it remains undecided whether this quality belongs to a small residuum of manganese, which however is probably the case, since the black powder treated with salt of phosphorus yielded a pearl of a faint amethyst colour, whilst otherwise nearly the whole covered the pearl of salt of phosphorus with a metallic pellicle; and this result remained unchanged on the experiment being continued. Treated with soda and borax, no other re-action took place but that of the salt being covered with a metallic pellicle, which remained unchanged even after the pearl had been repeatedly touched with solution of caustic potash, and a scrap of tin added to it. If, however, the granule of slag had not been sufficiently treated with aqua regia, the black metallic powder gave a blueish green substance with soda, with salt of phosphorus, a yellowish brown pearl in the external flame, a greenish one becoming clear on cooling in the inner flame, and which therefore contained iron and manganese.

After several experiments made with the black powder, I could perceive, at least in this manner, no indication of titanium; but another granule, also treated with aqua regia, left undissolved small rose copper coloured crystals of titanium. Another piece of a melted granule in the titaniferous slag gave on the filed surface visible particles of titanium, as it were sprinkled among it. The granule just described could be filed like the other portions described in what follows, adhering a little to the file.

No. 2.—These parts proved to be almost entirely the same as the former, but were a little harder, and yielded more of the black metallic powder.

No. 3.—They scarcely lost any thing in aqua regia, preserved their lustre, and did not crumble. Similar granules remained behind in another experiment with the same solvent, scratched glass, and when beaten and broken with a hammer, made red hot with caustic potash, and treated with the blowpipe apparatus as well as with humid re-agents, did not give any indications of titanium. A portion, however, having been melted with potash, and acidulated with nitric acid, the solution remained for several days exposed to the air, and being afterwards filtered, was by means of a solution of galls precipitated rather abundantly, of a dirty orange colour; whilst hydrosulphuret of potash produced a scarcely perceptible turbidity (*tantalium?*).

No. 4.—The brass colour was only superficial, the mass underneath almost entirely resembling Nos. 2 and 3.

No. 5.—The dark rose red, yellowish, and metallic colour of these pieces extended deeper in the last-mentioned portions; they evidently contained titanium, were more difficultly filed, were magnetic, and might contain a mixture of iron and titanium on the surface, one of tantalium and iron, and some titanium itself further in.

The black powder obtained from the portion of slag of the other mass, was ignited with carbonate of soda; the mass, which when cold appeared white with blueish green margins, was dissolved in water, in which the green colour, which is the property of manganese, disappeared. A white, somewhat loose and flaky powder was precipitated. The remaining fluid, mixed with muriatic acid, when treated with oxalate and ferroprussiate of potash, gave a yellowish red precipitate, small in quantity. The fluid, when exposed to the atmosphere, turned to a fine grass-green, without the least deposit of cyanuret of iron; it was not acid. An infusion of galls produced a dirty yellowish white precipitate, which after some time became grayish white, and subsequently yellowish green. Sulphocyanate of potash produced a reddish deposit. Neither the solution nor the precipitate gave any indications of titanium on being treated with salt of phosphorus; nor even after a short contact with a cylinder of zinc, tin or iron, muriatic acid having first been added to the solution. But on mixing another portion of the dissolved mass with nitric acid, drying it and treating it with salt of phosphorus and tin before the blowpipe, I obtained, by adding a little oxide of iron, a glass of a hyacinth colour, which by a greater addition of the evaporated solution, turned to a violet blue, and became almost transparent.

On comparing these experiments with the slag, with the known chemical properties of the substances, it is evident that they contained not only an abundance of *titanium* (as has been shown before by Karsten), but probably also *tantalium.* As both these metals are so scarce, especially *tantalium,* I have thought it of use to call the public attention to this slag.

It is not improbable but that the titanium originated by reduction from prototitanate of iron and manganese, and the tantalium from the tantalates of the same bases (*tantalite*). It would be a very meritorious performance, and highly important to the geology of Silesia, if any one would institute a strict examination of the iron ore which yielded this slag, as well as of its locality, and the process of smelting it undergoes. Perhaps by so doing some pure tantalium, or combinations of that metal, as well as of titanium (and perhaps wolfram), may be found in larger or smaller quantities. It need scarcely be mentioned, that titanium is found in the Riesengebirge of Silesia, especially in the Iser (*Iserin*) in the shape of *nigrin.*

XXIII. *Notices respecting New Books.*

THE following are the Contents of those Parts lately published, of the Philosophical Transactions, and of those of the Linnean and Astronomical Societies:

Philosophical Transactions for 1827.—*Part* II.

On a new form of the differential thermometer, with some of its applications. By William Ritchie, A.M.—On the structure and use of the submaxillary odoriferous gland in the genus *Crocodilus.* By Thomas Bell, Esq.—On the permeability of transparent screens of extreme tenuity by radiant heat. By William Ritchie, A.M.—On the derangement of certain transit instruments by the effects of temperature. By Robert Woodhouse, A.M.—On some of the compounds of chromium. By Thomas Thomson, M.D.—Rules and principles for determining the dispersive ratio of glass; and for computing the radii of curvature for achromatic object-glasses, submitted to the test of experiment. By Peter Barlow, Esq.—On the change in the plumage of some hen-pheasants. By William Yarrell, Esq.—On the secondary deflections produced in a magnetized needle by an iron shell in consequence of an unequal distribution of magnetism in its two branches. First noticed by Captain J. P. Wilson. By Peter Barlow, Esq.—On the difference of meridians of the royal observatories of Greenwich and Paris. By Thomas Henderson, Esq.—On some observations on the effects of dividing the nerves of the lungs, and subjecting the latter to the influence of voltaic electricity. By A. P. W. Philip, M.D.—On the effects produced upon the air cells of the lungs when the pulmonary circulation is too much increased. By Sir Everard Home, Bart.—Theory of the diurnal variation of the magnetic needle, illustrated by experiments. By S. H. Christie, Esq. M.A.—On the ultimate composition of simple alimentary substances;

with some preliminary remarks on the analysis of organized bodies in general. By William Prout, M.D.

Presents received by the Royal Society:—Meteorological Journal.

Transactions of the Linnean Society of London.—Vol. 15th. Part II.

Some account of a collection of cryptogamic plants from the Ionian Islands. By Robert Kaye Greville, LL.D.—Description of a new genus belonging to the natural family of plants called *Scrophularinæ.* By Mr. David Don, Libr. L.S.—On *Boswellia* and certain Indian *Terebinthaceæ.* By Henry Thomas Colebrooke, Esq.—The natural history of *Oiketicus,* a new and singular genus of Lepidoptera. By the Rev. Lansdown Guilding, B.A.—Observations on the tracheæ of birds; with descriptions and representations of several not hitherto figured. By William Yarrell, Esq.—On two new genera of land tortoises. By Thomas Bell, Esq.—Of the insect called *Oistros* by the ancients, and of the true species intended by them under this appellation: in reply to the observations of W. S. MacLeay, Esq., and the French naturalists. To which is added, a description of a new species of *Cuterebra.* By Bracy Clark.—A review of the genus *Combretum.* By Mr. George Don.—Description of a new genus of plants belonging to the order *Nymphæaceæ:* in a letter to H. T. Colebrooke, Esq. By Nath. Wallich, M.D.—Observations and experiments, made with a view to ascertain the means by which the spiders that produce gossamer effect their aërial excursions. By John Blackwall, Esq.—Descriptions of two quadrupeds inhabiting the South of Africa, about the Cape of Good Hope. By Andrew Smith, M.D.—An account of a pair of hinder hands of an orang otang, deposited in the collection of the Trinity-House, Hull. By John Harwood, M.D.—On systems and methods in natural history. By J. E. Bicheno, Esq., Sec. L.S.—An account of a new species of Pinus, native of California. By David Douglas.—Remarks on the Antilope Chickara: in two letters addressed to the Secretary. By Robert Hills, Esq.—Extracts from the minute-book of the Linnean Society, &c. &c.

Memoirs of the Astronomical Society, Vol. 3rd. Part I.

Observations made in the Island of Cuba. By the late Don José Joaquin de Ferrer, of Cadiz, member of the Phil. Acad. Boston. Comprehending, 1. Observations of the comet of 1807, with the determination of the elements of its orbit; 2. Observations of the lunar eclipse, Nov. 14, 1807; 3. Observations of the comet of 1813, with the determination of the elements of its orbit, together with remarks on its magnitude, and that of the comet of 1811; 4. Observations, and computations of the elliptic orbit, of the comet of 1811.—On the longitude of Port Bowen, by the method of moon-culminating stars. By Lieut. H. Foster, R.N.—Approximate places and descriptions of 295 new double and triple stars, discovered in the course of a series of observations with a 20-feet reflecting telescope: together with some observations of double stars previously known. By J. F. W. Herschel, Esq. Pres.—Notice respecting some errors common to many tables of logarithms. By Charles Babbage, Esq., For. Sec.—Astronomical observations made at Bushey Heath (north latitude 51° 37′ 44″,3; west longitude, in time, from Greenwich 0^h 1^m 20^s,93), in the years 1825 and 1826. By Colonel Mark Beaufoy: comprehending, 1. Transits of the moon and moon-culminating stars; 2. Occultations of stars by the moon; 3. Lunar eclipses; 4. Eclipses of Jupiter's satellites.—On a new application of the method of determining the time by observations of two stars when in the same vertical, to the case of *Polaris*, when so situated with respect to any other circumpolar star in the course of its diurnal revolution below the pole. By Dr. Tiarks.—On the passage of the comet of *Bootes* over the disc of the sun on the 18th of November 1826. In a letter from M. Gambart to J. F. W. Herschel, Esq. Pres.—On a new period of eclipses. By James Utting, Esq.—On an appearance, hitherto unnoticed, in the nebula of *Orion*. By John Pond, Esq., Astronomer Royal.—Notice of a comet discovered by M. Flaugergues at Viviers, March 29th, 1826. Extracted from a letter from M. Flaugergues to F. Baily, Esq., Pres.—Astronomical observations: I. Observations taken at Stargard, and the Paramatta observatory, New South Wales, in the years 1825 and 1826. By Dr. Rumker. 1. Of the great comet of 1825; 2. Of the comet in *Leo* in 1825; 3. Of the lunar eclipse in May 1826—of an occultation during the eclipse—and of Mars near the opposition in 1826. II. Observations of the solar eclipse in November 1826, taken at Bushey Heath. By Lieut. George Beaufoy, R.N. III. Observations of a comet in *Eridanus*, and of *Ceres*, *Pallas* and *Vesta*, near their oppositions in the year 1826, taken at Padua. By Professor Santini. IV. Observations of the eclipses of Jupiter's satellites, taken at the Madras observatory, in the years 1817—1825. By John Goldingham, Esq. V. Observations taken at Calcutta in the year 1822; and an extract of a letter from Major J. A. Hodgson to Dr. Gregory. 1. Of the transit of Mercury over the sun's disc in November, 1822; 2. Of occultations of stars by the moon; 3. Extract of a letter relative to the mode adopted for determining the times of the observations of Jupiter's satellites, recorded at p. 440, vol. ii. of the Mem. Ast. Soc.—Report of the council of the society to the seventh annual general meeting.—Address delivered at a special general meeting of the Astronomical Society of London, on April 11, 1827, &c.

XXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Dec. 6, 1827.—THOMAS Henry Hall, Esq., and William Phillips, Esq., were admitted into the Society.

A paper was read, entitled, "On the Corrections in the Elements of Delambre's Solar Tables, required by the Observations made at the Royal Observatory at Greenwich; by G. B. Airy, Esq., M.A., Lucasian Professor of Mathematics in the University of Cambridge, communicated by J. F. W. Herschel, Esq."

The author was desired by the Board of Longitude to examine the discordancies between the right ascension of the sun, as observed at

Greenwich since the erection of the new transit instrument, and as computed by the solar tables of Delambre, which are used in the computation of the Nautical Almanac, with a view to the discovery of the errors in the elements of those tables. The result of the comparison at first indicated the necessity of a correction of the epochs of the sun's longitude, and of the longitude of the perigee, and perhaps also of the equation of the centre. But upon pursuing the examination through a series of years, it became manifest that some other source of irregularity existed, and that this could be no other than an erroneous estimate of the masses of some of the planets, especially of Venus and of Mars. A more critical examination showed that there was also an error in the assigned mass of the moon. It was found necessary in these investigations to take into account an error which occurred in the tables with regard to the secular motion. It results from the author's researches that the epochs for 1816, and those for 1821 to 1825, ought to be increased respectively by 4″,734 and 5″,061; that of the perigee increased by 46″,3, and the greatest equation of the centre diminished by 0″,84; the mass of Venus should be reduced in the proportion nearly of 9 to 8, and that of Mars nearly in the proportion of 22 to 15. On a comparison of these results with those which have been derived from an examination of some of Dr. Maskelyne's observations as given by Burckhardt in the *Connaissance des Tems* for 1816, they are found on the whole to agree in the most satisfactory manner.

Dec. 13.—A paper was read, entitled, "On the measurement of high temperatures; by James Prinsep, Esq., Assay Master of the Mint at Benares, communicated by P. M. Roget, M.D. Sec. R.S."

In the commencement of this paper the author describes the many abortive endeavours of former experimenters to obtain instruments for the accurate admeasurement of high temperatures, and afterwards describes several attempts of his own to effect this very desirable object. In the course of his inquiries a remarkable fact presented itself in the change which occurred in an index constructed on the compensation-principle, and formed by two slips of metal, the one of silver, the other of gold, originally quite pure, and united without any alloy. In the course of a few years, although it had never been subjected to a very high temperature, the surface of the gold became converted into an alloy of silver, the impregnation extending gradually to a considerable depth in the gold, and destroying the sensibility of the instrument to changes of temperature.

After trying various plans, he gave the preference to the one founded on the following principles: namely, that the fusing points of the pure metals are fixed and determinate; that those of silver, gold, and platina, comprehend a very extensive range of temperature; and that between these three fixed points in the scale, as many intermediate ones as may be required, may be obtained by alloying the three metals together in different proportions. When such a series of alloys has been once prepared, the heat of any furnace may be expressed by the alloy of least fusibility which it is capable of melting. The determinations afforded by a pyrometer of this kind will, independently of their precision, have the advantage of being identifiable at all times and in all countries; the smallness of the apparatus is an additional recommendation, nothing more being necessary than a little cupel, containing in separate cells the requisite number of pyrometric alloys, each of the size of a pin's head. The specimens melted in one experiment need only to be flattened under the hammer in order to be again ready for use. For the purpose of concisely registering the results, the author employs a simple decimal method of notation, which at once expresses the nature of the alloy, and its correspondence with the scale of temperature. As the distance between the points of fusion of silver and of gold is not considerable, the author divides this distance on the scale into ten degrees; obtaining measures of each by a successive addition of 10 per cent of gold to the silver, the fusion of which, when pure, marks the point of zero, while that of gold is reckoned at 10 degrees. From the point of fusion of pure platina to that of pure gold, the author assumes 100 degrees, adding to the alloy which is to measure each in succession, one per cent of platina. The author then enters into a detailed account of the method he employed for ensuring accuracy in the formation of the requisite series of alloys, and of various experiments undertaken to ascertain their fitness as measures of high temperatures. The remainder of the paper contains the recital of the author's attempts to determine, by means of an apparatus connected with an air-thermometer, the relation which the fusing point of pure silver bears to the ordinary thermometric scale.

A paper was likewise read, entitled, " On Alimentary Substances; by Sir George Smith Gibbes, M.D. F.R.S."

After a few remarks on the very scanty assistance which physiology and pathology have yet derived from the chemistry of organized substances, notwithstanding the great improvements which have been lately introduced in the methods of analytical research, the author maintains the proposition, that all alimentary substances, whether belonging to the animal or vegetable kingdoms, retain some principles originally the result of vitality, which they communicate to other bodies, and thus constitute their food. He considers the appearances of the infusory animalcula, and more especially of the *Monas Termo,* during the decomposition of alimentary substances as favouring this hypothesis. In support of this argument, he adduces experiments by Dr. Ingenhouz, himself, and others, from which he infers that these animalcula perform some definite function during the growth of vegetables.

From the circumstance of chloride of lime preventing the development both of vegetation and of infusory animalcula, the author infers that these animalcula are instrumental in the production and growth of plants.

LINNÆAN SOCIETY.

Dec. 4.—Read a paper " On the Locomotive Power of the Snail, by Mr. James Main." The author describes as the species which have chiefly come under his notice the following :—*Limax maximus: ater:*

ater: lichenivora: rufus: mutabilis: tenax: and *agrestis.* The belly of the snail being perfectly smooth, there are no appendages to do the office of feet; and the whole of the body moves at once, and not in parts successively. By placing the animal on a piece of glass, Mr. Main was enabled to observe a muscular motion; but this, instead of being from head to tail, was directly the reverse, so that the animal's motion cannot be caused by impulses in the direction of its progress. He gives, in conclusion, two conjectures as to the cause of the animal's motion; namely, 1st, that the body is moved forward by the retromissive discharge of slime, which being emitted simultaneously from every part of the under surface, he conceives may exercise a force adequate to the propelling of the animal:—or 2ndly, from its power of forming its lower surface into segments of circles along the whole of its length; and thus by assuming a vertical vermicular action on the plane of the sustaining surface, impelling the body forward by alternate contraction and expansion.

As dry air deprives the animal of motion, Mr. Main is inclined to consider the first surmise the more probable.

Read an extract from a letter from Dr. Rigby to Mr. R. Taylor, dated Berlin, on the ova of the *Hirudo medicinalis*;" with some specimens.

Read also "An account of *Margarōdes*, a new genus of insects found in the neighbourhood of Ants' Nests; by the Rev. Lansdown Guilding, B.A., F.L.S. &c." Mr. Guilding, after quoting Dr. Nugent (Geol. Trans. vol. v. p. 463), who states that the *ground pearl* (improperly supposed to be fossil) is found in prodigious quantity in the furrows of the land in Antigua when newly turned up, and suggests that it may be the production of an insect, informs us that he has succeeded, by watching some that he preserved in moist marl, in detecting the insect which issued from them. He conceives it to be a parasite on the ants, whose formidable numbers in the dry islands, they are calculated to keep down. The entire want of a mouth is remarkable in this new insect, the food being absorbed by tubes in the fore claws. It also possesses the extraordinary power of throwing out long filaments, when in dry situations, supposed to be for preserving itself by obtaining moisture. Its scales effervesce and disappear in nitric and muriatic acid; sulphuric turns them black; and vinegar slowly decomposes them. In flame they burn like horn. Mr. G. is uncertain at present what station is to be assigned to this insect.

Dec. 18.—Three new species of Land Tortoise were exhibited by Mr. Bell:—*Testudo pardalis, actinodes.* and *tentoria,* descriptions of which are given in the number of the Zoological Journal just published.

Read a portion of Dr. Hamilton's "Commentary on the *Hortus Malabaricus.*"

Jan. 15, 1828.—Some specimens of *Ianthinæ* were exhibited by L. W. Dillwyn, Esq., washed ashore in July last in Oxwick bay near Swansea, many of them picked up alive and yielding a beautiful dye. Specimens of the *Medusa Vellela* and *M. Navicula* were found with them.

Read "Descriptions of three new species of Plants, natives of New Granada, by M. Gondot, Professor of Botany at Bogota: the plants are,

1. *Sessea corymbiflora* foliis obovatis attenuatisque, floribus corymbosis.—In woods near Bogota.

2. *Cinchona Muzonensis* foliis ovato-oblongis acutis basi attenuatis, stipulis revolutis, paniculâ brachiatâ, corollis albis, limbo imberbi. —In great forests near the city of Muzo.

3. A plant of a Genus nearly akin to *Theobroma*, from which it differs chiefly in habit, in the form of the calyx, and the structure of the stamens. Monadelphia Dodecandria. *Büttneriocæ*, Brown. "Arbuscula foliis digitatis, quinatis."—Forests near Muzo. Called by the inhabitants the Cacao *Montaras* or *Symoron*. The poor people mix the fruit with the cultivated cacao.

GEOLOGICAL SOCIETY.

Dec. 7.—John Braddich, Esq. of Boughton-Mount near Maidstone; G. W. Featherstonhaugh, Esq. of Duanesburgh, New York; Arthur Kett Barclay, Esq. of Grosvenor Place; and Lord Francis Leveson Gower, of Albemarle Street, were elected Fellows of the Society.

A paper was read, "On the Geology of Quebec and its Vicinity, by J. T. Bigsby, M.D. F.L.S. G.S." &c. &c.

The author, who acknowledges the assistance he has derived from the manuscripts of Lieut. Skene, R. E., first describes the tract, on the eastern termination of which the city of Quebec is situated, as an oblong ridge of about seven miles and a half in length, and in average width about one mile and a half; subsiding on the north-west by steep and rocky slopes, into rich meadows; whilst on the south-east it advances in the form of cliffs towards the northern bank of the St. Lawrence.

Various rivers traverse the district above mentioned, nearly from north to south, of which the most considerable are the St. Charles and the Montmorenci. On the southern bank of the St. Lawrence, Point Levi is the most conspicuous promontory; and to the west of it the country is intersected by several streams running from south to north.

Diluvium.—The districts above mentioned are partially covered with boulders of gneiss, granite, syenite, and labrador felspar; the greatest quantities of which are found on and near Cape Diamond, Point Levi, and Point Montmorenci; whilst occasional deposits of clay, gravel, and sand, including organic remains, the author supposes to be of diluvial origin,—and not produced by the operation of any existing watercourses.

The rocks of this region repose upon each other in the following descending order:—1st. A slaty series, composed of shale and grauwacke, occasionally passing into a brown limestone, and alternating with calcareous conglomerate in beds, some of which are charged with fossils.—2nd. A conchiferous brown and black limestone sometimes based upon a calcareous conglomerate.—3rd. *Gneiss.* The author's chief reason for considering the slaty-series as superior to the limestone, is, that the latter is in some situations in immediate contact with gneiss; while in others it passes into beds of the first series above mentioned; of which the conglomerates contain organic remains derived from the conchiferous limestone.

1. The *slaty-series* occupies the whole of the southern shore of the St. Lawrence, the Island of Orleans, and a considerable portion of the north bank of the river, including the ridge upon which Quebec is placed. In that neighbourhood the mass of the deposit consists of a black and brown slaty limestone, inclined at high angles, in some instances nearly vertical; and alternating with semi-crystalline limestone, and various conglomerates. The limestone contains several varieties of crystallized carbonate of lime, intermixed with quartz crystals, and occasionally traversed by seams of bituminous matter. Near Cape-Rouge, and on the plains of Abraham and Kilgraston, some of the strata consist of red and greenish clay-slate. In the calcareous conglomerates, organic remains are mixed with fragments of clay-slate, and the beds alternate with compact gray limestone and quartzose layers. Between Quebec and Cape-Rouge, boulders of primary rocks, and fragments of compact grauwacke, are buried deep in the red schist.

The channels of the various streams east and west of Quebec, afford instructive sections, which, according to the author, prove these slaty deposits to be more recent than the conchiferous limestone.

On the south side of the river St. Lawrence, the slaty limestone of Quebec is no longer seen, but several new beds of conglomerate present themselves; one of the lowest of which contains trilobites, encrinites, corallines, and other fossils associated with vegetable impressions, probably *fuci* and *amansiæ*. In the schistose beds near the mouth of the Etchemin are thin seams of coal; and at the village of St. Henry the slate is so compact as to be used for hones.

2. The horizontal conchiferous limestone occupies a zone from two to three miles in breadth, on the north of the slaty tract, and included between the slate and a mountainous range of gneiss. It is exposed in the beds of all the rivers which flow southwards into the St. Lawrence, and its characters are well developed at the falls of the Montmorenci and the St. Charles, and at the quarries of Beaufort. The organic remains consist of several species of trilobites, orthoceræ, terebratulæ, encrinites, ammonites, &c. On the Montmorenci the beds are from eighteen inches to two feet in thickness, nearly horizontal, and of a blackish-brown colour; in one situation they pass into a subjacent calcareous conglomerate, whilst in other places the limestone itself contains large blue nodules, and reposes immediately upon gneiss. At Beaufort-quarries, ledges of fetid limestone alternate with calcareo-bituminous shale, containing organic remains similar to those noticed on the Montmorenci.

From the characters and fossils of the limestone above described, the author regards it as the same with the *calcaire intermediaire* of D'Aubuisson, or the equivalent of the "Carboniferous-limestone" of English geologists.

Dec. 21.—Henry Holland Stutzer, Esq. River-Terrace, Islington, was elected a Fellow of the Society.

The reading was begun of a paper "On a group of Slate-Rocks in Yorkshire, between the rivers Lune and Wharfe, from near Kirby Lonsdale to near Malham, by John Phillips, Hon. Mem. of the Leeds and Yorkshire Philosophical Societies."

1828. Jan. 4.—John Murray, Esq. Jun. of Albemarle Street; Henry Tuffnell, Esq. of Christchurch, Oxford; The Right Hon. Viscount Cole, of Christchurch, Oxford; R. C. Fergusson, Esq. M.P., of Craigdarroch, Dumfriesshire, and of Great Cumberland Street; John Phillips, Esq. of York, Hon. Mem. of the Yorkshire, Leeds, and Hull Philosophical Societies; and John Gurdon, Esq. of Assington Hall, Suffolk,—were elected Fellows of the Society.

The reading of Mr. Phillips's paper begun at the last meeting, was concluded.

The object of this paper is to describe the geological structure and relations of a group of rocks which the author characterizes as "aberrant from the slate district of Cumberland," and extending about fifteen miles towards the east under the summits of Greygarth, Ingleborough, and Pen-y-gent,—a tract remarkable for the variety and singularity of its geological appearances, among which the proofs of dislocation are peculiarly striking and important.

To this description a sketch is premised of the slate-series of the Lakes of Westmorland and Cumberland, where the rocks are grouped in three principal divisions, the lowest consisting of dark soft slate much contorted, with fine-grained gneiss beneath it passing into granite. The second division occupies a country of very different aspect from that of the slate. The mountain-ranges being marked by abrupt precipices, as at Helvellyn, Langdale Pikes, and the Lakes of Ulswater, &c. and composed of brecciated argillaceous rocks containing calcareous spar, green-earth, and calcedony, with greenstone and other forms of trap. On the south of this chain is a tract of transition limestone, containing caryophyllia, productæ, spiriferæ, and other fossils; and this is covered by a third zone of slate, the most recent rock of the country, usually divisible into rhomboidal blocks, of which two principal varieties are observable, alternating with each other; the one homogeneous and fissile, and containing organic remains sparingly distributed, of the genera trigonia, pecten, gryphea, turritella terebratula;—the other more granular and micaceous. This formation is in some cases succeeded by red conglomerate, but more commonly by mountain-limestone, the lowest beds of which contain numerous pebbles of slate and quartz; and above the limestone are the carboniferous rocks, including the millstone grit and the upper coal-measures. The highest strata known in the country, consist of the new red sandstone, placed in an unconformable position above the coal formation.

The tract, which is the more immediate object of this paper, extends from the valley of the Lune in an easterly direction, to that of the Wharfe. Along its middle, from Casterton Fells to a few miles east of the Ribble, ranges an almost continuous line of argillaceous rocks, generally fissile, and belonging to the third division of slates above mentioned. This tract is bounded on the north by the elevated strata that support the summits of Greygarth and Pèn-y-gent; and on the south (in consequence of great dislocations) by millstone grit and the coal measures. If the rivers Lune and Wharfe are included, no fewer than nine streams cross the district from north to south, and exhibit very distinctly the structure and relations of the rocks; the greater number of the streams cutting through the limestone and millstone grit, exposing the subjacent slate, and finally passing off on the depressed strata of the coal measures. The author describes in detail, the phænomena presented in these several sections, and illustrates his observations by sectional views and sketches.

The structure of the country is very well displayed in the course of the Ribble, where the slate first appears on the north, beneath parallel bands of limestone; while on the south, the carboniferous strata, of which the northern portion is horizontal, decline at a high angle, thus indicating a vertical dislocation of about four hundred feet. Besides this fault on the southern verge of the slate, another still more important one may be traced in a parallel direction across the valley of Ribbles-dale, and over Malham Moor; by which, strata have been brought into opposition, that in their original place were separated by a thickness of more than five hundred feet. Various facts are stated by the author in proof of this derangement, and descriptive of the phænomena produced by it.

The author subjoins to his descriptions some remarks on the stratification of slate, and on the difficulty of discriminating between the planes of general stratification, or dip, and those of the cleavage effected by a blow,—the latter of which are often disposed at considerable angles to those of the dip. He is disposed to think, that in the fissile granular varieties of slate approaching to sandstone, the laminæ of cleavage may really be those of deposition; since the surfaces are frequently coated with mica, and the fossil remains are in a disposition parallel to them. Besides this more general cleavage, however, the slate is also traversed by other planes, oblique to those of cleavage, and less conspicuous, to which the quarry-men give the name of "Bate." The direction of these planes, though nearly alike in limited spaces, is found to vary considerably in different portions of the same tract; and even the better-defined planes of the ordinary cleavage are seldom parallel to each other throughout any great extent of country.

A collection of fossil vegetables, chiefly from the Jarrow and Felling collieries in the Northumberland and Durham coal-field, presented to the Society by William Hutton, Esq. of Newcastle-upon-Tyne, with drawings describing the plants according to the system of M. A. Brongniart, was accompanied with some remarks by the donor, comprised in a catalogue.—The collection consists of Calamites, Sigillaria, Sagenaria, Stigmaria, Filices, Sphænophyllum, Asterophyllum, &c.; also several specimens of undescribed confervæ, leaves, stems, &c.

A notice was read on the occurrence of "Chlorophæite" in basaltic dykes in Northumberland; and of carbonate of strontian in the lead measures at Fallowfield near Hexham, by William Hutton, Esq. of Newcastle-upon-Tyne.

The author discovered "Chlorophæite" in a basaltic dyke near the river Coquet, about two miles N.E. of Felton, in the form of small nodules, which upon fracture exhibit the changes of colour and appearance mentioned by Dr. MacCulloch, who first found this mineral in the Isle of Rum. This substance has also been observed by the author at Coaley Hill near Newcastle, in the steatitic or earthy form, and but rarely crystallized.

ASTRONOMICAL SOCIETY.

Dec. 14.—A letter was read from Maj. Hodgson the Surveyor-General of India, dated from Calcutta, April 18, 1827, accompanied by two copious lists of astronomical observations. The *first* contained a series of transits of the moon and some of the principal fixed stars, as observed at his own house (situated 5 seconds in time, east of the Flag-staff of Fort William) from November 18, 1826 to March 20, 1827, both inclusive. The observations are above 300 in number: and the stars observed are not those which are usually denominated moon-culminating stars, but consist of some of the principal stars only; and many of these situated occasionally at very considerable distances from the moon in declination. In fact, the list of moon-culminating stars has never yet been computed early enough to enable it to be sent to such distant places in sufficient time for the observations. It is therefore to be feared that correspondent observations of many of the stars observed by Major Hodgson may not be found at the observatories in Europe. The results of the transits are regularly computed by Major Hodgson, agreeably to a printed form, where every step of the process is minutely laid down, and of which he has forwarded to the Society two examples. It is Major Hodgson's intention to continue his observations for one whole year at least, and to transmit them to the Society, together with the daily computations showing the results. The instrument employed was a 31-inch transit by Dollond; but he is in expectation of a more powerful one shortly from England. The *second* list contained a series of upwards of 200 observations of the eclipses of Jupiter's satellites taken by gentlemen in the civil service, and by officers of the Bengal army, at different times and at different places. The dates extend from July 18, 1795 to April 11, 1827: and the names of the observers are given, together with the length, the aperture, and the magnifying power, of the telescope employed. Major Hodgson states that he shall transmit several others as soon as he can collect them: for (he observes) "observations of this kind have been of much use in determining geographical positions in this widely extended country; and the probability of obtaining correspondent observations through the medium of the Astronomical Society, will excite travellers and surveyors to take advantage of all opportunities of making observations of the eclipses of the satellites of Jupiter, and of the culmination of the moon and stars."

A paper was also read, entitled "On the computation of the geocentric places of the planets for ephemerides:" by J. F. Littrow.

The usual mode of computing the geocentric places of the planets for ephemerides consists in instituting a first computation of their heliocentric positions, which are then reduced to geocentric by well known formulæ. To avoid the very laborious calculations entailed by this process, Prof. Gauss devised a method much more expeditious, and which gives at once the geocentric right ascensions and declinations, as well as the distance of the planet from the earth, in terms of the co-ordinates of the earth and planets at the instant of observation. The equations expressive of these relations are stated by M. Littrow, and are extremely simple. By them, the right ascension (α), the declination (δ), and mutual distance (ρ) are given directly as follows:

$$\tan\alpha = \frac{Y+y}{X+x}$$

$$\tan\delta = \frac{Z+z}{X+x}\cdot\cos\alpha \qquad (I)$$

$$\rho = \frac{Z+z}{\sin\delta}$$

X, Y, Z being the co-ordinates of the earth, and x, y, z of the planet.

Now it is obvious, that if we neglect the perturbations (on which neglect it will be observed the application of this method mainly depends), all these co-ordinates are, ultimately, functions respectively of the mean longitudes of the earth and planet, and their values may therefore be tabulated as such; and the tables, once constructed, will last till the secular variations of the elements of the planetary orbits shall have so changed them as to produce an error surpassing that which can be tolerated in an ephemeris.—These tables once computed, the foregoing equations reduce the annual computations to the utmost brevity. The only point then to consider is the construction of the tables themselves.

The expressions of the co-ordinates in terms of the mean longitude are made to depend on the true longitude as the independent variable, by means of six constant quantities for each planet. These are, 1st, the respective inclinations of the plane of the planet's orbit to the solstitial and equinoctial colures and the earth's equator; and, 2ndly, the plane angles made by the line of intersection of the planet's orbit, and the ecliptic respectively with its line of intersection with the solstitial and equinoctial colures and equator. The author states the method of computing these constants from the known elements of the orbit, and gives their actual numerical values for all the planets with their secular variations.—These once known, and the true longitude, as a preliminary step, computed for every mean longitude, he states the equations by which the co-ordinates are formed from them for every given value of the longitudes; and, finally, embodies the whole result of his computations in a table stating their values for each of the planets, and for intervals of 4° for Mercury and Venus, 3° for Mars, 2° for Jupiter, Saturn, and Uranus, and for each day in the year for the Sun.

The publication of the 1st Part of the 3rd Volume of the Memoirs of the Society, was announced by the President.

At the conclusion of the business of the evening, An instrument contrived by Mr. Henry Atkinson, of Newcastle-upon-Tyne, to illustrate some of the phænomena of rotation, was submitted to the inspection of the members, by Mr. Riddle.—It consists of a flat circular disk, through the centre of which passes a small endless screw, having a cup in one of its extremities. When the cup is *below* the centre of gravity, and set on a point on which the disk (inclined to the horizon) is made to rotate in its own plane, the whole rotating plane performs at the same time, a slow revolution round the perpendicular to the horizon, which passes through the point of rotation; and the revolution of the plane is in the *same* direction as the rotation. When the point of rotation is *above* the centre of gravity, the plane moves slowly round in a direction *contrary* to that of rotation, affording an apt *illustration* of *precession.* When the point of rotation is *in* the centre of gravity, the position of the revolving plane continues permanent.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

At a special meeting of this Society, assembled for the express purpose, the following Address of Congratulation was unanimously adopted.

To Davies Gilbert, Esq. M.P. &c. &c.

Sir,—At the last annual meeting of this Society, over which you have presided from its first institution, and which is so deeply indebted, in its origin and advancement, to your zeal and ability, we ventured to express a hope that your continued exertions, and successful labours in the wider fields of philosophy and science, might be crowned with honour in a higher sphere, by your being elected to the office of President of the Royal Society of London.

It is gratifying to us to see that our wishes are fulfilled; and we most sincerely congratulate you on being chosen to fill that chair, which having been the seat of Newton, may be deemed the throne of intellectual eminence.

That two of our countrymen, natives of this remote province, almost of the same spot, should have been raised in succession to the dignity of this station, is to us a source of pleasure and of pride.

As members of this Society, we rejoice at an appointment which forms for us, though at an humble distance, a connecting link with the most renowned Society of our country, and brings us within the influence of its notice and support; for although your enlarged mind will lead you equally to protect and encourage every branch of science and knowledge, since they all mutually assist each other, blending their various colours into one ray of intellectual light; yet we shall indulge the feeling, that Geology has, as it were, a filial claim upon your regard, and will experience that kind and fostering care which may fairly be asked for a science, now only in its infancy.

While we request you to accept our congratulations, we deeply lament the occasion of our illustrious townsman's resignation.

Be pleased, Sir, to take an early opportunity of conveying to Sir Humphry Davy our condolence on his illness, and our sincere wishes for his restoration to perfect health, and to the exercise of those faculties which he has employed to the noblest purposes—the enlargement of the boundaries of human knowledge, and the gaining for his name the most enviable kind of immortality (such is his own estimate) "that which is connected with the gratitude and blessing of his fellow-creatures."

May the enjoyment of health enable you, Sir, to continue for many years in the discharge of the high situation in which you have been so honourably placed, with happiness to yourself, with satisfaction to your associates, and with benefit to the world.

(By order of the Royal Geological Society of Cornwall,)
JOSEPH CARNE, Treasurer,
Penzance, Dec. 22, 1827. Chairman of the Meeting.

ROYAL ACADEMY OF SCIENCES OF PARIS.

May 28.—The following are the titles of the works or manuscripts received by the Academy at this sitting:—Description of a new steam-apparatus for boats; by M. Tourasse.—New facts relative to the therapeutic employment of the Pyrothonide, by M. Rauque.—Notice respecting comets; by M. Courbon, surgeon. —New notice on the bursting of steam-boilers; by M. Tabareau. —New theory of the phænomena of vision; by M. Plagge.

M. Delessert read a letter from M. Brunel on the subject of the accident which happened at the Thames Tunnel.—M. Thenard, on the part of the Commissioners, read a favourable account of the memoir by M. Isidore Boullay.—M. Bonastre had presented a memoir On the compounds of the oil of cloves and of pimento with the alkalies, and several salifiable bases. The principal fact contained in this memoir is, that these oils, which do not redden litmus, combine nevertheless with salifiable bases; but M. Chevreul, reporter of the Academy, stated, that this fact not being accompanied with the necessary details and precision, is devoid of the interest which it may hereafter possess. M. Bonastre was recommended to continue his researches.

June 4.—The Minister of the Interior communicated the edict of the King, by which the nomination of M. Cassini, jun., as a free academician, is confirmed.—M. Girard presented a notice respecting hydraulic mortars, made with fossil argillaceous sands.—M. Dulong, in the name of the Commission, made a report relating to the compression of liquids.—M. Cordier read the first part of a memoir On the temperature of the interior of the earth.—M. Bonnard read a memoir On the regularity of the geognostic facts which exist in the territory of Arkose in the East of France.

June 11.—At this, which was a public sitting, M. Cuvier read An historical eulogium respecting M. Hallé.—M. Dupin read Statistical researches respecting the canals of the north and south of

France.—M. Cuvier read An historical eulogium respecting M. Corvisart; and M. Cordier, a memoir On the temperature of the earth.

June 18.—The following are the titles of the memoirs or manuscript works presented to the Academy:—Notice respecting two mussels and a toad, which were taken alive from a well which had been filled up for one hundred and fifty years; by Dr. Quenin, mayor of Orgon.—A sealed packet, containing An account of a new process for breaking calculi; by M. Cazenave.—A sealed packet, containing New chemical researches; by MM. Quesneville and Julia Fontenelle.—Description of a method by which boats may go up rivers alone and without expense or danger; by M. Anatasi.—Design for a new pump for fire-engines; by M. Stolz.—New considerations respecting the boilers of steam-engines; by M. Tabareau.—On certain organs of the *Hymenoptera*, *Diptera*, &c.; by M. Robineau-Desvoydi.—Researches respecting the vibration of several sonorous bodies, and particularly elastic cords; by M. Cagniard de la Tour.—Notice respecting the extinct volcanoes of the South of France, the eruptions of which occurred since the deposition of the second fresh-water formation; by M. Marcel de Serres.—On the elementary principles relating to the division of territories.—The Minister of the Interior announced his having sent the Academy a collection of the fossil bones found in the grottoes of Osselles.—M. Geoffroy-Saint-Hilaire sent from Montélimart several particulars which he had collected respecting the scientific establishments of the South of France.—M. Blainville, in the name of the Commission, read An account of an extremely curious memoir, by MM. Raspail and Robineau-Desvoydi, but which appears to require new researches.—M. Brochant gave a favourable account of the new memoir by M. Bonnard On the territory of Arkose.—M. Constant Prévost read a memoir, entitled An examination of the geological question, "Whether the continents which we inhabit have been repeatedly covered by the ocean?"

June 25.—MM. Raspail and Robineau-Desvoydi announce that they possess myriads of *Alcyonellæ*, and sent a packet of them.—M. Lacroix gave a favourable account of M. Denaix's work, entitled A methodical comparative geographical essay.—M. Cuvier read a memoir On the *Scarus* of the ancients.—M. Berthier read A notice of four memoirs which he presented to the Academy on some new mineral species.—M. Roger communicated some results which he had obtained respecting the height of Mont-Blanc.—M. Raspail read a memoir, entitled A physiological analysis of the *Spongilla friabilis* of Linnæus.

The Academy decided that the place vacant by the death of M. Ramond should be supplied.

July 2.—M. Pons, of Florence, informed the Academy that he had discovered on the 20th of June, a small comet, invisible to the naked eye, in the constellation of *Cassiopæia;* and according to a letter from Marseilles, M. Gambart also observed it at 2 o'clock in the morning of the 21st of June.—M. Beudant reported, in the name of the Commissioners, very favourably with respect to four memoirs which M. Berthier had presented to the Academy at one of its late sittings.—M. Prévost read an extract from a new memoir on geology.

July 9.—The Minister of the Marine forwarded Observations on various subjects, which had been sent to him from New Holland, by M. Durville.—M. Rembielinski presented a memoir, entitled *Description des courbes-productionelles.*—M. Velpeau read a memoir, entitled *Récherches sur l'œuf humain.*—M. Sérullas read a memoir already noticed in the *Annales.*—M. Cordier, in the name of a Commission, gave a favourable account of the memoir lately read by M. Prévost.—The Commission for the presentation of candidates for the place of Foreign Associate, vacant by the death of Volta, was composed of MM. Arago, Fourier, Legendre, Cuvier, Thenard, and Desfontaines.—A secret Committee of the Section of Mineralogy presented the following list of candidates for the place vacant by the death of M. Ramond: MM. Bonnard, Berthier, and Constant Prévost.

July 16.—The lamented death of M. Fresnel was announced to the Academy.—M. Keller, engineer in the Marine, requested permission to deposit a sealed packet.—M. Boucharlat also sent a sealed packet, containing the results of his experiments upon ammonia, and an analysis of a memoir On the possibility of considering ammonia as an hydracid as well as an alkali.—M. Cuvier reported respecting the fossil bones found in the cavern of Oisselles near Besançon.—M. Berthier was elected a member of the Academy.—M. Andreossy presented an historical notice of the giraffe.—M. Turpin read a memoir On the structure and reproduction of the truffle compared with those of the toadstool.

July 23.—M. Arago communicated several new experiments on bromine, by M. Aug. de la Rive.—M. Cordier finished the reading of his paper On the temperature of the earth.—M. Turpin concluded his memoir On the reproduction of the truffle.—M. Dutrochet communicated some observations on *Endosmosis* and *Exosmosis.*

July 30.—M. Roger sent the Elements of the calculation by which he determined the height of Mont-Blanc.—M. Thenard, in the name of a Commission, reported that the manuscripts of M. Reineck, who died at Ancennis, contained nothing worthy of being printed: the examination was made at the request of the Minister of the Interior.—M. Collard de Martigny communicated an announcement of the principal consequences deduced from his researches into the alterations produced in the quantity and composition of the blood and lymph, by complete abstinence from liquid and solid food.—M. Binet read a memoir On the determination of the orbits of planets and comets.—MM. Robiquet and Colin read a second memoir on madder.—M. Savart presented a new memoir On the vibrations of solid bodies. — At a secret sitting, the President, in the name of the Commission of the 9th of July, presented the following candidates for the vacant place of Foreign Associate. In the first rank, Dr. Thomas Young, of London; and afterwards,

terwards, in alphabetical order, MM. Bessel, of Kœnigsberg; Blumenbach, of Gottingen; Robert Brown, of London; Leopold de Buch, of Berlin; Dalton, of Manchester; Olbers, of Bremen; Œrsted, of Copenhagen; Plana, of Turin; and Soemmering, of Frankfort.

Aug. 6.—The following memoirs were received at this sitting:—On the use of chloride of lime in disinfecting the air in which silkworms are kept, by M. Bonafous; On the propagation of heat in a triangular prism, by M. Ostrogratzki; On an artificial nose from the skin of the forehead, by M. Delpech; On the motion of fluids in the atmosphere, by M. Le Chevallier.—Dr. Young was elected Foreign Associate of the Academy.— M. Geoffroy-Saint-Hilaire presented the head of a young giraffe, in which it was seen that the bony nucleus of the horn in the young animal is separated from the forehead by a distinct suture. There were read a memoir On some electrical phænomena produced by the pressure and cleavage of crystals, by M. Becquerel; On the compression of gases, by M. Despretz; On a new theory of sonorous vibrations, by M. Cagniard-Latour; On the operation for the artificial pupil, by M. Faure.—M. Molard gave a favourable account of tachygraphy, and of the tachytype, invented by M. Conti.

Aug. 13.—Titles of memoirs received;—Memoir in continuation of the history of quina, cinchonia and kinic acid, by M. Henry; Remarks on the weight and dimensions proper for the sails of machines in order to produce the desired effects, by M. Landormy; A third notice on the motion of fluids, by M. Le Chevallier; Memoir on two cases of luxation of the cervical vertebræ with compression of the spinal marrow, by M. Barny.—M. Chevreul read a notice On the discovery of phocenic acid in the alkanet.—M. Lisfranc read a memoir On *rhynoplastie*, and presented an individual who had successfully undergone the operation.—M. Geoffroy-Saint-Hilaire communicated Some observations on a polydactyl horse, with toes separated by membranes.—M. Savart read a memoir On vibrations. The Academy received at this sitting two verbal reports: one by M. Freycinet, On M. Balbi's ethnographical atlas; the other by M. Silvestre, on M. Francœur's work On the teaching of linear drawing.

Aug. 20.—The ordonnance of the King confirming the nomination of M. Berthier was received.—A letter from M. Pons announced the discovery of a new comet.—M. Fossombroni communicated a report respecting the Cæsarean operation lately performed in a hospital at Florence.—Titles of memoirs received: Researches and observations upon false consecutive aneurism, by M. Breschet; Discoveries on the treatment of scrofulous affections, and a process for healing *fistula in ano* (a sealed packet), by M. Deygallières; New observations on yellow fever, by M. Leymerie.—M. Cassini, in the name of a Commission, gave a favourable account of M. Turpin's researches on the reproduction of truffles.— M. Pouillet read a memoir On electro-magnetism.—M. Chabrier read An extract of some new observations on the progressive motion of animals.

Aug. 27.—The Academy received a memoir from M. Madelaine On steam-engines.—M. Chevreul gave a very unfavourable account of a new process, proposed by M. Ratieuville, for dyeing wool of a blue colour.—The following papers were also read: Researches on the determination of the series which represent the functions given in one part only of their extent, by M. Brisson; A memoir on the dyeing of woollen with prussian blue, by M. Raymond; A memoir on the formation of sulphuric æther, by MM. Dumas and Boullay; Researches on the human ovum, by M. Velpeau.—M. Frédéric Cuvier gave a favourable verbal account of the Baron Gérando's work On the education of the deaf and dumb.

Sept. 3.—The Minister of the Interior transmitted the ordonnance of the King, by which the nomination of Dr. Young was confirmed. —M. Deygalières sent two Observations on scrofulous diseases, cured according to the principles of his new method.—M. Tournal, junior, of Narbonne, announced the discovery of caverns containing fossil bones, near that city.—M. Lalanne presented an instrument which he names *sécateur perspectif.*—Mr. Perkins read a memoir On new high-pressure machines.—M. Dumeril gave an account of the researches of M. Velpeau on the human ovum.—M. Chevreul, in the name of the Commission, made a favourable report respecting the memoirs of M. Sérullas.—M. Blainville began the reading of a report on the work of M. Jacobson On the reproduction of bivalves. —M. Parseval presented a memoir On the integration of linear equations.—M. Cauchy read a memoir On the determination of the series of Lagrange, and the conditions of convergence.

Sept. 10.—M. Rambur, a physician of the department of Inare and Loire, sent a description of a monstrous child which had two bodies and only one head.—M. Blainville made a verbal communication respecting the organization of a species of *Terebratula.*—M. Navier read the report respecting M. Clement's memoir On the escape of elastic fluids and safety valves, &c.—MM. Julia Fontenelle and Poisson read a notice respecting a new kind of paper made from liquorice.—M. Boisduval read a memoir On the monograph of the tribe *Zyneides.*

Sept. 17.—M. Raspail announced that he had discovered in the subterraneous logs of typha, a fecula possessing very peculiar characters, which he details.—M. Milne Edwards deposited a sealed packet.—M. Haldat sent two memoirs, one On diffraction, and the other On magnetism in motion.—The following memoirs were read: On the general system of the internal navigation of France, by M. Brisson; A second memoir, by M. Cauchy, On the application of the calculus of residuums to physico-mathematical questions; Researches into the vertebral organization of the lower order of animals, by M. Robineau Desvoidy.—On the magnetic action exerted in all bodies by the influence of a very powerful magnet, by M. Becquerel.

Sept. 24.—M. Gendrin, a physician, sent some researches into the heat of hot springs.—M. Mozembas, of Bordeaux, sent a memoir On the means of constructing lightning conductors with œconomy.

nomy.—M. Tournal sent a second letter On the caverns containing fossil bones, which he had discovered.—M. Raspail read a memoir On the *Alcyonellæ* of marshes.—M. Gerard, in the name of a Commission, gave a favourable account of the last memoir by M. Vicat. —M. Dugez read a memoir On a monstrous conformation in the heart of a new-born infant.—M. Chevreul gave a detailed account of the recent labours of MM. Robiquet and Colin On the colouring matters of madder.—M. Villermet read a memoir entitled, *Statistique des conceptions.*—M. Velpeau communicated new researches On the human ovum.

Jan. 7, 1828.—Mr. Ivory was nominated a correspondent, in the section of Geometry.

XXV. *Intelligence and Miscellaneous Articles.*

ON THE FLUIDITY OF SULPHUR AND PHOSPHORUS AT COMMON TEMPERATURES:—BY MR. FARADAY.

I PUBLISHED some time ago a short account of an instance of the existence of fluid sulphur at common temperatures; and though I thought the fact curious, I did not esteem it of such importance as to put more than my initials to the account. I have just learned through the *Bulletin Universal* for September, p. 178, that Signor Bellani had observed the same fact in 1813, and published it in the *Giornale di Fisica*, vol. vi. (Old Series.) I also learn, by the same means, that M. Bellani complains of the manner in which facts and theories, which have been published by him, are afterwards given by others as new discoveries; and though I find myself classed with Gay-Lussac, Sir H. Davy, Daniell, Bostock, &c. in having thus erred, I shall not rest satisfied without making restitution,—for M. Bellani, in this instance, certainly deserves it at my hand.

Not being able to obtain access to the original journal, I shall quote M. Bellani's very curious experiments from the *Bulletin*, in which they appear to be fully described. "The property which water possesses, of retaining its fluid state, when in tranquillity, at 10° or 15° below its freezing point, is well known: phosphorus behaves in the same manner; sometimes its fluidity may be retained at 13° (centigrade ?) for a minute, an hour, or even many days. What is singular is, that, though water cooled below its freezing point, congeals easily upon slight internal movement, however communicated, phosphorus, on the contrary, sometimes retains its liquid state even at 3°, even though it be shaken in a tube or poured upon cold water. But as soon as it has acquired the lowest temperature which it can bear without solidifying, the moment it is touched with a body at the same temperature, it solidifies so quickly, that the touching body cannot penetrate its mass. If the smallest morsel of phosphorus is put in contact with a liquefied portion, the latter infallibly solidifies, though it be only a single degree below the limit of temperature necessary;—this does not always happen when the body touching is heterogeneous.

" Sulphur presented the same phænomena as phosphorus; fragments of sulphur always produced the crystallization of cold fluid portions. Having withdrawn the bulb of a thermometer which had been plunged into sulphur at 120°, it came out covered with small gobules of sulphur, which remained fluid at 60°; and having touched these one after another with a thread of glass, they became solid: although several seemed in contact, yet it required that each should be touched separately. A drop of sulphur, which was made to move on the bulb of a thermometer, by turning the instrument in a horizontal position, did not congeal until nearly at 30°; and some drops were retained fluid at 15° *i. e.* 75° of Reaumur below the ordinary point of liquefaction."—*Quarterly Journal*, Jan. 1828, p. 469.

ELEMENTARY NATURE OF BROMINE.

Iodine colours a solution of starch blue, bromine renders a similar solution orange colour. M. A. de la Rive added a few drops of bromine to a solution of starch coloured blue by iodine, and obtained a compound which gave two distinct colours with starch—one brown, the other yellow; the difference of colour corresponding with the two bromides of iodine described by M. Balard. These compounds of iodine and bromine, dissolved in a solution of starch, were subjected to the voltaic pile: immediately the yellow solution became blue about the negative pole and orange about the positive pole, indicating the separation and places of the iodine and bromine. Thus the smallest quantity of iodine may be discovered in bromine; but when the experiment was resorted to, to prove whether the idea thrown out, that bromine was a compound of chlorine and iodine, was founded in fact or not, it gave no such indication, and a solution of bromine in starch electrified for a long time together, gave no appearance of iodine. Hence M. de la Rive concludes, that bromine contains no iodine, but is an element analogous to iodine and chlorine. When bromine and iodine are combined, the former passes to the positive pole, and is consequently more negative than the latter; which accords with the observation of M. Balard, that it should occupy a place between chlorine and iodine. According to the *Bulletin Universel*, when the letter to M. Arago, containing an account of the facts above referred to, was read to the Academy of Sciences, that body decided that the assertion of M. Dumas, that bromine was a compound of chlorine and iodine, should be considered as retracted, and that it should be so entered upon the *procès-verbal* of the sitting.—*Ibid.* p. 466.

QUANTITY OF BROMINE IN SEA WATER.

One hundred pounds of sea water, taken up at Trieste, treated by chlorine, ether, &c. according to M. Balard's process, produced five grains of bromide of sodium, or 3·278 grains of bromine. It would appear, that in the sea water of Trieste, the bromine is unaccompanied by any iodine; and the same is the case, according to M. Hermbstadt with the waters of the Dead Sea. In the water of the Mediterranean, on the contrary, iodine always appears with the bromine.—*Ibid.* p. 466.

SALE OF BROMINE.

The discoverer of bromine, M. Balard, has been enabled, by his improvements, to prepare that peculiar body in quantities sufficient to permit its sale. It may be obtained at his shop, Rue Argenterie at Montpellier, or at M. Quesneville's manufactory of chemical substances, at Paris. The price is four francs the gros (about 60 grains), fourteen francs the half ounce, and twenty-three francs the ounce.—*Ibid.*

PREPARATION OF IODOUS ACID.

M. Pleischl says that, in preparing this acid, three parts of chlorate of potash with one of iodine are to be used, and not equal parts, according to M. Sementini; and also that it is indispensable to cool the receiver considerably during the whole operation.—*Ibid.*

NEW BORATE OF SODA.

M. Payen lately presented to the Society of Pharmacy a new borate of soda, which will advantageously be substituted for calcined borax. It crystallizes in regular octahedrons, is harder than common borax, and is almost as sonorous as cast iron: its fracture is vitreous, and rather undulated. When immersed in water, the crystals become opake, and retain their opacity in dry air.

This borate differs but little from common borax, except in containing less water of crystallization. It is more convenient for soldering copper than common borax, because it does not swell so much.—*Journal de Pharmacie*, Dec. 1827. p. 624.

ANALYSIS OF A SALIVARY CONCRETION.

This concretion was analysed by M. Lecanu: it weighed 0·45 gramme, its form was oval and slightly rough in the surface: when broken it presented two distinct layers; the central one was hard, compact, and of a gray colour; the outer one was friable and perfectly white. It yielded by analysis:

Phosphate of lime	75
Carbonate of lime	20
Animal matter and loss	5
	100

This salivary concretion differs, then, in its composition from those previously analysed by MM. Wollaston, Laugier, and Henry jun.;—from the first, by the presence of carbonate of lime; from the second, by the absence of carbonate of magnesia; and from the third, by the absence of carbonate and phosphate of magnesia and common salt. It contains the same principles as the salivary concretions of the horse, elephant, and cow, analysed by M. Lassaigne.—*Ibid.* p. 626.

HAIDINGERITE,—A NEW MINERAL SPECIES.

This substance, so called in honour of M. Haidinger, is an ore of antimony which is found near Chazelles in Auvergne. The ore was rejected, because it yields metal of so bad quality that manufacturers could not use it. A portion of the ore was sent to M. Berthier, who gives the following account of it. Haidingerite has not yet been met with in regular forms, but some cavities exhibit rudiments of prismatic crystals, having the appearance of those of common sulphuret of antimony: this new mineral occurs generally in confused laminated masses, mixed with quartz, white ferruginous carbonate of lime, and cubic pyrites. Its colour is iron gray, and its surface is frequently covered with iridescent tints. It has generally less splendour than sulphuret of antimony, without any shade of blue. It does not obey the magnet. It has not been procured in pieces sufficiently pure to admit of its specific gravity being ascertained. It readily melts before the blowpipe, and is quickly acted upon by cold muriatic acid, and gives out pure sulphuretted hydrogen gas, and is totally dissolved, except some pyrites and quartz; no sulphur is deposited.

By analysis;—taken the mean of several experiments, it yielded

Sulphur	28·3
Antimony	48·3
Iron	14·9
Zinc	00·3
Sulphuret of Iron	03·2
Quartz	03·2
	98·2

or independently of the gangue, it consists of

Sulphur	30·3	or	Sulphuret of Antimony	71·5
Antimony	52·0		Protosulphuret of Iron	25·5
Iron	16·0		Sulphuret of Zinc	00·5
Zinc	00·3			
				97·5
	98·6			

Ann. de Chim. et de Phys. tom. xxxv. p. 351.

The above affords an excellent illustration of the complex views of foreign chemists with respect to atomic composition. M. Berthier considers it as composed of

18 atoms of Sulphur, or 4 atoms of Sulphuret of Antimony.
4 atoms of Antimony, 3 atoms of Protosulphuret of Iron.
3 atoms of Iron.

Adopting the simpler views generally entertained in this country, the mineral in question is constituted of

3 atoms of Sulphur	or	2 atoms of Sulphuret of Antimony	120
2 atoms of Antimony		1 atom of Protosulphuret of Iron	44
1 atom of Iron			
			164

Edit.

BROMIDE OF SELENIUM:—BY M. SÉRULLAS.

Selenium combines with bromine in several proportions; but five parts of the former and one part of the latter appear to form the most intimate compound. To obtain bromide of selenium, the latter in powder is to be put into a moderately large tube, and the bromine is to be poured upon it. Combination takes place rapidly with a noise similar to that produced by immersing red-hot iron in water; much heat is given out, and the mixture becomes instantaneously solid: the compound has a red brown colour, similar to that of iodide of phosphorus; and some parts are yellowish like chloride of iodine; it gives out vapour when exposed to the air, which has an odour similar to that of chloride of sulphur. It is entirely soluble in water, except some particles of selenium, which precipitate. The filtered solution is colourless when there is no uncombined bromine; it is strongly acid, and consists of selenic and hydrobromic acids. The solution when saturated by potash, gives, by evaporation, a crystallized mixture of seleniate and hydrobromate: on the addition of muriatic acid, either before or after saturation with potash, the selenium is precipitated in particles of a fine red colour.

When a bar of iron or zinc is kept immersed in the solution of bromide of selenium, rapid action occurs, and especially with the zinc; hydrogen gas is abundantly evolved, and the metallic bar is covered with selenium, which adheres in the same manner as one metal precipitated by another; by agitation the selenium is detached, and bubbles of hydrogen gas, which had also been retained, are given out. The selenium being separated by filtration, the hydro-bromate of zinc may be decomposed by carbonate of potash, by which carbonate of zinc is precipitated and hydro-bromate of potash remains in solution. This action of zinc or iron, in the solution of bromide of selenium, is similar to that which occurs when either of these metals is put into a mixture of selenic and muriatic acids; the selenium is deoxidized, and hydrogen evolved by the decomposition of water. Is the whole of the hydrogen evolved, or is part of it employed in reducing the selenic acid? Or is there, as with the metals, a voltaic circuit formed? A slight smell of decayed cabbage is perceptible, which is one of the characteristics of oxide of selenium. When bromide of selenium is strongly heated, a portion of it sublimes, and assumes a yellow appearance, the remainder is decomposed into bromine and selenium.—*Ibid.* p. 349.

ELECTRICITY OF GASES AND THE ATMOSPHERE.

M. Pouillet concludes from numerous experiments: 1st, That gases give out electricity, when they combine together, or when they unite with solid or fluid bodies; and in these combinations, oxygen always gives out positive electricity, and the combustible body, whatever it may be, gives out negative electricity; and reciprocally, when a compound is decomposed, each of the elements then requiring the electricity which it had given out, is in the opposite state of electricity. This reciprocity shows in what consists the difference of the nascent state of a body from its ultimate condition.

2ndly. The action of vegetables upon the oxygen of the air is one of the most permanent and powerful causes of atmospheric electricity; and when it is considered on one hand that a gramme (about 15·5 grains) of charcoal, in becoming carbonic acid, gives out sufficient electricity to charge a Leyden jar; and on the other hand, that the charcoal which is contained in vegetables does not give out less electricity than charcoal which burns freely, one may conclude, says M. Pouillet, as my direct experiments tend to prove, that over a surface of vegetation 100 metres square, more electricity is produced in a day, than is necessary to charge the strongest electrical battery.—*Ibid.* p. 420.

ROSE-COLOURED PETROSILEX FROM SAHLBERGH IN SWEDEN.

M Berthier has analysed this substance. Its characters are, that it is compact, homogeneous; its fracture is very fine-grained, waxy; its colour is deep flesh red; it is very translucent, and capable of receiving a fine polish. By the blowpipe it melts into a white enamel; but it is much less fusible than felspar. When it is calcined in a strong white heat, it does not soften, nor does it alter in colour or appearance. When strongly heated in a charcoal crucible in a porcelain furnace, its angles are rounded, it becomes milk-white, and resembles chalcedony. It loses only about 1-200dth of its weight in this high temperature—a loss which is to be attributed to hygrometric moisture. It yielded by analysis

Silica	79·5
Alumina	12·2
Soda	06·0
Magnesia	01·1
Oxide of iron	00·5
	99·3

Ibid. t. xxxvi. p. 22.

NONTRONITE,—A NEW MINERAL.

This mineral occurs in the arrondissement of Nontron, in the northern part of the department de la Dordogne, which contains a mine of manganese of considerable importance, near the village of Saint Pardoux, in heaps of which this mineral was found. This substance is disseminated throughout the ore in kidney-shaped masses, which are usually very small, and are rarely as large as the fist. This mineral is compact, of a straw or canary yellow colour, with a shade of green; its fracture is uneven: it is opake, unctuous to the touch, and very tender; its consistence is that of clay, and it is easily scratched with the nail; it takes a fine polish, and assumes a resinous lustre by being rubbed with very soft bodies. It flattens and becomes lumpy under the pestle, and is not reduced to powder; it has not the argillaceous smell, nor any action upon the magnet. When immersed in water it immediately disengages numerous bubbles of air; it becomes translucent on the edges without softening or losing its form, as clays do; and if after some hours it is taken from the water and weighed, after being well wiped, it is found to have gained about 1-10th of its weight of water. When heated in a glass tube it loses water at a low temperature, and becomes of a dirty red colour. When strongly calcined in a crucible, it assumes a similar appearance, and its weight is diminished 19 to 21 per cent. After calcination it is sensibly magnetic.

Muriatic acid acts very readily upon it; the solution does not contain the smallest trace either of manganese, protoxide of iron, or of alkali: it contains peroxide of iron, alumina, and magnesia; the insoluble portion is gelatinous, and is composed of silica, soluble in the alkaline solutions, and sometimes mixed with a small quantity of clay, when the mineral has not been picked with sufficient care.

By analysis this substance yielded

Silica	44·
Peroxide of Iron	29·
Alumina	3·6
Magnesia	2·1
Water	18·7
Clay	1·2
	98·6

Ibid. p. 22.

ANALYSIS OF SOME VEGETABLE PRODUCTS.

M. F. Marcet has analysed some vegetable bodies: the method employed was that proposed by M. Gay-Lussac of using peroxide of copper. Common starch yielded

Carbon	43·7
Oxygen	49·7
Hydrogen	6·6
	100·

Roasted starch gave

Carbon	35·7
Oxygen	58·1
Hydrogen	6·2
	100·

It appears therefore that roasted starch contains much more oxygen and less carbon than common starch. *Hordein* is the name given by Proust to a substance separated from barley flour. It yielded

Carbon	44·2
Oxygen	47·6
Hydrogen	6·4
Azote	1·8
	100·

M. Marcet remarks that some chemists have considered this substance as a modification of starch, and others as analogous to sawdust. Dr. Thomson appears to regard it as of the same nature as the parenchyma of the potato: this latter substance gave M. Marcet

Carbon	37·4
Oxygen	58·6
Hydrogen	4·
	100·

It appears therefore that hordein differs very materially from this substance as well as from saw-dust, of which the analysis is thus given by MM. Gay-Lussac and Thenard:

Carbon	52·
Oxygen	42·4
Hydrogen	5·6
	100·

M. Marcet therefore considers hordein a peculiar substance, but most resembling starch. The azote which it contains may, he thinks, be derived from the presence of gluten.

The following are the results of M. Marcet's analysis of gluten and yeast:

	Gluten.	Yeast.
Carbon	55·7	30·5
Oxygen	22·0	57·4
Hydrogen	7·8	4·5
Azote	14·5	7·6
	100·	100·

Ibid. p. 27.

NEW CHLORIDE OF MANGANESE.

M. Dumas obtained this compound by putting a solution of manganesic acid into contact with sulphuric acid and fused common salt. Water and the new chloride are formed; the former is retained by the acid, the latter volatilizes in a gaseous form, with a greenish tint, and when passed into a tube cooled to 5° or 4° of Fahrenheit, it condenses into a liquid of a brownish-green colour. The most simple process appears to be to form a common green chameleon, to convert it into red chameleon by sulphuric acid, and to evaporate the solution, which will give a residue consisting of sulphate and manganesate of potash. This mixture acted upon by concentrated sulphuric acid produces the solution of manganesic acid, into which the common salt is to be thrown in small pieces until the vapours which rise are colourless; the latter effect is a sign that all the manganesic acid is decomposed, and that muriatic acid only is produced. This chloride of manganese corresponds in proportions to the manganesic acid; it is readily formed and examined, but not easily preserved. An analogous compound is obtained when a fluoride is used instead of the chloride, but a sufficient quantity for examination has not yet been procurable.—*Ibid.* xxxvi. 81.

ON THE POWER OF WATER AND BROMINE IN CONDUCTING ELECTRICITY.

M. de la Rive found, as had been previously ascertained by M. Balard, that pure dry bromine did not conduct the electricity of a voltaic battery, consisting of sixty pairs of plates very strongly charged, a delicate galvanometer being the test. A similar experiment was then made with pure water contained in a glass capsule, and communicated with the battery and galvanometer by platina wires; and the deviation of the needle was scarcely sensible. A few drops of bromine were then added to the water, which soon imparted a yellow colour to the water; being now included in the voltaic circuit, the galvanometer needle was deviated 70°, and an abundant disengagement of gas took place from the platina wires. There were oxygen and hydrogen in the usual proportions, showing that water only had been decomposed. From these experiments it results, that a body which conducts voltaic electricity very imperfectly, namely, pure water, may be rendered a good conductor by holding in solution a very minute quantity of a perfectly non-conducting substance, namely, bromine: the same fact occurs with iodine, and iodine and water.

Ibid. xxxv. p. 161.

EFFECTS OF HEAT UPON SULPHUR.

According to M. Dumas fused sulphur begins to crystallize between 226° and 228°; between 230° and 284° it is as liquid as a clear varnish, and of an amber colour; at about 320° it begins to thicken, and acquire a red colour; on increasing the heat, it becomes so thick, that it will not pour. This effect is most marked between 428° and 572°; the colour is then a red brown: from 572° to the boiling point it becomes thinner, but never so fluid as at 248°; the deep red brown colour continues till it boils. When the most fluid sulphur is suddenly cooled, it becomes brittle; but the thickened sulphur similarly treated, remains soft, and more soft as the temperature has been higher. Thus at 230° the sulphur was very liquid and yellow; and cooled suddenly by immersion in water, it became yellow and very friable; at 374° it was thick, and of an orange colour; but by cooling became at first soft and transparent, but soon friable and of the ordinary appearance: at 428° it was red and viscid; and when cooled, soft, transparent, and of an amber colour; at the boiling it was deep brown red colour; and when cooled very soft, transparent, and of a red brown colour. It is not necessary, as is sometimes stated, to heat the sulphur a long time to produce this effect; all depends upon temperature. The only precaution necessary is, to have abundance of water, and divide the sulphur into small drops or portions, that the cooling may be rapid. If it be poured in a mass, the interior cools slowly, and acquires the ordinary hard state. When the experiment is well made at 446°, the sulphur may be drawn into threads as fine as a hair, and many feet in length.

Ibid. xxxvi. p. 83.

PEROXIDE OF BARIUM.

M. Quesneville recommends the following process for preparing this compound.—Put nitrate of barytes into a coated earthenware retort, to which a tube is to be attached, for the purpose of conveying the liberated gases to a water-trough. The heat is to be continued until pure oxygen gas comes over, and the operation is then to be stopped. The peroxide of barium thus obtained falls to pieces in water without producing heat; when boiled in water, oxygen gas is evolved,

ved, and by a strong heat it is reduced to protoxide. Sulphuric acid evolves no nitric acid, nor does nitric acid produce nitric oxide when mixed with it. The protoxide of barium evidently becomes peroxide by combining with the nascent oxygen of the decomposed nitric acid. *Ibid* xxxvi. p. 108.

ON CERTAIN HABITUDES OF SEPIÆ.

Portsmouth, Aug. 15, 1827.

GENTLEMEN,—A fisherman this morning brought me a large bunch of the eggs of the Sepia, or common cuttle-fish, fastened to a sea weed. The young were alive in the fluid contained in the membranaceous coverings, and their motions could be readily seen by removing the outer or dark membrane. As I wished to preserve them for a specimen to be added to a long series of bottles containing the natural history, anatomy, &c. of this fish, which I have presented to the museum of the Portsmouth Institution (the eggs being larger than any I had before obtained), I laid the specimen on a mahogany table while I prepared some spirit. On returning in a few minutes, I found the young Sepiæ had (with one exception) burst the membranes near the stem-like process, and were swimming about in the fluid which escaped during the time of my removing them by means of a broad spatula into a glass of salt water. I was surprised by the amazing strength with which these minute fish (not bigger than a large pea) ejected a current of water, followed by the dark-coloured fluid peculiar to them, through an extent of at least half a yard, and with such force as to be quite unpleasant to the face. I have deemed it worth while to notice these facts, as it is uncommon to obtain the eggs at the state of perfection, even here on the sea coast. The little creatures lived in my window for several hours, exhibiting all the voracious propensities of the full-grown ones. I am, &c.,

HENRY SLIGHT, Surgeon,
Librarian to the Portsmouth Institution.

NOTICE OF AN ERROR IN GALBRAITH'S MATHEMATICAL TABLES AND FORMULÆ.

In the practical problems, at page 167, for computing the deflection of beams, fixed at one end and loaded at the other with any given weight, an erroneous rule is copied from Mr. Barlow's Treatise on the Strength of Timber. When errors of this nature are circulated in books of such general practical utility, it becomes an important although an ungracious duty, to point them out.

The rule should be—

Multiply the tabular value of E by the breadth, and cube of the depth, both in inches.

Multiply also the cube of the length in inches, by the given weight, and that product again by 16.

Divide the latter product by the former for the deflection sought.

N.B. The tabular value of E is four times the weight in pounds of the modulus of elasticity. B. BEVAN.

ERRORS IN HUTTON'S LOG. TABLES (FIFTH EDITION).

For Log. Tang. of 0° 44′ 13″—$8\cdot1\overset{*}{1}93361$ read $8\cdot1\overset{*}{0}9$ &c.
14 —8·1194998 read 8·109 &c.
15 —8·1196634 read 8·109 &c.
16 —8·1198269 read 8·109 &c.
17 —8·1199904 read 8·109 &c.

J. NIXON.

CRYSTALLIZATION OF PHOSPHORUS.

By the fusion and careful refrigeration of a large quantity of phosphorus, M. Frantween has obtained very fine crystals of an octahedral form, and as large as a cherry-stone.

CHEMICAL EXAMINATION ON ANCIENT INSTRUMENTS, &c.

M. Vauquelin has analysed a poignard blade, formed entirely of copper; a mirror found to consist of 85 parts of copper, 14 tin, and 1 of iron per cent; and a blue colour found in a tomb, gave by analysis, silica 70, lime 9, oxide of copper 15, oxide of iron 1, soda and potash 4. A blue colour similar to this both in tint and composition, was found in the bottom of a furnace in which copper had been fused at Romilly.—*Bull. Univ.* A. vii. 264.

LONDON INSTITUTION.

Encouraged by the success of the Royal Institution, the managers of the London Institution propose, during the present season, to try an experiment which they trust will prove acceptable to the proprietors.

The library and theatre of the London Institution will be prepared during eight evenings for a soirée or converzatione, to which proprietors and their friends will be admitted under certain regulations. Coffee and tea will be supplied in the library, where it is intended to introduce all new scientific inventions or discoveries, that can be exhibited by models or drawings. New literary works, original drawings, and scarce books, will constitute another source of interest and information. This plan will be attended with the advantage of introducing learned foreigners and men of science to a complete view of the establishment, and to a social conversation with its managers and proprietors. At some period during the evening an adjournment will take place to the Theatre, where it is intended to give a short discourse or lecture, either honorary or professional, on any new machinery, or on any chemical, philosophical, or literary novelty that may be considered by the managers to deserve the attention of the Institution.

The following is a notice of the series of lectures for the season, to be delivered in the Theatre of this Institution:—

Course I.—On the motive forces of the arts: illustrated by models of machines, steam-engines, &c.; by Norton Webster, Esq. To commence on Monday, the 4th of February, at seven o'clock in the evening.

Course II.—On the phænomena and history of igneous meteors and meteorites: illustrated by a series of transparent paintings of meteors, and an extensive collection of meteorites; by E. W. Brayley, jun. Esq. A.L.S. To commence on Saturday, the 9th of February, at one o'clock in the afternoon.

Course III.—On music, particularly as to vocal music (illustrated by voices), by Samuel Wesley, Esq. To commence on Tuesday, the 4th of March, at one o'clock in the afternoon.

Course IV.—On poetry and the drama in general, and on Milton and Shakspeare in particular, by John Thelwall, Esq. To commence on Thursday, the 6th of March, at seven o'clock in the evening.

LIST OF NEW PATENTS.

To R. W. Winfield, of Birmingham, for his improvements in tubes or rods produced by a new method of manufacturing and in the construction only, and for manufacturing the same, with various other improvements, into parts of bedsteads and other articles.—Dated the 4th of December 1827.—6 months allowed to enrol specifications.

To J. Meadon, of Millbrook near Southampton, for improvements on wheels for carriages.—4th of December.—6 months.

To S. Wilkinson, of Holbeck, Yorkshire, for improvements in mangles.—4th of December.—6 months.

To Maurice de Jough, of Warrington, cotton-spinner, for improvements in machines adapted for spinning, doubling, twisting, roving, or preparing cotton, &c.—4th of December.—6 months.

To T. Tyndall, of Birmingham, for improvements in the manufacture of buttons, and in the machinery for manufacturing the same: communicated from abroad.—4th of December.—6 months.

To D. Ledsam and W. Jones, of Birmingham, for improvements in machinery for cutting sprigs, brads, and nails.—4th of December.—6 months.

To J. Robinson, of Merchants-row, Limehouse, for an improvement in the manufacture of brushes of certain descriptions, and in the manufacture of a material and the application thereof to the manufacture of brushes and other purposes.—4th of December.—6 months.

To Paul Steenstrup, of Basing-lane, London, esquire, for improvements in machinery for propelling vessels, and other purposes. —11th of December.—6 months.

To J. H. Sadler, of Hoxton, Middlesex, for improvements in power-looms.—13th of December.—6 months.

To R. Rewcastle, of Newcastle-upon-Tyne, for an improved method of ballasting ships or vessels.—13th of December.—6 months.

To R. Stein, of Regent-street, Oxford-street, for an improvement in applying heat to the purpose of distillation.—13th of December.—6 months.

To F. B. Geithner, of Birmingham, for improvements on castors for furniture, &c.—13th of December—6 months.

To H. Peto, of Little Britain, for an apparatus for generating power. —13th of December.—6 months.

To J. A. Berrollas, of Nelson-street, City-road, for a method of winding up a pocket watch, or clock, without a key, which he calls "Berrollas's keyless watch or clock;" and also a certain improvement to be applied to his late invented detached alarum watch.—13th of December.—2 months.

To Lieut. A. M. Skene, of Jermyn-street, for an improvement in propelling vessels, and for working under-shot water-mills.—13th of December.—6 months.

To J: L. Stevens, of Plymouth, for a new method of propelling vessels by the aid of steam or other means, and for its application to other purposes.—18th of December.—6 months.

To T. Tyndall, of Birmingham, for improvements in the machinery for making nails, brads, and screws: communicated from abroad.—18th of December.—6 months.

To J. George, of Chancery-lane, esquire, barrister-at-law, for his invention for preserving decked ships or vessels, so as to render them less liable to dry-rot, and for preserving goods on board such ships and vessels from damage by heat.—18th of December.—6 months.

To T. S. Holland, of the city of London, esquire, for combinations of machinery fot generating and communicating power and motion, applicable to propelling of fixed machinery, as also floating bodies, carriages, and other locomotive machines.—19th of December.—6 months.

To W. Harland, M.D., of Scarborough, for improvements in apparatus for propelling locomotive carriages, which improvements are also applicable to other useful purposes.—21st of December.—6 months.

To C. A. Furguston, of Mill Wall, in the parish of All Saints, Poplar, mast-maker, and J. Falconer Atlee, of Prospect-place, Deptford, for their improvements in the construction of made masts.—22nd of December.—6 months.

To W. Hale, of Colchester, for his improvements in machinery for propelling vessels.—27th of December.—6 months.

To W. Gossage, of Leamington Priors, Warwickshire, for improvements in the construction of cocks for the passage of fluids.—2nd of January, 1828.—6 months.

To T. Botfield, of Hopton Court, Salop, for improvements in making iron, or in the method or methods of smelting and making of iron.—2nd of January.—4 months.

To J. Hall, jun., of Ordsall near Manchester, for improvements in dyeing piece-goods by machinery.—2nd of January.—2 months.

To J. Cl. Daniell, of Stoke, Wilts, for improvements in dressing cloths, and in the machinery applicable for that purpose.—2nd of January.—6 months.

To W. Morley, of Nottingham, for improvements in and additions to machinery now in use for making lace or net.—9th of January.—6 months.

To J. A. Hunt Grubbe, of Stanton Saint Bernard, Wilts, clerk, for a transmitting heat wall for the ripening of fruit.—9th of January.—6 months.

To J. Gilbertson, of Hertford, for an improvement in the construction

tion of furnaces, by which they consume their own smoke.—15th of January.—2 months.

To C. Hooper, of Spring Gardens, in the parish of Marston Bigott, Somersetshire, for an improved machine for shearing and cropping woollen and other cloths.—15th of January.—2 months.

To J. Evans the younger, of Moreton Mills, near Wallingford, Berks, for improvements on steam engines.—15th of January.—6 mon.

To J. Blades, of Clapham, Surrey, for an improvement in the waterproof stiffening for hats: communicated from abroad.—15th of January.—6 months.

To W. Newton, of Chancery-lane, for an improved surgical chair-bed with various appendages.—15th of January.—6 months.

To G. D. Harris, of Field-place, near Stroud, Gloucestershire, for improvements in dressing and preparing woollen yarns, and in cleansing, dressing, and finishing woollen cloths, &c. and in the apparatus for performing the same.—15th of January.—6 months.

To J. Falconer Atlee, of Prospect-place, Deptford, for improvements on bands or hoops for securing made and other masts, bowsprits and yards, and applicable to other purposes.—15th of January.—6 months.

To W. Erskine Cochrane, esquire, of Regent-street, for improvements in certain apparatus for cooling, and other purposes.—15th of January.—6 months.

To J. Taylor Beale, of Church-lane, Whitechapel, and G. Richardson Porter, of Old Broad-street, for their new mode of communicating heat for various purposes.—19th of January.—6 months.

To W. Percivall, of Knightsbridge, for improvements in the construction and application of shoes without nails to the feet of horses and certain other animals.—19th of January.—6 months.

To G. Jackson, of Saint Andrew, Dublin, for improvements in machinery for propelling boats and other vessels, which improvements are also applicable to water-wheels and other purposes.—19th of January.—6 months.

METEOROLOGICAL OBSERVATIONS FOR DECEMBER 1827.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·58 Dec. 28. Wind NE.—Min. 29·07. Dec. 1. Wind W.
Range of the mercury 1·51.

Mean barometrical pressure for the month	29·810
——— for the lunar period ending the 18th instant	29·763
——— for 15 days with the moon in North declination . . .	29·693
——— for 15 days with the moon in South declination	29·834
Spaces described by the rising and falling of the mercury . . .	8·900

Greatest variation in 24 hours 0·560.—Number of changes 26.
Therm. Max. 57° Dec. 5. Wind W.—Min. 31° Dec. 29. Wind N.
Range 26°.—Mean temp. of exter. air 47°·82. For 30 days with ☉ in ♐ 47·50
Max. var. in 24 hours 17°·00—Mean temp. of spring water at 8 A.M. 53°·87

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air on nine different days	100°
Greatest dryness of the air in the afternoon of the 6th	55
Range of the index	45

Mean at 2 P.M. 78°·2—Mean at 8 A.M. 83°·0—Mean at 8 P.M. 85·5
—— of three observations each day at 8, 2, and 8 o'clock . . . 82·2.
Evaporation for the month 0·60 inch.
Rain near ground 5·625 inch.—Rain 23 feet high 5·115 inch.
Prevailing Wind S.W.

Summary of the Weather.

A clear sky, 3; fine, with various modifications of clouds, 9; an overcast sky without rain, 7½; foggy 2; rain, 9½.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
15	8	28	0	14	12	23

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
1½	½	1	1	4	10½	8	4½	31

General Observations.—The weather to the 24th inclusive was remarkably mild for the season, but windy and wet, five inches and a quarter of rain having fallen; and on several days before the winter solstice it was remarkably dark, from there being no opening in the prevailing strata of clouds to admit the passage of the solar rays.

Early in the morning of the 12th there was a thick fog, which gave out a very strong sulphurous smell: the 29th was also distinguished by a cold dense fog throughout the day and night.

The last spring tides were high in Portsmouth harbour, and along the southern coast, also at Liverpool, and along the western coast, in consequence of so great a fall of rain, with accompanying south and south-west gales. The highest spring tide was a few minutes before 12 P.M. on the 18th, although it was the first after the change of the moon. The rain on the 18th, 19th, and 20th continued fifty hours without intermission.

On the 21st and 22nd, thunder storms, with lightning and hail, were experienced in several places in Hampshire.

On the 25th a parhelion appeared at mid-day on the western side of the sun, when the *cirrocumulus* and attenuated *cirrostratus* clouds were beautifully tinged with faint prismatic colours thirty degrees distant from the sun's centre.

The *maximum* temperature of the external air occurred in the nights of the 3rd, 7th, 9th, 13th, 15th, 17th, 21st and 23rd, instead of in the days. These frequent occurrences rendered the circumstance remarkable and deserving an explanation. It seems to be influenced by the sun's position in the zodiac at this time of the year; the temperature of the ground at and near the surface; and the inosculation of the sea air with that over the land in the evening, just before the coming on of rain from the western side of our meridian. 1st. The sun's meridional altitude in this latitude in December varies from 16 to 17¾ degrees, and from so great an obliquity of his rays, or so small an angle, that from this altitude they make with the horizon, only three or four degrees of thermometrical heat are produced near the earth's surface, more than the temperature of the ground. 2ndly. The temperature of the atmosphere in a wet December like the present, depends very much on the temperature of the ground, and the quality of the prevailing vapours, which, if humid and accompanied with mild currents of air, then the temperatures of the ground and air are found to coincide nearly. 3rdly. If under these circumstances of temperature and condensing state of the lower medium of the atmosphere, the sea air should unite with the land air on the approach of rain after sunset, on any day in December or January on which the temperature of the air has been rather low, there is a great probability, from the sea air being warmer than the land air, that the *maximum* temperature will happen in the night instead of in the day.

Long solar lights and shades were very conspicuous half an hour before sunset on the 30th, and moved round the sun, in a westerly direction, like the spokes of a large wheel in slow motion. This singular appearance was no doubt caused by a brisk wind blowing against the eastern side of a hemispherical *cumulus* cloud (on whose dense surface the sun's horizontal rays had previously impinged and were reflected) so as to give it a revolving motion.

These reflected rays repeatedly varied in length, perhaps from the uneven surface of the cloud. A short time before they disappeared, they were nearly equidistant, their angular distance from each other in at least a semicircle, was about twelve degrees; and each of the horizontal rays reflected to a distance of nearly sixty degrees. The sun being near the horizon, the lower rays could scarcely be traced: but the phænomenon certainly exhibited a grand and gratifying appearance.

The mean temperature of the external air this month, is 5¼ degrees higher than the mean of December for the last twelve years, and is also for that period unprecedented.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are one parhelion, one large lunar halo in the night of the 26th, which set with the moon; four meteors, one rainbow; lightning and thunder in the night of the 20th; and fourteen gales of wind, or days on which they have prevailed; namely, one from the East, three from the South, seven from South-west, and three from the West.

REMARKS.

London.—Dec. 1. Cloudy. 2. Drizzly. 3. Fine. 4. Cloudy: drizzly. 5. Cloudy. 6. Fine. 7. Stormy night. 8, 9. Fine. 10. Rain: drizzly. 11. Foggy morning: rainy night. 12. Cloudy: rain at night. 13. Cloudy: foggy morning. 14. Rainy. 15. Cloudy: rainy night. 16. Fine morning: heavy shower at 3 P.M. 17. Cloudy. 18. Rain: boisterous night. 19. Rainy. 20. Fine. 21, 22. Cloudy. 23. Fine. 24. Rain. 25—27. Fine. 28. Fine day: foggy night. 29. Gloomy: dense fog all day. 30. Fine. 31. Fine: rain at night.

Penzance.—Dec. 1, 2. Showers. 3. Fair. 4, 5. Misty. 6. Clear. 7. Clear, fair: rain at night. 8. Clear. 9. Misty: rain. 10. Rain. 11, 12. Rain: fair. 13. Clear. 14. Rain: fair. 15. Rain. 16. Clear: hail-showers. 17. Fair: rain. 18, 19. Rain. 20. Hail-showers. 21, 22. Rain. 23. Fine: rain. 24. Rain: fair. 25. Clear. 26. Fair. 27. Clear. 28. Fair. 29—31. Clear.—Rain-gauge ground level.

Boston.—Dec. 1. Cloudy: rain early A.M. 2. Rain. 3. Cloudy. 4. Cloudy: rain early A.M. 5. Fine. 6. Stormy. 7—9. Fine. 10. Cloudy. 11. Fine: rain P.M. 12. Misty: rain at night. 13. Cloudy: rain early A.M. 14. Rain: rain early A.M. 15—17. Fine. 18. Cloudy: rain P.M. 19. Cloudy. 20—21. Fine. 22. Fine: rain P.M. 23. Fine. 24. Cloudy. 25. Fine: rain A.M. 26. Cloudy. 27—29. Fine. 30. Cloudy. 31. Fine.

RESULTS.

Winds, NE. 1: SE. 4: SW. 14: W. 3: NW. 9.

London.—Barometer: Mean height for the month .... 30·038 inch.
Thermometer: Mean height for the month .... 44·274
Rain .... 3·60
Evaporation .... 1·24 inch.

Meteoro-

Meteorological Observations by Mr. HOWARD near London, Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1827.	Barometer.							Thermometer.							Wind.				Evapor.		Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost. 8½ A.M.	Lond.	Penz.	Gosp.	Bost.	Lond.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.											
Dec. 1	29·31	29·21	29·30	29·26	29·14	29·07	28 54	54	42	52	50	56	45	46	W.	NW.	W.	S.	...	...	...	...	...	0·24
2	29·91	29·31	29·50	29·34	29·26	29·16	28·70	45	42	50	46	50	46	44	NW.	NW.	NW.	calm	...	...	·15	0·520	...	·11
○ 3	29·96	29·91	29·72	29·70	29·80	29·62	29·28	60	40	53	46	52	46	44·5	NE.	NW.	N.	calm	...	0·05	·09	...	0·090	·10
4	30·15	29·96	29·88	29·84	29·92	29·80	29·26	54	49	54	50	55	50	53	W.	W.	W.	W.	...	...	·03	...	...	·10
5	30·15	30·10	30·00	30·00	29·98	29·92	29·40	49	47	55	50	57	47	53·5	W.	W.	W.	W.	...	...	·03	...	·030	...
6	30·44	30·15	30·10	30·00	30·14	29·83	29·30	48	31	49	46	50	35	45·5	NW.	NW.	NW.	NW.	...	·10	...	...	...	...
7	30·44	30·05	30·08	29·86	30·23	30·00	29·77	54	34	49	42	52	48	38	SW.	W.	SW.	NW.	...	...	·03	...	·260	...
8	30·13	30·05	29·86	29·84	29·95	29·82	29·84	48	28	52	46	51	37	43	NW.	N.	NW.	NW.	...	...	...	...	...	...
9	30·13	29·81	29·70	29·60	29·86	29·67	29·55	54	32	54	42	54	52	35	SE.	SW.	S.	calm	...	·05	...	...	·060	...
10	29·81	29·68	29·36	29·26	29·60	29·36	29·15	55	44	55	50	56	46	52·5	SW.	SW.	SW.	SW.	...	...	·06	...	·070	...
☾ 11	29·68	29·63	29·26	29·26	29·46	29·37	28·96	52	35	50	47	54	42	44	SW.	W.	S.	W.	·48	...	·83	1·645	·830	...
12	29·72	29·63	29·30	29·26	29·33	29·30	29·06	49	37	48	42	51	39	42·5	SW.	W.	E.	calm	...	·10	·15	...	·120	·14
13	29·72	29·68	29·56	29·52	29·64	29·50	29·00	47	37	54	40	45	41	42·5	NW.	NW.	NW.	calm	...	...	·02	...	·180	·31
14	29·86	29·68	29·40	29·30	29·54	29·32	28·95	51	42	54	42	53	45	45	SE.	W.	S.	NW.	...	...	·40	...	·770	·12
15	29·88	29·86	29·56	29·54	29·66	29·58	29·26	54	47	54	45	55	45	43	NW.	SW.	SW.	NW.	...	·05	·30	0·840	·140	·35
16	30·39	29·88	29·90	29·70	29·94	29·66	29·16	47	34	50	44	51	39	44	SW.	W.	W.	NW.	...	...	·02	...	...	...
17	30·39	30·00	30·06	30 06	30·17	30·03	29·66	52	38	52	44	52	47	38	SW.	SW.	SW.	NW.	...	...	·08	...	·370	...
● 18	30·00	29·80	29·74	29·66	29·82	29·70	29 33	54	51	53	49	55	52	51	SW.	SW.	SW.	NW.	...	...	·33	...	·475	...
19	29·80	29·75	29·46	29·42	29·61	29·57	29·05	54	50	52	49	55	46	54·5	SW	SW.	SW.	SW.	...	·05	·34	1·785	·750	·16
20	29·87	29·75	29·52	29·42	29·56	29·50	29·00	49	38	50	43	50	42	45	SW	W.	SW.	W.	...	...	...	0·500	·120	...
21	29·75	29·60	29·62	29·44	29·68	29·36	29·22	48	37	50	42	53	48	37	SW.	SW.	SW.	W.	...	...	·07	...	·180	...
22	30·09	29·60	29·58	29·52	29·50	29·42	28·90	54	41	50	44	54	43	44	SW.	W.	W.	W.	...	·05	·38	1·105	·370	...
23	30·09	30·01	29·90	29·80	30·03	29·83	29·42	52	38	55	44	54	46	42	NW.	S.W.	NW.	W.	...	...	·06	...	·330	·09
☽ 24	30·52	30·01	30·06	29·74	29·94	29·77	29·34	49	38	55	47	56	40	51	SW.	W.	SW.	N.	...	...	·10	0·500	·110	...
25	30·62	30·52	30·24	30·20	30·36	30·30	29·85	48	42	51	44	51	46	43	NW.	W.	W.	W.	·46	.05	...	0·025	...	·17
26	30·62	30·58	30·30	30·30	30·42	30·40	30·00	51	41	52	48	53	44	47	SW.	W.	SW.	calm	...	...	...	...	...	·09
27	30·81	30·58	30·34	30·30	30·46	30·38	30·00	50	31	52	48	53	42	44	SW.	W.	SW.	calm	...	...	...	...	...	...
28	30·81	30·71	30·40	30·38	30·58	30·57	30·33	40	31	50	48	47	35	33	NW.	E.	NE.	calm	...	·05	...	...	...	...
29	30·71	30·58	30 36	30·34	30·47	30·40	30·15	38	30	50	46	43	31	30	NW.	SE.	NW.	calm	...	...	...	...	·030	...
30	30·58	30·14	30·12	30·06	30·27	30·16	30·04	40	31	50	45	45	40	30·5	SE.	SE.	SE.	calm	...	...	...	...	...	...
31	30·14	29·75	29·66	29·64	29·87	29·66	29·56	41	34	50	47	51	46	37	SE	S.	S.	NW.	·30	·05	·13	0·660	·340	...
Aver. :	30·81	29·21	30·40	29·26	30·58	29·07	29·38	60	28	55	40	57	31	43·3					1·24	0·60	3·60	7·580	5·625	1·98

THE

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AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

MARCH 1828.

XXVI. *On the Regular or Platonic Solids.* *By* DAVIES GILBERT, *F.R.S.* *&c.*

To Mr. Richard Taylor.

Sir,

THE following trifle, extracted from my memorandum-book of many years standing, may perhaps (as a mere curiosity) be favoured with a place in your Journal. It exhibits an easy method of deriving these bodies from a modern discovery in spherics; and it illustrates the extreme generalization attached to algebraic expressions.

Montucla and other writers on mathematics, attribute to Albert Girard, about the year 1629, the discovery of this very curious property of the sphere:—That if the whole surface of the sphere, or what may be considered as its subtense from the centre, be represented by eight right angles; then will the subtense from the centre of any figure, on the surface of the sphere bounded by arches of great circles, equal the excess of the spherical angles above the plane angles formed by the chords of the sides. A discovery applied in recent times with great advantage to all geodetical operations conducted on an extensive scale.

Now as three planes at the least must meet at each solid angle, or point of every solid; it is obvious that no regular equal and similar planes of any figures exceeding the pentagon in number of sides, can possibly meet in one point or solid angle. Thus the angle between two sides of an hexagon being $1\frac{1}{3}$ right angle, three of these would make up 4 right angles, and consequently the sides would expand themselves into one plane. The same would take place with four squares and with six equilateral triangles.

The solid angles or points of all solids bounded by regular equal and similar sides, must then consist either of three, four, or five equilateral triangles. Of three squares or tetragons, or of three pentagons. And consequently the number of these finite bodies cannot exceed five.

That five such actually exist, is thus proved.

When three equilateral triangles meet in a point, the three equal spherical angles must, to complete space, amount each to one-third of four right angles, or to 120°. The three angles therefore of each spherical triangle will equal four right angles, or be two right angles above the two right angles of a plane triangle. The solid angle therefore at the centre of each spherical triangle will subtend one quarter part of the surface of the sphere, and the solid must be a tetraëdron.

If four equilateral triangles meet at a point, the angle of each spherical triangle must, for the same reason, be in this case a right one. Consequently, the three angles of each spherical triangle will equal three right angles, or be one right angle in excess of the three angles of a plane triangle. The solid angle therefore, at the centre of each spherical triangle, will subtend one-eighth part of the surface of the sphere, and the solid must be an octaëdron.

If five equilateral triangles meet in a point, the angle of each spherical triangle must be one-fifth of four right angles, or 72°; that is, four-fifths of one right angle: the three angles of each spherical triangle will amount, therefore, to two right angles and two-fifths of a right angle, exceeding the two right angles of a plane triangle by two-fifths of a right angle, or 36°, being the twentieth part of eight right angles; the solid angle therefore at the centre of each spherical triangle is one-twentieth part of the spherical surface, and the solid must be the icosaëdron.

If three squares or tetragons meet in a point, each spherical angle must be $1\frac{1}{3}$ right angle, and the four spherical angles of the tetragon will amount to $5\frac{1}{3}$ right angles, exceeding $(2n-4)$ right angles, or the four right angles of a plane tetragon by $1\frac{1}{3}$ right angle, or by one-sixth of eight right angles: the solid must therefore be the hexaëdron or cube.

If three pentagons meet in a point, each spherical angle must be $1\frac{1}{3}$ right angle, as in the last instance; and the five spherical angles of the pentagon will be $6\frac{2}{3}$ right angles, exceeding the $(2n-4)$ right angles of the plane pentagon by $\frac{2}{3}$ds of a right angle, or by $\frac{1}{12}$th of eight right angles. The solid must therefore be the dodecaëdron.

The same results may be obtained analytically in a manner exhibiting the extensive generality of algebraic language.

Let $x =$ the number of regular similar and equal planes bounding the solid body.
$y =$ the number of sides in each plane.
$z =$ the number of planes, and consequently of spherical angles, meeting at each point.

Then $\frac{8 \text{ right angles}}{x} =$ the solid angle subtended by each side of the solid from the centre of the sphere.

But since all the spherical angles meeting at any point of the solid must in every instance equal four right angles to fill space; each spherical angle will be equal to $\frac{4 \text{ right angles}}{z}$ and all the spherical angles of a side of the solid $= \frac{y}{z} \times 4$ right angles. But the plane angles of each side of the solid $= (2y - 4)$ right angles. Consequently, the excess of the spherical angles above the plane angles $= (4\frac{y}{z} - 2y + 4)$ right angles. And this equals the solid angle subtended by each side of the solid from the centre of the sphere. Consequently, $(4\frac{y}{z} - 2y + 4)$ right angle $= \frac{8 \text{ right angles}}{x} . 8z = 4xy - 2xyz + 4xz$, and $x = \frac{4z}{2y + (2-y) \times z}$.

Now it is obvious, from the nature of the quantities represented by x, y, and z, that they must in all cases be integral and positive. But since $x = \frac{4z}{2y + (2-y) . x}$. The denominator $2y + (2-y)$ must be also affirmative. To ascertain therefore the limit which y cannot exceed without making the expression negative, put $2y + (2-y) . z = 0$. Then $y = 2 + \frac{4}{z}$.

If z were infinite $y = 2$ and it attains its next integral value;
when $z = 6$ .... $y = 3$;
when $z = 5$ .... $y = 3$;
when $z = 4$ .... y may be 4;
when $z = 3$ .... y may be 6;
when $z = 2$ .... y at its maximum infinite: therefore any finite number may be taken for it.

If z less than 2, the highest value of y is negative.

If

If therefore $z =$ any number from infinity to 2.

y may be 2. Indicating that the sphere is divided into any number of parts by great circles (meridians) passing through the same two poles.

$z = 6$ y at its maximum 3, x infinite. Hence the sphere may be covered by an infinite number of equilateral triangles meeting with 6 angles at each point.

$z = 5$, y at its maximum $= 3$ $x = 20$. Hence the sphere may be covered by 20 equilateral triangles meeting 5 in a point.

$z = 4$, y at its maximum $= 4$, x infinite. Hence the sphere may be covered by an infinite of squares or tetragons meeting 4 in a point.

$y = 3$, $x = 8$. Hence the sphere may be covered by 8 equilateral triangles meeting 3 in a point.

$z = 3$, y at its maximum $= 6$, x infinite. Hence the sphere may be covered by an infinite number of hexagons meeting 3 in a point.

$y = 5$, $x = 12$. The spheres may therefore be covered by 12 pentagons meeting 3 in a point.

$y = 4$. $x = 6$. The sphere may therefore be covered by 6 squares or tetragons meeting 3 in a point.

$y = 3$, $x = 4$. The sphere may therefore be covered by 4 equilateral triangles meeting 3 in a point.

$z = 2$ y at its maximum infinite x any number whatever. Appearing to indicate that a sphere being divided by a mathematical plane into two equal parts, may have that plane bounded by any regular polygon, when 2 angles only will meet in a point.

$y = 2$ The plane appears to contract itself into a right line.

The quantities may be arranged in the following manner:

$x = 4 \quad y = 3 \quad z = 3$

$x = 6 \quad y = 4 \quad z = 3$

$x = 8 \quad y = 3 \quad z = 4$

$x = 12 \quad y = 5 \quad z = 3$

$x = 20 \quad y = 3 \quad z = 5$

x infinite $\quad y = 3 \quad z = 6$

$y = 4 \quad z = 4$

$y = 6 \quad z = 3$

x any number whatever, $y = 2$, $z =$ the same number as x

XXVII. *On the Ellipticity of the Earth as deduced from Experiments with the Pendulum.* By J. Ivory, *M.A. F.R.S.*[*]

IN the *Conn. des Tems* 1830, lately published, there is a notice of experiments made with an invariable pendulum, by M. Duperrey, in the course of a voyage round the world. The places of observation are not numerous, and one only, namely, Toulon, is a new station. But, in the present state of this inquiry, no less instruction is to be reaped from the repetition of the experiments at the old stations, than from the extension of our knowledge by new observations. If we could compare the lengths of the pendulum determined by different observers on the same spot, the errors to which such experiments are liable would become known, and we should be able to estimate the degree of precision with which the figure of the earth can be investigated by this mode of experimenting. An attentive examination of all that has been accomplished in this research will clearly prove that the high expectations of accuracy which were at first entertained from it, have not been realized in practice. It is remarkable that the pendulum-experiments within 30° of the equator are very irregular; while in higher latitudes, if we except Drontheim, the results obtained are not chargeable with inconsistency in any great degree. Two causes only can be imagined in order to account for the irregularity near the equator: either the experiments must be erroneous, or gravity must be very unequally distributed in that quarter of the globe[†].

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[*] Communicated by the Author.

[†] Captain Sabine's experiments have lately appeared in this Journal, corrected from an error in estimating the divisions of the level of his repeating circle. The corrections are excessively trifling, so much so that it seemed hardly worth while to call the attention of the public to the circumstance, as the experiments must be considered as remaining *in statu quo.* That very great irregularity prevails in these experiments, and indeed in all those made near the equator, is a fact that all will allow; but the cause has not been ascertained in a satisfactory manner.

The pendulum which beats seconds at Paris, at the temperature of 15° Cent., being represented by unit, M. Duperrey has determined the relative lengths, at the same temperature and at the level of the sea, at the five following stations:

	Latitude.	Relative Length.
Toulon	43° 7′ 20″ N.	0·99950585
Isle of Ascension	7 55 48 S.	0·99729881
Isle of France. .	20 9 23	0·99789022
Port Jackson . .	33 51 40	0·99871430
Falkland Islands	51 31 44	1·00025995

Adopting 39·12929 for the length of the pendulum at Paris in English inches, at the temperature 62° Fahr., the following table shows the lengths of the pendulums at the stations of M. Duperrey, in English measure, at the standard temperature, and likewise the errors according to my formula published in this Journal for October 1826.

	Observed Pendulum.	Computed Pendulum.	Excess of Calculation.
Toulon.	39·10996	39·11071	+·00075
Port Jackson . .	39·07899	39·07637	—·00261
Isle of France . .	39·04674	39·03643	—·01031
Ascension	39·02360	39·01566	—·00796
Falkland Islands	39·13945	39·13940	—·00005

The pendulums in this table at Ascension and the Isle of France coincide very nearly with the lengths previously determined by Captain Sabine and M. de Freycinet. From some cause or other, they are both anomalous, and they are treated as such by M. de Freycinet and M. Duperrey, being left out in computing the ellipticity which they adopt. But the other three pendulums fall within the limits laid down in this Journal for October 1826, and increase the number of instances that come under my formula to 31, out of 40 the total number of experiments that have been made. My formula was originally constructed from 26 experiments; and all the pendulums since determined by M. de Freycinet, Mr. Foster, and M. Duperrey, agree with it within the prescribed limits, except in the three instances of the islands of Mowi, Guam, and the Isle of France, which are greatly irregular and irreconcileable with the other experiments. It is therefore very probable that the mean ellipticity of the earth will ultimately be found to approach very near $\frac{1}{300}$, as deduced from my formula.

At Port Jackson the pendulum determined by M. Duperrey is less than the length found by M. de Freycinet, and approaches nearer to the pendulum at Paramatta, to which it ought to be very nearly equal: and this proves the justness of the remarks I made relative to this point at pp. 351, 352, of this Journal for November 1826.

Comparing the results obtained by M. Duperrey and M. de Freycinet at the same stations and on the same spot, the pendulum of the former observer at the Falkland Islands is longer than that of the latter by ·00233; and, at Port Jackson and the Isle of France, the pendulums of the former are respectively less than those of the latter by ·00145 and ·00095. We may therefore conclude, that such experiments are liable to an error amounting from ·002 to ·003 in the length of the pendulum, or from two to three vibrations in a mean solar day. We are sure that the error may amount to the quantity mentioned; but it may be much greater. At Ascension the difference between the pendulums of Captain Sabine and M. Duperrey is very small.

In deducing the ellipticity of the earth, M. Duperrey combines his own experiments with M. de Freycinet's, exclusively of those made by other observers. The results he obtains vary between the limits $\frac{1}{266}$ and $\frac{1}{290}$. Upon the whole he concludes that the two hemispheres of the earth are similar, and he fixes definitively upon $\frac{1}{288}$, or $\frac{1}{290}$, as the ellipticity common to both. But on examining the calculations of M. Duperrey, it will be found that the ellipticity he adopts, depends entirely on the equatorial pendulum determined by M. de Freycinet at Rawak. If we suppose that the pendulum at Rawak errs in excess, which is very probable, and diminish its length by correcting the possible error, the ellipticities will come out of less quantity in all the combinations in which they were before equal to $\frac{1}{288}$ or $\frac{1}{290}$. Wherefore, although M. Duperrey finds five combinations of his experiments in which the ellipticity is $\frac{1}{288}$ or $\frac{1}{290}$; yet, whether its real value be equal to either of those numbers, or to a less fraction, will depend entirely upon the error that may exist in the pendulum at Rawak. Captain Sabine has deduced the same ellipticity, viz. $\frac{1}{288}$, by many combinations of his own experiments and those made in England and France; and I have already remarked that this uniformity of result is occasioned by the pendulums at the Islands of Ascension and St. Thomas, which enter into all the computations. If these two stations be left out in any, or in all, the combinations, the ellipticity will no longer be the same as before, but a less fraction. The arriving at the same result by a multiplicity of arithmetical operations is in reality a play of calculation caused by one common datum, or several common data; and therefore it does not furnish independent proofs in favour of the number in question. There must, in all probability, be some error in the pendulum at Rawak; and we know that the two pendulums of Captain Sabine are anomalous, involving an irregularity much greater than usually occurs in such experiments; from which considerations we must conclude that the evidence for the ellipticity $\frac{1}{288}$ rests but on slender foundations.

It is reasonable to think that every new set of experiments with the pendulum should be joined to the stock we already possess, in order to add to the number of unexceptionable experiments, and to correct such as are of doubtful authority. By proceeding in this manner our *data* for obtaining an exact knowledge of the figure of the earth and of the distribution of gravity on its surface, would continually increase and improve in accuracy; and, by applying proper methods of calculation, we might hope to bring this great question to a satisfactory solution. But if every new set is to be taken by itself, we may, by making arbitrary combinations of the experiments, be led into great error, and to entertain speculations that have little foundation in nature.

The number of experiments made with the pendulum by good observers at present amounts to 40, contained in the subsequent table; but of these, six, placed last in the table, are decidedly anomalous, and must be separated from the rest. In setting aside these six experiments I do not now proceed on my own opinion; I follow the example of M. de Freycinet and M. Duperrey. There remains 34 experiments which may be compared together; and we have now to inquire, What mode of calculation must be adopted in order to obtain a result entitled to the greatest confidence the case will admit of. On reflection it will appear that we cannot expect to attain what we wish for by the method of the least squares as usually applied, nor by the modification of that method I used in this Journal for October 1826. These more direct methods are useful in bringing out a first approximation; but it seems necessary to correct the approximate elements thus obtained by the methods used with success in so many problems of astronomy. It is on these principles that the following investigation is conducted.

If we put $39 + \delta$ for the pendulum in English inches at any station; λ for the latitude; and $39 + \Delta$ for the equatorial pendulum; we shall have this equation,

$$\Delta + f \sin^2 \lambda = \delta + e, \qquad (1)$$

e being the error of observation, and f a quantity which is the same at all points of the earth's surface. The equation (1) being general, if we add all the like equations at the 34 stations we propose to include in our investigation, we shall get

$$34\Delta + f\Sigma\,(\sin^2\lambda) = \Sigma\,(\delta) + \Sigma\,(e).$$

Now I have found,

$$\Sigma\,(\sin^2\lambda) = 15{\cdot}9316,$$
$$\Sigma\,(\delta) \quad = 3{\cdot}72872;$$

and hence by dividing all the terms by 34, we get,

$$\Delta + {\cdot}4686 f = {\cdot}10966, \qquad (2)$$

neglecting the term containing c, which must be very small, both because the errors must in some degree compensate one another, and on account of the great divisor 34. The equation (2) must be very exact, provided we take care not to enhance its error by introducing small divisors.

Again, when the ellipticity is $\frac{1}{288}$, we have $f = {\cdot}202$ nearly, as appears from the calculations of Captain Sabine; and, when the ellipticity is $\frac{1}{300}$, f is nearly ·208, according to my formula in this Journal for October 1826. The mean is ·205, which cannot err from the true value of f more than ± ·003. Now it is evident that this small error will not affect the product $f\sin^2\lambda$ near the equator, and so long as $\sin^2\lambda$ is less than 0·2. I have therefore computed the values of Δ by means of the formula,

$$\Delta = \delta - f\sin^2\lambda,$$

on the supposition that $f = {\cdot}205$, for all the stations in the table where $\sin^2\lambda$ is less than 0·2, as follows:

	Δ
Maranham......	·01173
Bahia............	·01398
Rio Janeiro.....	·01264
Trinidad........	·01188
Madras..........	·01289
San Blas........	·01066
	6)·07378(·01230, mean of 6.
Rawak..........	·01479
Sierra Leone...	·01550
Jamaica	·01560
	9)·11967(·01330, mean of 9.

The inequality of the several values of Δ is very remarkable; in particular the three stations placed last, greatly exceed the mean of the first six. But whatever be the cause of the irregularity, it cannot arise from the approximate value assigned to f. The mean of the nine stations must be an approximation sufficient for our present purpose. Let ·01330 be substituted for Δ in the equation (2), then $f = \cdot 2057$, which will serve for a first value of f. Now put

$$a = \cdot 01330, \qquad \Delta = a + s,$$
$$b = \cdot 2057 \qquad f = b + \tau,$$

s and τ being the corrections of the approximate quantities: then since Δ and f must satisfy the equation (2), and a and b likewise satisfy the same equation, we get,

$$s + \cdot 4686\,\tau = 0.$$

The formula (2) is one of the equations of the method of the least squares applied to all the 34 experiments; and in order to find the other equation of the same method, let all the terms of equation (1) be multiplied by the coefficient of f: then,

$$\Delta \sin^2\lambda + f \sin^4\lambda = \delta \sin^2\lambda + e \sin^2\lambda;$$

by substituting the values of Δ and f,

$$s \sin^2\lambda + \tau \sin^4\lambda = \sin^2\lambda\,(\delta - a - b \sin^2\lambda) + e \sin^2\lambda:$$

and the sum of the like equations for all the 34 stations will be the equation sought, viz.

$$s\,\Sigma\,(\sin^2\lambda) + \tau\,\Sigma\,(\sin^4\lambda) = \Sigma\,.\left(\sin^2\lambda\,(\delta - a - b \sin^2\lambda)\right),$$

the term containing e being neglected. Now,

$$\Sigma\,(\sin^2\lambda) = 15\cdot 9316,$$
$$\Sigma\,(\sin^4\lambda) = 10\cdot 4381;$$

wherefore,

$$15\cdot 9316 s + 10\cdot 4381\,\tau = \Sigma\,.\left(\sin^2\lambda\,(\delta - a - b \sin^2\lambda)\right):$$

and, by a former equation,

$$15\cdot 9316 s + 7\cdot 4654\,\tau = 0;$$

consequently,

$$2\cdot 973\,\tau = \Sigma\,.\left(\sin^2\lambda\,(\delta - a - b \sin^2\lambda)\right).$$

The sum on the right-hand side consists of thirty-four terms, which must be separately calculated by substituting for δ and $\sin^2\lambda$ their values at the several stations. The computation being made, and all the results combined according to their respective signs, I have found

$$2\cdot 973\,\tau = -\cdot 00061.$$

Hence, $\tau = -\cdot 00020$; $s = -\cdot 4686\,\tau = +\cdot 00009$;

$$f = \cdot 2055, \qquad \Delta = \cdot 01339.$$

If therefore $l = 39 + \delta$, be the length of the pendulum at the latitude λ, we have this general formula, viz.

$$l = 39{\cdot}01339 + {\cdot}2055 \sin^2 \lambda,$$

$$\text{ellipticity} = {\cdot}00865 - \frac{{\cdot}2055}{39{\cdot}01339} = {\cdot}00338 = \frac{1}{295{\cdot}5}.$$

Stations.	Latitude.	Observed Pendulum.	Computed Pendulum.	Excess of Calculation.	Observers.
		inches.	inches.		
Falkland Islands	51° 31′ 44″ S.	39·13945	39·13935	—·00010	Duperrey.
Ca. Good Hope	33 55 15	39·07817	39·07739	—·00078	Freycinet.
Port Jackson…	33 51 40	39·07899	39·07719	—·00180	Duperrey.
Paramatta ……	33 48 43	39·07622	39·07702	+·00080	Sir T. Brisbane.
Rio Janeiro. …	22 55 22	39·04374	39·04457	+·00083	Hall and Foster.
Bahia…………	12 59 21	39·02433	39·02377	—·00056	Sabine.
Maranham……	2 31 43	39·01213	39·01379	+·00166	Sabine.
Rawak ……….	0 1 34	39·01479	39·01339	—·00140	Freycinet.
Sierra Leone …	8 29 58 N.	39·01997	39·01787	—·00210	Sabine.
Trinidad………	10 38 56	39·01888	39·02041	+·00153	Sabine.
Madras ……….	13 4 9	39·02338	39·02390	+·00052	Goldingham.
Jamaica………	17 56 7	39·03503	39·03288	—·00215	Sabine.
San Blas . ……	21 32 24	39·03829	39·04109	+·00280	Hall and Foster.
Formentera. …	38 39 56	39·09424	39·09361	—·00063	Biot.
New York ……	40 42 43	39·10120	39·10081	—·00039	Sabine.
Toulon. ……….	43 7 20	39·10996	39·10941	—·00055	Duperrey.
Figeac. ……….	44 36 45	39·11322	39·11476	+·00154	Biot.
Bourdeaux……	44 50 26	39·11303	39·11557	+·00254	Biot.
Clermont. …..	45 46 48	39·11809	39·11893	+·00084	Biot.
Paris … ………	48 50 14	39·12929	39·12929	+·00056	Biot.
Dunnose . ……	50 37 24	39·13614	39·13618	+·00004	Kater.
Dunkirk………	51 2 10	39·13773	39·13762	—·00011	Biot.
London ………	51 31 8	39·13929	39·13932	+·00003	Kater.
Arbury Hill. …	52 12 55	39·14250	39·14174	—·00076	Kater.
Clifton. ………	53 27 43	39·14600	39·14605	+·00005	Kater.
Leith …………	55 48 41	39·15554	39·15454	—·00100	Kater.
Portsoy ………	57 40 59	39·16159	39·16016	—·00143	Kater.
Stockholm……	59 20 34	39·16541	39·16546	+·00005	Suanberg.
Unst …………	60 45 28	39·17146	39·16985	—·00161	Kater.
Drontheim……	63 25 54	39·17456	39·17778	+·00322	Sabine.
Hammerfest …	70 40 5	39·19475	39·19636	+·00161	Sabine.
Port Bowen …	73 13 39	39·20347	39·20177	—·00170	Foster.
Greenland ……	74 39 19	39·20328	39·20428	+·00100	Sabine.
Spitzbergen …	79 49 58	39·21464	39·21248	—·00216	Sabine.
Ascension ……	7 55 48 S.	39·02385	39·01730	—·00655	Sabine & Duperrey.
Isle of France.	20 9 40	39·04721	39·03780	—·00941	Freycinet & Dupʸ.
St. Thomas……	0 24 41 N.	39·02074	39·01339	—·00735	Sabine.
Galapagos……	0 32 19	39·01717	39·01341	—·00376	Hall.
Guam ……….	13 27 51	39·03023	39·02453	—·00570	Freycinet.
Mowi …………	20 52 7	39·04737	39·03946	—·00791	Freycinet.

This table shows the errors of the formula. The greatest error is at Drontheim, and it is nearly as much with the ellipticity $\frac{1}{288}$. The next greatest error is at San Blas; but this would be considerably diminished by adopting the length found by Captain Foster, viz. 39·03881, instead of the mean of the lengths found by that gentleman and Captain Hall, which stands in the table. The excess at Galapagos is little greater than the defect at Drontheim; and as we know that the former pendulum was determined in unfavourable circumstances, the experiment seems to be sufficiently well represented by the formula. We may therefore affirm that, if we adopt Captain Foster's pendulum at San Blas, the formula represents 35 experiments out of 40, within the limits of the probable errors: for the difference between the observed and the calculated lengths is less than what would arise from an error of 2 vibrations in a mean solar day, except at Sierra Leone, Jamaica, San Blas, Bourdeaux, and Spitzbergen, where the error is 2½ vibrations; and at Drontheim and Galapagos, where it amounts to 3½ and 4 vibrations. But at the five anomalous stations, the errors are between 6 and 10 vibrations in a day.

Without laying any stress on the mode of investigation we have here followed, it cannot, I think, be denied that the ellipticity $\frac{1}{293 \cdot 3}$ represents the observations much better than $\frac{1}{288}$; it ought therefore to be adopted until another shall be found, if it be possible, that will represent the same observations still better, or until future experimental researches shall have corrected and enlarged our present knowledge on this subject. What other reason can be alleged for preferring one ellipticity to another?

But if the experiments can be represented within the limits of the probable errors, what becomes of the splendid speculation about local attraction, which connects the apparent inequality of gravity with the variation of the soil. It is evident that we can have no measure of the excess or defect of gravity caused by the attraction of the adjacent matter, if the errors of observation be sufficient to account for the irregularity in the length of the pendulum. This theory too is quite uncertain, so long as there remains any doubt about the exact quantity of the ellipticity. At Maranham and Trinidad, Captain Sabine makes the observed pendulums gain upon the calculated ones upwards of 4 vibrations in a mean solar day; in my table the acceleration is only 1½, which may be attributed to the inaccuracy of experiment. According to the same gentleman,

tleman, the observed pendulum at Spitzbergen falls short of the calculated one $3\frac{1}{2}$ vibrations in a day;—in my table the defect is only $2\frac{1}{7}$. The density of the matter near the surface of the earth, varying at the different stations of the pendulum, must in some degree influence the time of vibration: but the speculation is premature; and there is good reason to think, at least in far the greater number of cases, that the effect in question will never be separated from the unavoidable errors of observation.

What has just been said will not, however, apply to the five anomalous experiments, all of which show an excess of gravity far surpassing the ordinary measure of experimental error. Besides, at two of the stations the pendulums have been verified by two independent experiments varying little in the results. It is remarkable that in all the five cases, and at Galapagos, there is an excess of gravity; and, according to my table, there is a like excess, though much less in quantity, at Rawak, Sierra Leone, and Jamaica. On the other hand, there appears to be a defect of gravity at Maranham, Trinidad, and San Blas. Thus there are nine instances of excess, and only three of defect; and we may suspect that there is some cause of error, different from local attraction, tending to make these tropical pendulums longer than they should be. At the Isle of France, the excess of gravity is no less than 10 vibrations a day; yet this is a small island, surrounded on all sides by an extensive sea; and we might infer, *à priori*, that a pendulum placed upon it very little above the level of the sea, instead of being accelerated, would be retarded by the great defect of density in the waters of the ocean. But the purpose of this paper is to deduce from the experiments the consequences that necessarily flow from them by a strict investigation; and I shall refrain from entering into the region of conjecture and opinion.

Feb. 11, 1828. J. IVORY.

XXVIII. *The Climate of Penzance, Cornwall.—Meteorological Results of the Temperature, Wind and Weather deduced from diurnal Observations made at Penzance for* 21 *Years: (the Thermometrical Observations made at* 8 *a.m. and* 2 *p.m.) to which are added the Maxima, Minima, and Media of the Register Thermometer for* 7 *Years, with the Inches of Rain fallen during that Period. By* EDWARD COLLINS GIDDY, *Curator of the Cabinet of the Royal Geological Society of Cornwall.*

JANUARY

JANUARY.															
	Thermom.			Winds.									Weather.		
Years	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
1807	52	37	47,0	1	6	2	5	2	0	4	11	NW	31	9	22
1808	50	26	41,5	1	4	0	1	2	6	11	6	W	31	18	13
1809	52	33	45,0	1	3	4	8	1	6	4	4	SE	31	19	12
1810	53	19	41,0	3	6	4	5	5	7	0	1	SW	31	10	21
1811	50	22	37,5	1	4	7	2	4	5	0	8	NW	31	14	17
1812	52	30	42,0	4	5	2	1	3	4	2	10	NW	31	16	15
1813	51	30	41,5	3	3	7	3	3	2	2	8	NW	31	12	19
1814	51	28	37,5	9	5	3	5	3	2	1	3	N	31	18	13
1815	50	26	40,0	4	10	0	3	3	3	0	8	NE	31	18	13
1816	52	32	43,5	2	1	2	5	2	4	3	12	NW	31	18	13
1817	54	32	47,0	7	0	1	2	4	2	12	3	W	31	19	12
1818	55	34	46,5	3	0	2	2	4	3	11	6	W	31	23	8
1819	55	38	47,0	0	2	1	3	7	6	8	4	W	31	23	8
1820	56	22	40,0	1	7	5	4	4	2	2	6	NE	31	14	17
1821	55	26	45,5	0	8	4	5	7	0	7	0	NE	31	16	15
1822	50	37	45,5	4	4	1	0	2	2	7	11	NW	31	16	15
1823	54	30	40,5	3	6	3	11	1	5	2	0	SE	31	17	14
1824	54	32	46,0	3	8	3	1	0	3	7	6	NE	31	14	17
1825	54	34	46,0	0	7	3	0	3	3	8	7	W	31	14	17
1826	55	27	43,0	4	2	8	10	0	2	1	4	SE	31	8	23
1827	54	25	44,5	2	4	4	3	0	3	8	7	W	31	19	12
For 21 years.	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
	56	19	43,0	56	95	66	79	60	70	100	125	NW	651	335	316

FEBRUARY.															
	Thermom.			Winds.									Weather.		
Years	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
1807	56	34	46,5	2	4	0	1	1	8	2	10	NW	28	15	13
1808	50	28	41,0	4	5	3	1	1	3	8	4	W	29	12	17
1809	54	34	47,0	0	3	0	2	0	16	0	7	SW	28	18	10
1810	56	26	45,0	0	5	3	2	5	7	5	1	SW	28	15	13
1811	54	41	45,5	0	0	0	7	3	9	4	5	SW	28	22	6
1812	54	37	47,0	4	0	0	3	4	7	5	6	SW	29	22	7
1813	52	34	46,0	0	1	1	4	4	7	6	5	SW	28	21	7
1814	52	32	42,0	1	5	2	8	2	2	4	4	SE	28	12	16
1815	56	36	49,5	1	0	0	6	3	10	4	4	SW	28	18	10
1816	56	26	44,0	0	6	0	1	9	1	5	7	S	29	10	19
1817	56	40	49,0	0	3	0	1	0	4	11	9	W	28	8	20
1818	58	34	48,5	3	0	1	4	6	2	3	9	NW	28	15	13
1819	55	38	47,5	3	1	0	1	4	4	7	8	NW	28	18	10
1820	56	30	44,0	4	8	2	1	6	5	0	3	NE	29	11	18
1821	52	36	43,5	2	1	13	6	2	0	3	1	E	28	3	25
1822	53	38	47,0	2	1	0	2	4	12	4	3	SW	28	11	17
1823	52	32	44,0	2	3	0	1	0	9	7	6	SW	28	22	6
1824	58	34	47,0	1	7	4	3	2	2	9	1	W	29	21	8
1825	56	36	47,5	1	0	5	6	0	5	7	4	W	28	12	16
1826	56	40	49,0	0	1	0	1	2	13	10	1	SW	28	19	9
1827	54	30	38,0	1	8	13	2	0	1	2	1	E	28	7	21
For 21 years.	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
	58	26	45,5	31	62	47	63	58	127	106	99	SW	593	312	281

MARCH.

	Thermom.			Winds.									Weather.		
Years	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
1807	54	30	42,0	2	15	4	3	2	0	1	4	NE	31	4	27
1808	54	30	42,0	0	6	10	12	2	0	0	1	SE	31	7	24
1809	60	32	46,5	0	4	12	1	2	7	0	5	E	31	7	24
1810	58	30	47,0	0	4	2	9	0	6	3	7	SE	31	12	19
1811	61	37	48,5	2	3	14	1	3	2	5	1	E	31	7	24
1812	60	28	44,0	6	3	5	5	0	7	1	4	SW	31	17	14
1813	62	28	49,0	6	6	0	1	3	4	3	8	NW	31	11	20
1814	58	30	42,5	0	7	4	3	0	8	2	7	SW	31	12	19
1815	62	38	51,5	0	0	0	2	12	3	4	10	S	31	21	10
1816	54	32	45,0	5	3	5	0	1	10	2	5	SW	31	16	15
1817	60	32	48,5	5	1	2	4	4	5	2	8	NW	31	18	13
1818	58	36	47,0	3	0	1	2	3	4	8	10	NW	31	23	8
1819	60	38	48,5	1	10	3	0	3	2	3	9	NE	31	10	21
1820	61	30	46,0	1	8	3	1	2	4	6	6	NE	31	9	22
1821	56	40	50,0	2	0	1	5	2	5	9	7	W	31	17	14
1822	56	44	50,0	2	1	1	4	5	10	6	2	SW	31	14	17
1823	54	38	46,0	5	0	2	6	0	7	6	5	SW	31	13	18
1824	62	36	47,0	4	7	0	0	4	3	5	8	NW	31	18	13
1825	60	38	48,5	1	8	5	5	2	4	4	2	NE	31	17	14
1826	66	38	49,0	3	7	5	2	6	1	3	4	NE	31	8	23
1827	55	36	48,5	2	1	0	2	1	1	18	6	W	31	18	13
For 21 years.	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
	66	28	47,0	50	94	79	68	57	93	91	119	NW	651	279	372

APRIL.

	Thermom.			Winds.									Weather.		
Years	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
1807	68	32	49,0	0	5	1	5	7	5	3	4	S	30	9	21
1808	60	34	46,0	5	1	1	4	3	3	2	11	NW	30	7	23
1809	58	34	46,0	4	7	1	1	0	6	2	9	NW	30	14	16
1810	66	34	51,5	1	2	3	6	4	10	2	2	SW	30	8	22
1811	62	32	51,0	1	3	3	5	7	6	2	3	S	30	10	20
1812	60	36	49,5	7	1	3	8	5	0	2	4	SE	30	9	21
1813	64	35	51,5	3	7	5	4	1	2	4	4	NE	30	10	20
1814	62	43	53,5	3	0	2	4	12	5	1	3	S	30	16	14
1815	66	40	52,5	6	6	6	3	1	0	0	8	NW	30	8	22
1816	68	38	50,0	6	4	2	8	2	2	2	4	SE	30	12	18
1817	62	39	50,5	6	10	6	3	1	0	1	3	NE	30	1	29
1818	64	38	51,5	1	4	9	3	7	1	2	3	E	30	14	16
1819	66	44	54,5	3	1	9	4	5	2	5	1	E	30	14	16
1820	66	40	53,0	6	3	7	1	0	3	1	9	NW	30	7	23
1821	64	45	53,5	0	4	0	6	1	5	3	11	NW	30	18	12
1822	64	40	50,5	9	2	2	3	4	5	2	3	N	30	10	20
1823	58	42	49,0	2	6	5	4	2	2	5	4	NE	30	12	18
1824	60	40	50,5	3	2	0	11	5	2	5	2	SE	30	17	13
1825	66	44	55,5	0	8	5	2	3	2	7	3	NE	30	11	19
1826	61	46	52,0	6	1	1	2	0	4	7	9	NW	30	10	20
1827	60	41	51,5	2	1	3	6	1	2	8	7	W	30	10	20
For 21 years.	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
	68	32	51,5	74	78	74	93	71	67	66	107	NW	630	227	403

MAY.

	Thermom.			Winds.									Weather.		
Years	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
1807	72	46	56,5	5	0	5	5	6	5	3	2	S	31	14	17
1808	71	44	58,0	1	0	2	7	8	11	1	1	SW	31	13	18
1809	72	44	59,5	1	0	2	5	10	5	3	5	S	31	8	23
1810	66	44	55,5	8	0	5	10	1	5	1	1	SE	31	8	23
1811	72	50	59,5	3	1	2	2	7	10	3	3	SW	31	20	11
1812	70	50	58,5	1	6	5	1	11	1	0	6	S	31	14	17
1813	68	46	58,5	2	1	0	5	9	3	7	4	S	31	15	16
1814	68	44	56,0	1	3	12	2	4	2	2	5	E	31	5	26
1815	71	52	60,5	9	0	1	2	8	3	6	2	N	31	16	15
1816	68	42	56,5	1	3	6	1	2	3	4	11	NW	31	14	17
1817	64	44	55,0	10	3	1	3	2	2	5	5	N	31	16	15
1818	70	48	59,0	2	5	11	0	3	4	3	3	E	31	7	24
1819	74	50	59,5	4	6	2	4	3	3	4	5	NE	31	16	15
1820	72	46	59,0	1	2	3	4	12	5	0	4	S	31	17	14
1821	62	46	55,5	1	3	3	5	4	0	9	6	W	31	15	16
1822	69	43	58,5	4	1	8	7	7	2	2	0	E	31	9	22
1823	70	52	58,0	1	2	4	3	5	9	4	3	SW	31	13	18
1824	72	44	57,0	4	7	6	1	3	1	3	6	NE	31	15	16
1825	67	48	60,0	4	4	3	4	7	3	2	4	S	31	16	15
1826	71	47	59,0	1	21	1	1	0	0	2	5	NE	31	5	26
1827	63	46	55,0	0	2	2	4	4	6	7	6	W	31	17	14
For 21 years.	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
	74	42	58,5	64	70	84	76	116	83	71	87	S	651	273	378

JUNE.

	Thermom.			Winds.									Weather.		
Years	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
1807	70	52	61,0	4	2	0	2	4	4	1	13	NW	30	3	27
1808	72	52	62,5	2	1	4	3	3	4	2	11	NW	30	5	25
1809	75	45	61,0	6	3	1	2	0	7	3	8	NW	30	9	21
1810	72	54	63,0	0	3	0	9	2	5	1	10	NW	30	7	23
1811	74	51	62,0	7	4	1	0	2	11	1	4	SW	30	14	16
1812	72	54	61,0	2	1	5	2	1	4	7	8	NW	30	9	21
1813	72	52	62,5	12	1	7	0	2	3	5	0	N	30	7	23
1814	74	50	61,5	8	2	2	2	6	3	3	4	N	30	4	26
1815	74	56	63,0	1	3	5	2	4	3	3	9	NW	30	11	19
1816	76	52	61,5	6	1	1	1	6	1	5	9	NW	30	8	22
1817	76	52	62,5	5	2	4	2	9	2	5	1	S	30	11	19
1818	77	58	67,5	4	0	7	0	1	3	7	8	NW	30	8	22
1819	72	56	61,0	6	2	1	1	3	4	7	6	W	30	17	13
1820	78	51	63,0	7	2	4	2	0	2	1	12	NW	30	8	22
1821	67	50	59,5	4	5	5	8	1	0	6	1	SE	30	9	21
1822	78	58	66,0	4	0	3	8	4	2	6	3	SE	30	10	20
1823	66	51	59,5	6	0	5	6	0	0	10	3	W	30	13	17
1824	70	55	62,5	3	7	3	3	5	4	2	3	NE	30	13	17
1825	75	52	65,5	3	6	2	2	5	3	5	4	NE	30	13	17
1826	80	58	68,5	1	4	7	9	1	0	0	8	SE	30	4	26
1827	70	53	60,0	1	6	0	3	1	3	4	12	NW	30	14	16
For 21 years.	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
	80	45	62,5	92	55	67	67	60	68	84	137	NW	630	197	433

JULY.

Years	Thermom.			Winds.									Weather.		
	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
1807	75	58	66,0	1	2	2	1	5	10	7	3	SW	31	6	25
1808	80	59	68,5	0	4	1	5	2	12	2	5	SW	31	12	19
1809	74	53	63,5	7	5	3	2	1	3	1	9	NW	31	9	22
1810	74	54	63,0	3	0	0	2	3	14	5	4	SW	31	17	14
1811	76	56	66,0	4	3	6	0	3	7	2	6	SW	31	6	25
1812	76	56	64,0	1	4	3	3	2	8	4	6	SW	31	11	20
1813	74	54	64,0	3	0	1	2	2	9	6	8	SW	31	14	17
1814	76	58	66,0	4	0	1	2	8	3	2	11	NW	31	11	20
1815	74	53	66,0	5	2	6	2	3	2	1	10	NW	31	12	19
1816	74	54	61,5	6	0	1	1	2	3	8	10	NW	31	18	13
1817	74	56	63,0	5	0	0	3	5	4	11	3	W	31	19	12
1818	78	59	68,0	9	0	6	0	3	2	6	5	N	31	9	22
1819	78	57	67,5	7	5	0	4	2	2	5	6	N	31	4	27
1820	77	60	66,0	2	1	6	4	7	1	5	5	S	31	9	22
1821	71	56	63,5	6	3	1	2	3	3	7	6	W	31	10	21
1822	71	56	64,0	3	0	1	4	3	4	9	7	W	31	22	9
1823	70	54	61,5	2	0	0	1	2	3	17	6	W	31	20	11
1824	71	60	67,0	5	4	3	2	2	6	5	4	SW	31	13	18
1825	84	60	69,5	3	7	4	13	1	2	0	1	SE	31	2	29
1826	75	60	68,5	2	2	2	5	0	9	3	8	SW	31	9	22
1827	72	58	65,5	1	3	1	4	0	3	7	12	NW	31	11	20
For 21 years.	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
	84	53	65,5	79	45	48	62	59	110	113	135	NW	651	244	407

AUGUST.

Years	Thermom.			Winds.									Weather.		
	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
1807	75	57	65,5	3	0	0	4	9	6	6	3	S	31	11	20
1808	74	54	66,0	1	8	0	0	4	9	3	6	SW	31	8	23
1809	73	53	62,5	0	0	0	1	4	14	3	9	SW	31	18	13
1810	70	53	62,0	3	0	2	1	5	6	3	11	NW	31	13	18
1811	70	54	61,5	4	0	1	0	4	7	7	8	NW	31	11	20
1812	72	54	64,5	8	1	4	3	6	2	1	6	N	31	5	26
1813	72	55	63,5	8	6	1	0	1	5	5	5	N	31	8	23
1814	74	56	64,0	1	0	1	0	6	7	6	10	NW	31	10	21
1815	74	58	65,0	6	0	4	0	1	9	6	5	SW	31	9	22
1816	70	56	62,0	10	2	5	0	4	3	4	3	N	31	12	19
1817	72	53	62,0	4	0	1	1	10	3	5	7	S	31	25	6
1818	76	58	66,0	11	4	9	0	0	1	3	3	N	31	6	25
1819	79	56	69,0	4	2	10	4	2	5	1	3	E	31	7	24
1820	72	50	63,0	5	2	1	2	2	9	7	3	SW	31	20	11
1821	72	58	65,0	2	0	0	11	2	4	9	3	SE	31	14	17
1822	76	54	63,0	3	2	0	7	2	7	8	2	W	31	10	21
1823	68	55	61,0	2	0	1	2	4	9	10	3	W	31	25	6
1824	72	58	64,0	3	4	1	4	0	2	11	6	W	31	18	13
1825	73	60	65,5	2	2	3	7	0	2	6	9	NW	31	13	18
1826	76	59	67,5	1	4	2	2	2	5	8	7	W	31	9	22
1827	69	55	65,0	3	13	1	1	1	2	4	6	NE	31	15	16
For 21 years.	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
	79	50	64,5	84	50	47	50	69	117	116	118	NW	651	267	384

SEPTEMBER.

Years	Thermom. Max.	Thermom. Min.	Thermom. Mean	Winds. N	Winds. NE	Winds. E	Winds. SE	Winds. S	Winds. SW	Winds. W	Winds. NW	P. Wind	Weather. N° of days	Weather. Wet	Weather. Dry
1807	68	44	57,0	6	3	0	0	3	7	0	11	NW	30	14	16
1808	68	42	59,0	6	5	3	1	3	4	1	7	NW	30	15	15
1809	74	48	59,5	0	6	0	4	0	4	5	11	NW	30	14	16
1810	72	52	58,0	5	3	4	3	5	4	0	6	NW	30	5	25
1811	70	49	60,0	1	1	13	4	3	2	0	6	E	30	8	22
1812	72	54	61,5	8	1	6	1	1	3	6	4	N	30	9	21
1813	68	49	60,0	3	3	7	2	3	4	5	3	E	30	13	17
1814	68	50	61,0	0	1	15	2	4	3	1	4	E	30	12	18
1815	74	52	63,0	0	6	5	2	4	8	4	1	SW	30	11	19
1816	68	49	60,0	2	3	3	1	3	4	7	7	W	30	12	18
1817	70	50	60,0	1	1	14	5	4	2	1	2	E	30	8	22
1818	72	52	61,0	4	0	2	3	5	5	6	5	W	30	17	13
1819	74	48	63,0	3	4	6	2	5	7	1	2	SW	30	14	16
1820	70	48	59,0	6	1	3	7	0	4	5	4	SE	30	13	17
1821	71	54	62,0	1	0	0	2	3	11	6	7	SW	30	16	14
1822	68	47	59,0	2	2	6	8	1	7	2	2	SE	30	9	21
1823	67	50	59,0	1	0	5	3	2	2	11	6	W	30	13	17
1824	68	49	61,0	6	0	1	8	1	6	5	3	SE	30	18	12
1825	72	57	64,0	2	4	1	3	8	3	5	4	S	30	10	20
1826	70	56	63,0	2	1	5	8	1	3	6	4	SE	30	7	23
1827	68	53	60,0	1	6	5	1	1	5	4	7	NW	30	13	17
For 21 years.	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
	74	42	60,0	60	51	104	70	60	98	81	106	NW	630	251	379

OCTOBER.

Years	Thermom. Max.	Thermom. Min.	Thermom. Mean	Winds. N	Winds. NE	Winds. E	Winds. SE	Winds. S	Winds. SW	Winds. W	Winds. NW	P. Wind	Weather. N° of days	Weather. Wet	Weather. Dry
1807	68	42	56,5	3	5	0	0	9	6	4	4	S	31	11	20
1808	60	46	50,5	4	0	2	0	3	3	3	16	NW	31	17	14
1809	66	45	53,0	0	0	5	15	7	0	3	1	SE	31	3	28
1810	65	38	55,5	3	3	5	6	2	7	0	5	SW	31	13	18
1811	64	40	58,5	0	1	0	1	10	15	1	3	SW	31	13	18
1812	67	42	54,5	5	0	0	0	10	4	4	8	S	31	18	13
1813	68	37	53,5	3	0	6	5	5	7	0	5	SW	31	15	16
1814	62	42	53,0	0	1	4	7	8	2	3	6	S	31	11	20
1815	65	44	55,0	0	4	2	4	6	6	2	7	NW	31	19	12
1816	68	42	56,0	6	1	2	9	5	4	2	2	SE	31	17	14
1817	58	40	50,0	7	5	9	2	2	0	0	6	E	31	10	21
1818	65	49	59,0	1	1	8	4	9	5	2	1	S	31	18	13
1819	68	38	54,5	4	6	1	2	4	2	2	10	NW	31	15	16
1820	62	45	53,0	2	1	7	4	0	4	0	13	NW	31	17	14
1821	64	46	55,5	5	1	0	3	3	10	5	4	SW	31	18	13
1822	62	47	54,5	3	0	0	7	9	3	8	1	S	31	20	11
1823	64	44	54,0	0	1	1	9	4	1	11	4	W	31	19	12
1824	64	42	55,0	2	2	0	3	7	5	3	9	NW	31	26	5
1825	65	46	58,0	5	2	1	1	5	1	9	7	W	31	12	19
1826	64	50	58,0	0	0	1	4	4	3	7	12	NW	31	7	24
1827	64	45	55,5	1	3	0	10	3	5	4	.5	SE	31	22	9
For 21 years.	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
	68	37	55,5	54	37	54	96	115	93	73	129	NW	651	321	330

NOVEMBER.

	Thermom.			Winds.									Weather.		
Years	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
1807	57	30	42,0	1	5	2	2	0	5	5	10	NW	30	19	11
1808	56	40	49,0	0	1	6	7	2	3	6	5	SE	30	14	16
1809	54	28	46,0	1	7	6	4	0	1	1	10	NW	30	9	21
1810	57	36	46,0	0	6	0	1	1	10	3	9	SW	30	21	9
1811	58	38	49,0	7	3	2	0	0	7	0	11	NW	30	20	10
1812	58	32	49,0	0	6	3	5	7	4	0	5	S	30	14	16
1813	62	40	48,5	0	1	5	4	6	2	4	8	NW	30	16	14
1814	56	36	47,0	1	5	1	2	8	2	2	9	NW	30	11	19
1815	59	32	46,0	6	2	9	1	2	4	1	5	E	30	10	20
1816	55	36	46,0	6	6	2	3	1	3	1	8	NW	30	16	14
1817	58	40	53,0	1	1	0	1	8	10	2	7	SW	30	11	19
1818	62	48	55,0	1	0	2	7	6	8	2	4	SW	30	20	10
1819	58	34	47,0	7	6	1	0	3	2	2	9	NW	30	21	9
1820	58	38	49,0	3	8	4	5	2	5	0	3	NE	30	18	12
1821	60	41	52,0	2	0	0	4	7	9	8	0	SW	30	21	9
1822	58	42	51,5	1	1	0	0	8	10	9	1	SW	30	22	8
1823	58	42	50,5	1	1	12	0	7	5	2	2	E	30	10	20
1824	58	44	52,0	0	0	0	4	1	8	6	11	NW	30	24	6
1825	57	38	50,5	4	2	0	0	0	3	9	12	NW	30	25	5
1826	56	36	48,5	5	7	0	1	0	1	5	11	NW	30	13	17
1827	56	40	52,0	0	4	0	3	4	0	12	7	W	30	21	9
For 21 years.	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
	62	28	49,0	47	72	55	54	73	102	80	147	NW	630	356	274

DECEMBER.

	Thermom.			Winds.									Weather.		
Years	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
1807	50	29	43,5	3	0	0	8	3	3	5	9	NW	31	13	18
1808	54	27	42,5	2	6	4	7	1	1	5	5	SE	31	12	19
1809	54	34	45,0	2	0	0	0	0	8	5	16	NW	31	23	8
1810	53	32	45,0	1	2	4	1	1	8	3	11	NW	31	21	10
1811	54	29	44,0	4	5	0	2	0	13	0	7	SW	31	20	11
1812	54	26	41,0	1	8	10	3	3	0	2	4	E	31	6	25
1813	54	30	46,5	3	9	4	5	2	1	5	2	NE	31	16	15
1814	58	34	48,0	2	1	2	2	5	6	7	6	W	31	20	11
1815	52	22	44,0	2	3	1	0	0	11	4	10	SW	31	16	15
1816	54	30	44,0	1	2	2	2	5	3	11	5	W	31	23	8
1817	58	30	44,0	1	5	1	0	5	2	3	14	NW	31	24	7
1818	58	34	45,5	5	6	3	7	2	4	2	2	SE	31	10	21
1819	56	26	43,5	3	6	1	0	3	7	2	9	NW	31	20	11
1820	56	28	46,5	1	7	1	3	1	8	7	3	SW	31	11	20
1821	55	38	48,0	0	0	0	2	4	16	2	7	SW	31	29	2
1822	53	28	42,0	1	6	10	4	0	2	6	2	E	31	19	12
1823	54	41	48,0	3	0	1	0	2	3	16	6	W	31	19	12
1824	57	39	50,5	3	0	1	3	0	7	12	5	W	31	28	3
1825	56	31	47,0	3	2	0	5	1	3	6	11	NW	31	16	15
1826	58	38	49,0	9	3	0	1	2	4	5	7	N	31	15	16
1827	54	42	49,5	0	1	0	2	1	7	15	5	W	31	21	10
For 21 years.	Max.	Min.	Mean	N	NE	E	SE	S	SW	W	NW	P. Wind	N° of days	Wet	Dry
	58	22	46,0	50	72	45	57	41	117	123	146	NW	651	383	268

Months	Thermom.			Winds.									Weather.										
	Maxima	Minima	Media	N	NE	E	SE	S	SW	W	NW	Prevailing wind	Partial showers	Showery days	Hail showers	Snow showers	Snowy days	Misty days	Partial rain	Rainy days	Number of days	Wet days	Dry days
Jan. .	56	19	43,0	56	95	66	79	60	70	100	125	NW	61	23	23	20	7	32	93	76	631	335	316
Feb. .	58	26	45,5	31	62	47	63	58	127	106	99	SW	52	22	25	11	0	33	104	65	593	312	281
Mar. .	66	28	47,0	50	94	79	68	57	93	91	119	NW	67	14	25	8	0	16	98	51	651	279	372
April	68	32	51,5	74	78	74	93	71	67	66	107	NW	68	18	27	0	0	15	64	35	630	227	403
May .	74	42	58,5	64	70	84	76	116	83	71	87	S	72	11	7	0	0	21	125	37	651	273	378
June .	80	45	62,5	92	55	67	67	60	68	84	137	NW	70	6	5	0	0	14	86	16	630	197	433
July .	84	53	65,5	79	45	48	62	59	110	123	135	NW	65	10	1	0	0	30	98	40	651	244	407
Aug. .	79	50	64,5	84	50	47	50	69	117	116	118	NW	88	14	0	0	0	22	110	33	651	267	384
Sept. .	74	42	60,0	60	51	104	70	60	98	81	106	NW	86	16	5	0	0	13	97	34	630	251	379
Oct. .	68	37	55,5	54	37	54	96	115	93	73	129	NW	79	38	17	1	0	16	100	70	651	321	330
Nov. .	62	28	49,0	47	72	55	54	73	102	80	147	NW	86	30	22	3	0	26	107	82	630	356	274
Dec. .	58	22	46,0	50	72	45	57	41	117	123	146	NW	84	35	43	13	0	16	100	92	651	383	268
For 21 years.	Maximum	Minimum	Medium	N	NE	E	SE	S	SW	W	NW	Prevailing wind	Partial showers	Showery days	Hail showers	Snow showers	Snowy days	Misty days	Partial rain	Rainy days	Number of days	Wet days	Dry days
	84	19	54,5	741	781	770	835	839	1,145	1,104	1,455	NW	878	237	200	56	7	254	1,182	631	7,670	3,445	4,225

Inches of Rain fallen during that Period.

Years.	January.				February.				March.				April.				May.				June.			
	Register Thermom.			Rain in inches	Register Thermom.			Rain in inches	Register Thermom.			Rain in inches	Register Thermom.			Rain in inches	Register Thermom.			Rain in inches	Register Thermom.			Rain in inches
	Max.	Min.	Med.		Max.	Min.	Med.		Max.	Min.	Med.		Max.	Min.	Med.		Max.	Min.	Med.		Max.	Min.	Med.	
1821	55	26	45,0	2,900	52	32	41,5	0,520	56	34	46,5	4,205	64	37	50,0	2,300	62	40	50,5	3,385	67	43	56,5	1,560
1822	50	34	44,0	2,42,5	54	36	45,5	2,440	58	39	49,5	2,800	65	36	48,0	2,380	69	42	55,5	1,340	78	52	64,0	2,240
1823	54	27	39,0	8,625	52	29	43,0	6,965	55	35	46,0	3,205	58	38	47,0	1,980	70	44	55,0	2,770	66	44	55,5	1,940
1824	54	30	44,5	2,00,5	58	34	46,0	3,280	62	32	45,0	5,740	60	36	45,0	2,600	72	38	53,0	3,95,5	70	47	57,0	4,660
1825	55	32	45,0	3,375	57	35	45,5	2,700	61	35	46,0	3,610	67	36	48,0	1,005	67	48	55,0	4,085	75	47	58,0	1,800
1826	55	26	41,0	3,075	56	36	47,0	5,295	66	34	46,5	2,310	61	33	50,5	1,110	71	38	54,0	0,600	80	40	62,0	0,660
1827	54	24	42,0	4,420	54	27	38,0	1,600	56	32	47,0	5,265	61	36	48,0	1,360	63	40	52,0	5,315	71	47	58,0	2,160
	Max.	Min.	Med.	Inches	Max.	Min.	Med.	Inches	Max.	Min.	Med.	Inches	Max.	Min.	Med.	Inches	Max.	Min.	Med.	Inches	Max.	Min.	Med.	Inches
	55	24	43,0	26,825	58	27	44,5	22,800	66	32	46,5	27,135	67	33	48,5	12,735	72	38	53,0	21,450	80	40	59,0	15,020

	July.				August.				September.				October.				November.				December.			
1821	71	51	60,5	1,660	73	55	63,0	4,470	72	49	60,5	4,520	64	41	54,0	5,770	60	40	51,0	5,170	56	36	47,5	9,500
1822	71	52	61,0	9,070	77	48	61,0	2,200	68	45	59,0	1,865	63	43	53,5	5,315	58	39	50,0	7,255	54	28	40,5	2,545
1823	70	49	59,0	3,030	68	48	59,5	7,085	67	46	57,5	3,645	64	37	51,5	8,210	58	38	48,5	3,705	55	40	46,5	6,295
1824	71	50	61,0	3,180	72	49	61,0	2,600	68	40	51,5	4,165	64	39	54,0	5,965	58	36	50,5	6,910	57	32	48,0	6,470
1825	84	49	63,0	0,310	73	52	62,0	3,140	72	46	58,5	2,730	65	38	54,0	5,090	57	35	47,5	5,820	56	29	45,5	5,285
1826	75	52	64,0	1,240	76	52	63,0	2,115	70	46	60,0	4,315	65	43	56,0	2,840	56	34	46,0	4,280	58	36	50,0	4,400
1827	73	52	61,5	2,250	70	50	59,0	2,865	68	48	58,0	2,820	63	42	54,5	6,105	56	33	50,5	3,165	55	40	49,0	7,580
	Max.	Min.	Med.	Inches	Max.	Min.	Med.	Inches	Max.	Min.	Med.	Inches	Max.	Min.	Med.	Inches	Max.	Min.	Med.	Inches	Max.	Min.	Med.	Inches
	84	49	61,0	20,740	77	48	61,5	24,475	72	40	58,0	24,060	65	37	54,5	39,295	60	33	49,0	36,305	58	28	46,5	42,075

Annual Results for 7 *Years.*

Months	Register Thermometer			Rain in inches	Years	Register Thermometer			Inches of rain fallen in each year	Wet days	Dry* days
	Max.	Min.	Med.			Max.	Min.	Med.			
Jan.	55	24	43,0	26,825	1821	73	26	52,5	46,020	186	179
Feb.	58	27	44,5	22,800	1822	78	28	53,0	41,875	172	193
Mar.	66	32	46,5	27,135	1823	70	27	51,0	57,455	196	169
Apr.	67	33	48,5	12,735	1824	72	30	51,5	51,470	225	141
May	72	38	54,0	21,450	1825	84	29	52,0	38,950	161	204
June	80	40	59,0	15,020	1826	80	26	53,5	32,240	114	251
July	84	49	61,0	20,740	1827	73	24	51,5	44,905	188	177
Aug.	77	48	61,5	24,475	..	..	..	..	..	..	..
Sept.	72	40	58,0	24,060	..	..	..	..	..	..	..
Oct.	65	37	54,5	39,295	..	..	..	..	..	..	..
Nov.	60	33	49,0	36,305	..	..	..	..	..	..	..
Dec.	58	28	46,5	42,075	..	..	..	..	..	..	..
	Max. of 7 years	Min. of 7 years	Med. of 7 years	Inches of rain fallen in 7 years		Med. of the Max.	Med. of the Min.	Med. of 7 years	Average quantity of rain for 7 years	Average wet days for 7 years	Average dry days for 7 years
	84	24	52,0	312,915		75,5	27,1	52,0	44,702	177,3	187,5

The thermometers I use are two double registers,—one made by Newman, and the other by Cary,—which are placed in a north-eastern aspect, in such a manner as not to be acted upon by reflected heat.

My rain-gauge is placed in my garden at the level of the ground, entirely free from the influence of buildings or trees. The gauge is exactly six inches diameter at the edge of the brass rim at the top. The basin is made of pewter 2½ inches deep. The tube attached to the basin is three inches long, tapered to about ⅛th of an inch at the point. The measure is a glass cylinder graduated to a quarter of an inch of water, or $1784\frac{8}{10}$ grains, nearly 1784·796 grains.

Penzance, Jan. 18th, 1828. EDWARD C. GIDDY.

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* Dry days are those on which no fall whatever takes place,—not the slightest shower.

XXIX. *Description*

XXIX. *Description of New Succulent Plants. By* A. H. Haworth, *Esq. F.L.S. &c.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

HAVING finished another new Decade, viz. the eleventh, of new Succulent Plants, which may now commence a second century of them, I lose no time in forwarding it to you, as under; hoping it may soon find a place in your very useful Annals.

The Decade contains, as usual, a regular and scientific distribution and description of ten distinct species of Succulent Plants; all, as far as my long experience enables me to determine, perfectly new and unrecorded. And no less than eight of these fine plants are from our old and most productive source, the Cape of Good Hope; where Mr. Bowie, detected them, and succeeded in transmitting them to the royal gardens of Kew, where they are now flourishing, and where the descriptions were made from the living plants.

Of the native countries of the two remaining plants which constitute the Decade, I am not quite so certain. But if they are not both from North America, one of them at least, viz. the *Yucca*, is probably from thence, as the greater part of its noble genus has been found there.

I remain, Gentlemen, yours, &c.

Chelsea, Sept. 1827. A. H. Haworth.

Decas undecima Plantarum Novarum Succulentarum.

Classis et Ordo. Pentandria Pentagynia.

Genus, Sedum *Auctorum.*

Sectio, * Acutifolia *Nob.*

viridulum. S. (small green) foliis suberectis lineari-subulatis
1. viridibus uno latere submucronulatis.

Nova species, ex horto Chelseiano, communicante amico Dom. Anderson.

Obs. Patriam nescio, nisi Europæam seu Americanam, vix Asiaticam. Sub dio cum affinibus viget.

Obs. *Sedo virenti* affine; et *S. recurvato* Willd. simillimum, at foliis paulò majoribus planioribus viridioribus, et sine dubio obtusioribus, potiùsve ad apicem quasi à latere inferiore obliquè truncatis. Minus est quàm *Sedo anopetalo*, atque viridius.

Genus, Curtogyne *Nob. in Revis. Pl. Succ. p.* 8.

undosa. C. (reflexing-leaved) foliis ovato-linguiformibus cris-
2. pis, ramulorum florentium retroflexis.

Habitat ad Caput Bonæ Spei, ubi in locis natalibus invenit amicus Dom. Bowie, misitque ad regium hortum Kewensem.

Floret Aug. G. H. ♄.

Obs. Simillima *Crassulæ albæ Sims. in Bot. Mag.* at major, floribus ut in illâ. Subpedalis ramosa, ramis florentibus, calamo sæpè crassioribus, et terram versus decumbentibus, apicibus assurgentibus. *Folia* arctè amplexicaulia decussata numerosa viridia: infra flores imbricatim retroflexa, atque in aëre aperto sæpè rufo-cincta: in ramis junioribus longiora magísque crispatim undosa, et sæpè sesquiunguicularia. Etiam simulat *Crassulam undosam Nob.* at major, magísque undosa: sed pòst id locarem.

Obs. *Squamula* ordinaria ad lentem quadratim truncata retusa pallida at cerina. *Styli* internè (lentis ope) cultrati et puberulo-ciliati.

Classis et Ordo. PENTANDRIA PENTAGYNIA.

PURGOSEA* (TURGOSEA *Nob. in Revis. Succ. p.* 14.) *Crassula Thunb.*

Inflorescentia spicato-thyrsoidea. *Corolla* 5-petala, *petalis* basi læviter imbricantibus, infra apicem mucronulatis. *Germina* intùs plana, extùs gibba, *squamulis* ordinariis emarginatis.

Sectio, LINGUÆFOLIA, foliis lorato-linguæformibus subacutis crassis.

pertusula. P. (small impress-dotted) foliis lanceolatis recur-
3. vulis suprà subimpresso-punctatis; bracteis summis cordatis integris, scapo paniculato.

Habitat ad Cap. B. Sp. ubi invenit Dom. Bowie.

Florebat in regio horto Kewensi, Oct., Nov., &c. A.D. 1824. G. H. ♃.

Obs. *T. pertusæ* simillima, at plùs duplò minor. *Folia* radicalia subbiuncialia 4-5 lineas lata crassa pallidè viridia, rubro sæpè infernè suffusa, minutè cartilagineo-serrulata; ad lentem parcéque (florendi tempore) impresso-punctata: (nec incurva semicylindracea impresso-punctata 4-5-uncialia, 9 lineas lata, ut in *T. pertusâ.*) *Scapus* infernè longè ramosus, ramis simplicibus, vix puberulus. *Bracteæ* numerosissimæ usque ad

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* A voce *πύργος*, *Turris*: wrongly printed *Turgosea*, in *Revis. Succ.*

30 paria, cordato-lanceolatæ expansæ internodiis longiores; imæ remotæ, superiores pedetentim magìs approximatæ. *Flores* pauciores in singulo fasciculo quàm in *C. pertusâ*, pedunculo sæpè longiore. *Corolla* nivea *petalis* erectis sed ultra medium revoluto-recurvis. *Styli* basi collecti erecti, supernè recurvo-patentes breves pallidè virides, *stigmate* (per lentem) hemisphærico limpido sive albo.

Obs. The specific character of *C. pertusa* will now require to be altered as follows:—Foliis lorato-acuminatis incurvis semicylindraceis, bracteis superioribus ovato-lanceolatis cartilagineo-serrulatis adscendentibus; scapo thyrsiformi.

Obs. *T. pertusa* is my ancient *Aloë pertusa*, taken from Commeline (Vide *Revis. Pl. Succ.* 15 & 201, A.D. 1821); and it is also *Crassula corymbulosa* of the same work, *p.* 10, (described from a bad and dead specimen); and likewise *Crassula corymbulosa* of *Link's Hort. Berl.* A.D. 1821.

Classis et Ordo. DECANDRIA PENTAGYNIA.

Genus, COTYLEDON *Auctorum.*

Sectio, * LATIFOLIA.

cuneiformis. C. (short, wedge-leaved) brevicaulis: ramosa:
4. foliis confertis obovato-cuneatis mucronatis subfarinoso-albis.

Habitat ad Cap. B. Sp. ubi invenit Dom. Bowie, ante ann. 1824.

Obs. *Caudex* brevis fruticosus valdè ramosus. *Folia* subindè (per culturam) virescent, sed sæpè farinoso-alba. *C. crassifoliæ Nob.* similis, at multoties humilior. *Flores* non vidi.

Sectio nova, VILLOSULÆ.

interjecta. C. (fleshy-stemmed) foliis angustè oblongis acutis
5. inflexo-canaliculatis; caudice brevi valido.

Habitat ad Cap. B. Sp. Dom. Bowie. G. H. ♄.

Flores non vidi.

Obs. *Caudex* teres adhuc 4-uncialis, tuberoso-ramosus carnosus. *Folia* incurva crassa carnosa glaucescente-cinerea triuncialia, quinque lineas lata; subtus convexa, basi semiteretia, sine carina, *C. spuriæ* proxima, sed altior, *foliis* brevioribus crassioribus angustioribus, magisque canaliculatis, et sine dubio incurvis, nec recurvis. In *C. spuria, folia* 4-5 uncias longa petiolo desinentia, 9 lineas lata recurva, et spatulato-lanceolata. Viget in regio horto Kewensi, sed *flores* non vidi.

Classis et Ordo. HEXANDRIA MONOGYNIA.

Genus, YUCCA *Auctorum*, et *Nob. in Suppl. Pl. Succ.* 31, charactere novo ampliori.

Sectio, * * FILIFERÆ *Nob. in loco.*

puberula. Y. (pubescent, thready) acaulis: foliis lorato-lanceolatis patentibus glaucis; fibris marginalibus pauculis fulvis: ramulis florigeris densè puberulis.
6.

Habitat fortè cum cæteris affinibus in Americâ Septentrionali. Communicavit florentem mense Augusti A.D. 1827, amicus Dom. Sweet, *Horti Britannici; Geraniacearum; Cistinearum; Floræ Australasiæ Iconum, &c. &c.* utilissimus Auctor.

Obs. In sectione FILIFERÆ inter *Yuccam glaucescentem Nob.* et *Y. recurvam Salisb. Parad. Lond.* 31. speciem hanc conspicuam pro certo locarem. *Folia* inflexo-concavula, deorsùm ad costam magìs magísque incrassescentia. *Scapus* affinium quadripedalis ramosus corymbosus, infernè magìs bracteolatus; *floribus* lævibus ovato-globulosis, pendulis lacteis. *Petala* lanceolata, interiora longiora, ferè duplò, atque quàm aliis latiora.

Obs. I believe Mr. Sweet received the fine plant above described, in bloom, from Mr. Millar's nursery at Bristol, the day before he brought it to me: and we may soon expect a figure of it in his beautiful *Hardy Flower Garden.*

Genus, HAWORTHIA *Duval in Cat. Pl. Succ. in Horto Alençonio*, A.D. 1809.

Sectio, MARGARITIFERÆ.

clariperla. H. (small, bright-pearled) foliis subulato-acutis undique præclari-perlatis, perlis subtùs majoribus.
7.

Habitat ad Cap. B. Sp. Etiam viget apud amicum Dom. Hitchin, Nordovici, qui nuper viventem communicavit.

Obs. Inter *H. attenuatam* et *H. fasciatam* apparet; priorem valdè simulat sed minorem, perlis longè pulchrioribus et clarioribus, et subtùs sæpè fasciatim serialibus; foliis longè brevioribus sive minùs attenuatis. Fortè *H. fasciatæ* magìs affinis, præcipuè in magnitudine. *Flores* ut in affinibus. Ab *H. attenuatâ* distinguitur, perlis undique magìs extantibus pulchrioribus; et ab *H. fasciatâ*, foliis undique (nec infrà solùm) perlatis.

Classis et Ordo. DODECANDRIA TRIGYNIA.

Genus, EUPHORBIA *Auctorum.*

Sectio, FLORISPINÆ *Nob.*

pentagona. 8. E. (Cape, 5-angled) erecto-decumbens: dodrantalis: spinis subsemuncialibus.

Habitat ad Cap. B. Sp. ubi invenit Dom. Bowie, ante ann. 1825. G. H. ♄.

Obs. Caudex adhuc pentagonus, diametro semunciali, supernè glaucescens, sulcis validis, et concinnè lineolâ exaratâ impressis. *Spinæ* adhuc trilineares ultráque, florigeræ ut in *Euphorbiâ enneagonâ Nob.* cuî simillima, et antè id locarem.

Obs. Caudices in horto caldario, decumbentes at grossi, apicibus assurgentibus, *spinis* citiùs marcescentibus; et postea decidentibus affinium more. *Flores* affinium, sed perfectos non examinavi.

Classis et Ordo. DODECANDRIA MONOGYNIA.

RÜLINGIA *Ehrhart.* Portulacca *Linn.*

intermedia. 9. R. (thready, intermediate) foliis densissimè numerosis expansis planis, extùs convexis; apice retuso-deltoidibus; filamentis axillaribus tortilibus fulvis.

Habitat ad Cap. B. Sp. ubi invenit Dom. Bowie, circa ann. 1823. G. H. ♄.

Obs. R. polyphyllæ Nob. in Suppl. Pl. Succ. p. 63. simillima, at duplò plúsve minor, seu angustior, sed fortassè elatior. Etiam simulat *R. filamentosam Nob.* at latior, foliis numerosioribus confertioribus pallidioribus, pilis axillaribus fulvicantibus, nec albis.

Obs. Surculi sæpiùs simplices subtrilineares, diametro semunciali, incomptè piliferi. *Folia* arachnoideo-incompta, sordida, sive fusco- seu rufo-viridia. *Flores* affinium.

Classis et Ordo. SYNGENESIA POLYGAMIA NECESSARIA.

Genus, CINERARIA *Auctorum.*

vestita. 10. C. (cottony) subacaulis: lanato-vestita: foliis multifariis subtereti-spatulescentibus planiusculis.

Obs. Caudex crassus, 2-3-uncialis, *ramulis* e terrâ pullulantibus et demùm cæspitosè crescentibus.

Folia 3-4-uncialia varianter depresso-teretiuscula succulenta, niveo lævi atque nitenti tomento, uti caudices eleganter vestita, *Kleinii tomentosi Nob.* exacto more, cui forsan ob multifaria folia affinis, sed non resinosa ut in illâ. *Foliorum* majorum apices sæpè plùs minùs impressi potiùs quàm planatim-spatulescentes. *Flores* non vidi, sed secundùm Dom. Bowie, corymbosi, et ut in *Cinerariâ.*

Obs. Cinerariam cacalioidem cui forsan simillima appareat proxima.

P.S.—The *Epiphyllum truncatum Nob. Sup. Pl. Succ. p.* 85. has, since that publication appeared, been figured on *t.* 696. of the *Botanical Register;* and my friend Mr. Hood, about the end of November last sent me, from his fine collection at South Lambeth, a specimen, in bloom, of this beautiful plant. And the following is a description of its remarkable flower:

Flos in medio apicis ramulorum terminalis, solitarius triuncialis subincurvulus elegantissimus, tubo cum germine subrectangulatim curvato. *Germen* nudum virescens truncatim semunciale, subpyriforme, utroque carinulato, seu lineâ protuberante. *Corollæ* tubus proprius satis crassus roseus nitens, distanter petalino-squamosus; et basi, calyculatus cum *foliolis* reflexis oblongo-ovatis omninò corallinis coccineis, sed infernè violascentibus. *Petala propria*, numerosa, ringenti-reflexa, calycinis foliolis valdè similia. *Stamina* numerosa collecta capillacea nitentia alba, apicem versus incurvula, petalisque parùm humiliora. *Antheræ* (in nostro exemplo) elegantes stramineæ, sed exiguæ; et (fortassè per Novembris frigorem) sine fœcundante farinâ.

XXX. *On the supposed Subsidence of the German Ocean.*

By A CORRESPONDENT.

HAVING only just met with No. 9, of Vol. II., for last September, of the Philosophical Magazine, it may be useless, probably, (from the disputed point having been settled,) to recommend that the following experiment should be tried, in order to decide the hypothesis of Mr. Robberds, on the former level of the German Ocean, viz.: A surveyor should be employed to take an *exact* level from high water mark of the present day—not the very highest tides, but a mean spot of spring-tides—up to the furthest spot recorded in Domesday, where a salina was situated, and then measure *down* to the marine deposition, which, according to Mr. Robberds, is found under an alluvial deposit. One composed of fresh water and marine shells intermixed. This *deposit** may naturally be allowed to mark the highest tide of those days. And if it is found that the *present* level of high water is below the more ancient one in the valleys, it is proved at once that the ocean has receded at that particular district; but if the levels are found to be nearly the same, it will be proved that the sea has been excluded, and thus set the question completely at rest: at present I lean towards the latter opinion, from circumstances in some degree similar on the Kentish coast near Hythe. The sea was known to have come much nearer the hills formerly, than it now does; but the level of the Military Canal proves that the level of high water has not decreased, and that the change has taken place by the formation, by the ocean, of natural barriers of shingle, over which it very rarely flows, and even then in such small quantities as to be harmless; whilst the lower parts of Romney Marsh are liable to inundations to a great extent whenever a breach is made in Dymchurch, or the other walls: thus adding proof that the sea has not decreased on this coast, but is kept out by natural and artificial boundaries.

It would add to the proofs, if the mean of the *low* water level was also carried on, and the *bottom* of the ancient sandy deposit bored to, and measured.

Sittingbourne, Feb. 4, 1828. J.

XXXI. *On the Measurement by Trigonometry of the Heights of the principal Hills in the Vicinity of Dent, Hawes, and Sedbergh, in Yorkshire. By* JOHN NIXON, *Esq.*

[Concluded from p. 95.]

THE vertical angles were measured by the horizon-sector, described in the Philosophical Magazine, vol. lix. p. 130.

At Noughtberry Hill the instrument stood upon a large square board screwed firmly to the tripod of the larger theodolite, and at Pen-y-gent it was placed upon the well-built wall crossing the summit of the hill; but at the other stations a flag firmly fixed upon the signal-tower reduced to an altitude of about four feet, formed the rude yet incomparable stand.

Formerly the minute error of adjustment of the levels, which was found to vary with the temperature, was ascertained when on the point of setting out for the station, and a register of the thermometer kept contemporary with the observations. To this plan two objections existed: the error of adjustment might have varied on the route; and the thermometer would rarely indicate the precise temperature of the levels, necessarily exposed on bright days during a pair of observations alternately to the sun and the shade of the instrument. Some time ago the original levels were replaced by two superior ones (by Dollond), with tubes so nearly cylindrical as to require little or no correction for temperature*. To avoid the other source of error, when the instrument had acquired at the station the temperature of the air, the telescope was placed within its Ys at a slight angle of *elevation*†. The left index being then *levelled* and the angle of elevation read off, the telescope was inverted, and the right index similarly levelled and read off. The telescope was subsequently taken out of the Ys, and reversed in position, when the indices were once more levelled and read off. With the levels correctly adjusted, the angle of inclination would be the same by the four readings; or if the sum of the two angles read off before the telescope was reversed, equalled that of the two succeeding measurements, a compensation of error would exist; otherwise one-fourth of their difference would give the mean error of the levels, additive to, or subtractive from angles of elevation, according as the second pair of readings exceeded or fell short of the first pair. Thus at Dod Fell the readings were:

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* I mean the upper surface of the one Mr. Robberds mentions, in speaking of the soil of the meadows and marshes; as "the lowest bed of their soil is sand in which marine shells are found."

Left index and level ...	3′ 30″; reversed ...	4′ 15″
Right	3 45	3 50
Sums	7 15	8 5
Difference	0 50	

Hence 12″·5 ($=\frac{50}{4}$) is the mean error of the levels additive to the elevations. The inclination of the telescope, as given by the left level, is 3′ 52″·5; by the right one, 3′ 47″·5; of which the minute difference may arise from errors of division and reading off. When the temperature had varied considerably in the course of the observations, the verification of the adjustment was repeated on their termination, and the mean of the two values registered as the proper correction.

In addition to this variable error is the constant one arising from an inequality in the size of the cylindrical rings; the one at the object end of the telescope exceeding the other by a quantity which affects the elevations with an error of at least

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* The bubbles are displaced one-eighth of an inch for a change of inclination of 5″, the angle to which the graduations (having a radius of about 15 inches) are read off.

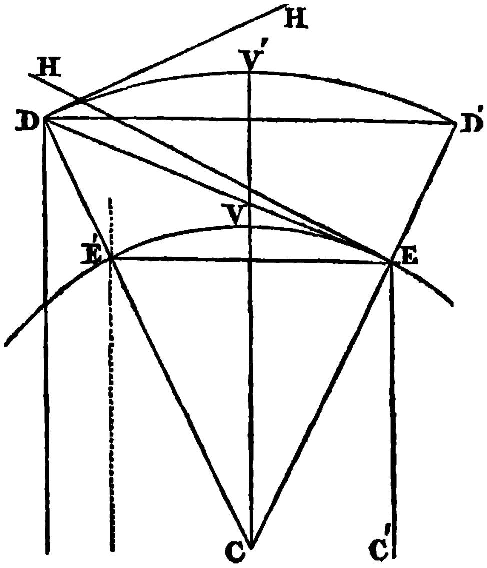
† Had there been no risk of mistaking an elevation for a depression in the readings with the telescope reversed, it might have been preferable to have placed the telescope in a *horizontal* position.

—11″.

$-11''$. As the observed refractions come out *negative* for terrestrial arcs not exceeding $5'$, it is to be suspected that this error, especially as it was determined by a method liable to some objection, may have been undervalued. In the beginning of the year, the graduated arcs (which had become bent from improper packing) were repaired, and the divisions renewed. At the same time, fine cross lines were substituted for the delicate filament extended diagonally from the original vertical wire to the horizontal one; an alteration which may account in part for the refractions alluded to appearing negative;—the horizontal line being probably depressed, in order to avoid the other extreme, some little *below* the summit of the observed hill.

With regard to the adjustment of the cross lines, although the line of collimation should in strictness be parallel to the plane of the graduated arcs, as well as to the axis of the tubes of the levels, it would nevertheless require a very gross error in this respect to affect elevations or depressions not exceeding 3°. As the sector is fitted up with two divided arcs (one on each side of the tube), each furnished with a moveable index carrying a spirit-level, the one used with the telescope erect, and the other with it inverted,—it becomes superfluous to render the line of collimation strictly parallel to the cylindrical rings; for the half sum of the readings by the two indices, when corrected for the error of the levels and cylindrical rings, is evidently the true angle. The transverse levels were adjusted by bringing their bubbles to the marks (by means of the nuts), whilst the planes of the arcs appeared to be parallel to a fine plumb-line suspended nearly in contact with them.

As a preface to the register of the observations it may be useful to point out the requisite corrections, and allude to the errors and uncertainties to which they are liable.



Admitting the earth to be a sphere, the difference of altitude or level of any two points on its surface ED will be equal to their difference of distance fom C the centre of the earth, where the vertical of D meets that of E. Through E, perpendicular to its vertical EC, draw the horizontal line EH; and perpendicular to DC, draw through D the corresponding horizontal line DH′. The angle formed at E or D by the meeting of a straight line passing through any point, and a horizontal line situated in the same vertical plane, is termed the elevation or depression of that point, according as it lies above or below the horizontal line: thus the angles HED, H′DE are both depressions. To find by trigonometry the difference of altitude of DE, no more data are required than the lineal distance EE′ or DD′, and the depressions HED, H′DE; or, simply, either of them, provided we have also given the terrestrial arc E′VE, or angle E′CE. Through C draw a line perpendicular to the chord of equal altitude or level E′E, which line, from the properties of the circle, will bisect the contained arc (at V). Make EC′ parallel to VC, whence VCE or half the arc will be equal to CEC′. As HEC and E′EC are both right angles, the horizontal line EH will be elevated above the chord of level E′E by the angle HEE′ = CEC′= half arc. It is also similarly demonstrable that the horizontal line DH′ must be elevated above the chord of level DD′ by the half arc DCV′=VCE, and that the angle D′DE, as the chords DD′, EE′ are parallel, will be equal to DEE′ the *angular* difference of level. The depression at the upper station will consequently *exceed* the angular difference of level by the half arc, and the elevation at the lower station will be equally in *defect;* whence the depression must exceed the elevation by the contained arc, and half their sum will be equal to the angle DEE′. When both angles are depressions, that at the upper station (or larger one) is the half arc *plus* DEE′, and the other is the half arc *minus* DEE′, so that their sum equals that of the half arcs, and half their difference the angle DEE′. With this angle, the distance EE′, and the angle E′DE (=90° plus half arc*), we get the side E′D or difference of altitude. When one only of the angles is given, the difference of the half arc and the *depression* (considered as an elevation when the former exceeds the latter), or the *elevation* increased by the half arc, will give the angular difference of level†.

Error of Collimation.—When the cylindrical rings of a telescopic-level are unequal in diameter, the line of collimation, supposed to be horizontal, describes, in a revolution in azimuth, the sides of a cone, erect or inverted, according as the line passes below or above the true horizon to which the axis of the cone will be perpendicular. This constant error might be ascertained, were there no refraction, by reciprocal observations of the elevations and depressions. Let $d\,e$ denote the true depression and elevation; $d'\,e'$, their values affected by the error of collimation y, and a the contained arc; then, as it has been shown that $(e + a) = d$, consequently $\frac{(e' + a) - d'}{2} = \frac{(e + a + y) - (d - y)}{2}$ will be equal to the error of collimation, to be *added* to the observed depressions, and $\frac{d' - (e' + a)}{2}$ the error subtractive. When both angles $d\,D$ are depressions, their sum, it has been demonstrated, will equal the contained arc; hence $\frac{(d' + D') - a}{2}$, or $\frac{a - (d' + D')}{2}$ will give the error of collimation; the depressions being observed in excess in the former case, and in defect in the latter.

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* In the calculations, E′DE has been considered as a right angle.

† In latitude 54° the log. of the mean value of the half arc in *seconds* may be found by subtracting the constant logarithm 2·308227 from the log. of the distance in feet.

When the line of gravity, disturbed by local attraction, is not in the direction of the centre of the earth, the line of collimation supposed to be horizontal, describes in its revolution a plane inclined to the true horizon at an angle equal to the deflection, and cutting it at right angles to the direction of the disturbing cause. The elevations must therefore be in defect in the direction of the attraction, and in excess in the opposite quarter of the horizon by a quantity equal to the inclination of the planes to each other; but in the direction of their intersection the error of collimation will be null. When the deflection (g) as measured on the vertical plane passing through two stations, is the same at both and in one direction (or parallel), the elevation and corresponding depression will be equally in excess or defect, and $\frac{(d + g) + (e + g)}{2}$ will exceed $\frac{d + e}{2}$ by g; but should the deflections be in opposite directions (or inclined to each other), a compensation of error takes place, $\frac{(d + g) + (e - g)}{2}$ being equal to $\frac{d + e}{2}$. Admitting the line of gravity to be disturbed at one station only, $\frac{(d + g) + e}{2}$ will differ from $\frac{d + e}{2}$ by $\frac{g}{2}$.

Refraction.—In the general state of the atmosphere the elevations may be considered as increased, and the depressions equally diminished by a constant ratio of the contained arc (a). In this case, if d and e represent, as before, the true depression and elevation, their values d' and e' as affected by the (positive) refraction r, will be $d-r$ and $e+r$. Hence $\frac{(e'+a)-d'}{2}$, or $\frac{a-(d'+D')}{2}$, will be the refraction*; but the angular difference of level may be obtained without previously ascertaining the refraction or having given the contained arc, DEE′ being equal to $\frac{e'+d'}{2}$, or to $\frac{d'-D'}{2}$.

When the instrument, from some imperfection in its construction, gives the elevations in excess or defect by an unknown but constant quantity y, then will e' be equal, in the former case, to $(e+y+r)$, and d' to $(d-y-r)$; but in the latter case we shall have $e'=(e-y+r)$ and $d'=(d+y-r)$; whence $\frac{d'+e'}{2}$, or $\frac{D'-d'}{2}$, will be the correct angular difference of level†, and $\frac{(a+e')\backsim d'}{2}$, or $\frac{a\backsim(d'+D')}{2}$, the refraction *plus*, or *minus* the constant error of the instrument, according as the elevations are in excess or defect‡. Were the refraction a constant ratio of the arc, its value, unaffected by the instrumental error, might be determined from reciprocal observation on arcs differing in extent. Let $(r \pm y)$ be the observed refraction for the arc a, and $(r' \pm y)$ that for the (much) greater arc a'; then $(r' \pm y)-(r \pm y)$ will be the true refraction for the arc $(a'-a)$.

Unfortunately for the accuracy of trigonometrical measurements, it will frequently occur, especially on low grounds with a cloudless sky in the spring, that a thin stratum of air in con-

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* When the observations are not reduced to the ground, the refraction, calling h the angle subtended by the *sum* of the heights of the eye at the two stations, will be $\frac{(h+a+e')-d'}{2}$, or $\frac{(h+a)-(d'+D')}{2}$.

† When the height of the eye is the same at both stations $\frac{d'+e'}{2}$, or $\frac{d'-D'}{2}$, will still give at once the angular difference of level l; for

$$\left.\begin{array}{ll} & l+\frac{a}{2}-r+y+\frac{h}{2}=d' \\ \text{and} & l-\frac{a}{2}+r-y-\frac{h}{2}=e' \end{array}\right\} \text{whence } d'+e'=2l;$$

$$\text{or} \quad \frac{a}{2}+r-y-\frac{h}{2}-l=D'; \text{ whence } d'-D'=2l.$$

‡ When y is known, and the observations are not reduced to the ground, the refraction will be equal to

$$\frac{(a+h+e'\pm 2y)-d'}{2}, \text{ or to } \frac{(a+h\pm 2y)-(d'+D')}{2}.$$

tact with the ground at the station is rarer in the heat of the day than the stratum immediately above it, and denser in the morning and evening, than comports with the natural constitution of the atmosphere. The refraction varies in consequence from $\frac{a}{n} + x$ in the morning to $\frac{a}{n} - x$, its amount about noon, and reverts in the evening to the first expression of its value. Hence the refraction on similar days is perpetually changing, and becomes a constant ratio of the arc $\left(\frac{a}{n}\right)$ only twice in the day; viz. in the middle of the forenoon and afternoon, when x or the maximum increment or decrement of the regular refraction resulting from the passage of the ray through the disturbed stratum of air, becomes 0. In such a condition of the atmosphere it would evidently be impossible to ascertain the correct angle for calculation from even *contemporary* reciprocal observations, unless x and its horary variations should have the same value at the same time at both stations.

On a careful consideration of the errors to which the determination of the angular difference of level is liable, we cannot but admit that the least objectionable method of measuring the difference of altitude of two hills is to make the requisite observations at an *intermediate* station. When the latter is equidistant from the former, so complete a compensation of errors (that arising from local attraction excepted) is effected, that it becomes superfluous to be acquainted with the error of collimation (from whatever cause arising), or even the refraction, although variable to a small amount, provided the two observations were nearly contemporary. But if we suppose the two hills AB, and the intermediate station S, to be in a line and equal in altitude, and admit the plumb-line to be drawn 10″ towards A, then shall we make A lower, and B higher than S by about $\frac{\text{tan. } 10'' \times \text{distance}}{\text{radius}}$, and B will be calculated to be higher than A by twice this quantity. As it may frequently be impracticable to have an equidistant station, we must select two (or more) places ST, so situated that the distances SA + TA may be nearly equal to SB + TB. In this latter case the error of collimation and the refraction should certainly be known approximatively; but great errors may be committed in this respect without materially affecting the *difference* of altitude of the two hills. When SA is more nearly equal to SB than TA is to TB, the difference of level is probably more correctly determined from S than from T, unless SA + SB should greatly exceed TA + TB. Hence the following correction:

Let f denote the reciprocal of $\left(\frac{SA+SB}{2}+SA\backsim SB\right)$, and g the reciprocal of $\left(\frac{TA+TB}{2}+TA\backsim TB\right)$: then if $l\,l'$ represent the difference of altitude calculated from the observations at S and T, the correct value may be considered as equal to $\frac{lf+l'g}{f+g}$. The heights of two or more hills having been similarly determined, it may be useful, in the event of being stationed on one of them, to observe the elevation or depression of the others, with a view to learn the actual refraction. On such occasions it is requisite to have given the instrumental and adjustment errors. Adopting this method, the following refractions were obtained:

	Arc.	Refn.
Gt. Whernside as observed from Pen-y-gent .	8′ 50″	+ 4″·5
———————————— Dod Fell . .	10 24	+25
———————————— Noughtberry	13 34	+28
Pendle Hill—————— Noughtberry	24 42	+ 1 ·30

The refractions deduced from the reciprocal observations are given below:

	Arc.	Refn.
Between Whaw Fell and Noughtberry	1′ 14″	+ 6″
Dod Fell and Noughtberry	3 10	+ 9
Whaw Fell and Whernside	3 21	− 18
Ingleborough and Whernside	3 41	− 9
Noughtberry and Whernside	4 6	+ 2
Ingleborough and Pen-y-gent	5 16	− 6
Dod Fell and Whernside	5 45	+ 4
Pen-y-gent and Whernside	6 54	+ 11
Noughtberry and Ingleborough . . .	7 14	+ 3·5
Dod Fell and Ingleborough	7 31	− 7·5
Noughtberry and Pen-y-gent	7 53	+ 16·5

Mean arc 5′·6; Mean refr. + 1″.

As the refraction resulting from observations made some years ago by the same instrument on numerous arcs, several of which were of considerable extent, appeared to be about 1-18th of the arc, the refraction adopted in the calculations, when not determinable from reciprocal observations, has been 1-18th *minus* the constant quantity 18″. In addition to the elevations being observed in defect after the substitution of the cross lines for the filament, two other causes may be assigned for the smallness of the refraction;—the ray, as the hills were lofty and differed little in elevation, would be confined to a stratum of rare air; and the observations were made (a little too early in the year) about the warmest part of the day, when the variable part of the refraction is at its *negative* maximum*. The refraction between Noughtberry Hill and the much lower hill Pendle, comes out, it is true, very considerable (1-16·5); but as the day on which the observations were made was of extraordinary clearness, which is generally attributed to the rate of decrement of temperature in the atmosphere being much less than wanted, the refraction would in consequence exceed its mean value.

The register of the observations will require explanation.—The *first* column gives the time at each observation; the *second*, the name of the hill of which the ground at the summit (Pen-y-gent excepted) was observed; the *third*, its elevation E or depression D as read off on the left index; the *fourth*, the half difference of the corresponding reading by the right index ± the constant error of the instrument and the variable one of the levels, forming together what is termed the error of collimation, additive to, or subtractive from the given reading according to its prefixed sign. (Were the graduations perfect and the adjustments of the cross lines and levels unaffected by variation of temperature, the error of collimation should be a constant quantity.) The *last* column contains the difference of level of the ground at the station (Pen-y-gent excepted) and the summit of the observed hill, the latter being higher or lower than the station according as the difference is marked H or L.

At Whaw Fell.

			Height of Eye 3·5 feet.			Feet.
Sept. 15,	11h	0m	Noughtberry Hill	2° 46′ 20″ E.	−19″	372·6H.
1824.	11	5	Lovely Seat	0 25 40	−11·5	377·1
	11	10	Ten End	0 12 5	− 9	82·4
	11	20	Bear's Head	0 20 0	−11·5	184·0
	11	30	Dod Fell	1 11 25	− 4	356·8
	11	40	Ingleborough	0 46 32	−17·5	537·9
	13	0	Ingleborough	0 46 30	−16·5	
Mar. 29,	15	25	Ingleborough	0 45 45	+16	
1825.	15	50	Ingleborough	0 45 50	+11	
	15	10	Bow Fell	0 45 30	+ 9·5	388·8
	15	40	Bow Fell	0 45 20	+21	
Sept. 15,	11	45	Shunnor Fell	0 35 30	− 9	513·7
1824.	12	50	Shunnor Fell	0 35 25	− 6·5	
	12	0	Whernside	1 35 20	−14	579·4
	12	10	Berkin	0 10 25	−16·5	165·6
	13	25	Pen-y-gent, Wall	0 34 40	−14	451·2

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* The mornings were generally slightly frosty.

		Height of Eye 3·5 feet.			Feet.
Sept. 15,	11^{h} 35^{m}	Blea Moor	0° 29′ 35″ D.	+ 6″·5	74·8 L.
1824.	15 30	Snays Fell	0 25 30	+11·5	51·5
Mar. 29,	15 15	Ryssell	0 4 20	−16	10·7
1825.					

As the *minute* error of the levels was unregistered, the greater part of the observations serve merely to verify those made at the other stations. The refraction, although the *filament* was made use of, it was found necessary to consider as 1-·18 *minus* 18″.

At Ingleborough.

		Height of Eye 4 feet.			Feet.
June 17,	10^{h} 30^{m}	Shunnor Fell	0° 7′ 10″ D.	+10″	27·2 L.
1822.	13 10	Lovely Seat	0 12 45	+15	162·8
		(Call the refraction for the above 1·18th.)			
July 2,		Colm	0 16 30	−21·5	122·6
1827.		Berkin	0 33 30	−22	376·5
		Noughtberry Hill	0 17 32	−22	170·5
		Dod Fell	0 18 28	−22	186·2
		Graygreth	0 49 2	−22·5	314·3
		Blea Moor	1 18 5	−25·5	614·9
		Pen-y-gent, Wall	0 13 35	−37	91·7
		Bow Fell (dim)	0 14 20	−37	148·4
		Whernside	0 3 20 E.	+22	41·2 H.

At Whernside.

		Height of Eye 4 feet.			Feet.
July 4,	12^{h} 55^{m}	Wildboar Fell	0° 10′ 15″ D.	− 2″	92·0 L.
1827.	13 0	The Calf	0 16 32·5	+ 6	196·9
	13 5	Bow Fell	0 22 40	+ 3·5	191·7
July 6.	14 10	Bow Fell	0 22 56	− 8	
	12 20	Colm	0 41 35	+ 1	163·3
July 4.	13 10	Colm	0 41 22·5	+12	
	13 50	Graygreth	1 10 35	+ 3·5	356·2
July 6.	12 30	Graygreth	1 11 10	−14	
	13 10	Shunnor	0 8 50	−12·5	64·2
	14 40	Shunnor	0 8 52	−10	
July 4.	13 45	Shunnor	0 8 30	+ 2	
	13 37	Noughtberry Hill	0 30 52·5	+14·5	209·0
July 6.	12 45	Noughtberry Hill	0 31 35	− 9	
	13 15	Lovely Seat	0 15 50	− 2·5	197·6
	12 40	Dod Fell	0 25 20	− 7·5	224·7
	12 0	Pen-y-gent, Wall	0 14 33	−15·5	131·0
	12 5	Ingleborough	0 9 7·5	−11	41·2
	12 35	Whaw Fell	1 40 27·5	− 4	582·2

At

At Noughtberry Hill.

Height of Eye 4 feet.

					Feet.
July 5,	15h 35m	Great Whernside	0° 2′ 5″ D.	− 1″	
1827.	15 45	Pendle Hill	0 19 48	−15	
	15 25	Dod Fell	0 5 5	− 4·5	16·1L.
	16 5	Berkin	0 20 50	0	205·7
	16 10	Whaw Fell	2 50 55	−15	371·6
	16 13	Blea Moor	1 38 45	+ 6	445·6
	15 0	The Calf	0 3 22·5	− 1	14·5H
	15 20	Lovely Seat	0 3 7·5	− 7·5	5·7
	15 4	Bow Fell	0 0 43 E.	+ 3·5	20·5
	15 8	Wildboar Fell	0 6 35	+13·5	119·6
	16 15	Wildboar Fell	0 6 52	+ 3	
	15 12	Cotter Fell	0 5 5	+ 1	122·4
	15 16	Shunnor Fell	0 9 6	+10	143·0
	15 40	Pen-y-gent, Wall	0 1 30	+ 1	76·6
	15 50	Ingleborough	0 9 20	+ 5	170·5
	15 55	Whernside	0 26 25	+ 6	209·0
	16 0	Colm	0 2 12·5	+ 6	48·4

At Pen-y-gent.

Height of Eye above the Wall 9 inches.

				″	Feet.
July 11,	11h 35m	Ingleborough	0° 6′ 52″E.	+ 8·5	91·7H.
1827.	14 50	Ingleborough	0 6 50	+ 7	
	11 55	Whernside	0 7 17	+ 5	131·0
	12 30	Wildboar Fell	0 4 52·5D.	− 6	47·6
	14 5	Wildboar Fell	0 5 5	−11	
	13 40	Cotter Fell	0 5 37·5	−21	47·8
	13 55	Shunnor Fell	0 3 12·5	−12	69·1
	11 50	Colm	0 6 20	− 4·5	30·0L.
	12 10	Bow Fell	0 8 12	−12	53·7
	12 16	Noughtberry Hill	0 9 6	+ 6	76·6
	12 35	Blea Moor	0 51 27·5	− 8	520·2
	12 45	The Calf	0 9 12·5	− 1	53·5
	14 55	The Calf	0 9 25	− 9·5	
	13 45	Lovely Seat	0 8 52·5	− 8	65·9
	14 35	Penhill (Wensleydale)	0 27 5	−13	
	14 30	Great Whernside	0 2 43	−11	
	14 40	Great Whernside	0 2 48	−15	

At Dod Fell.

Height of Eye 4 feet.

					Feet.
Aug. 25,	15h 35m	Great Whernside	0° 1′ 15″ E.	+16″	
1827.	13 40	Wildboar Fell	0 3 55	+20	139·0H.
	13 55	Cotter Fell	0 3 40	+20	142·6
	14 5	Shunnor Fell	0 9 10	+22	162·1
	14 25	Noughtberry Hill	0 0 20	+22·5	16·1
	14 40	Colm	0 0 40	+18·5	66·4
	14 45	Whernside	0 18 28	+20·5	224·7
	15 5	Ingleborough	0 9 15	+26	186·2
	15 20	Pen-y-gent, Wall	0 5 15	+23·5	95·0
	14 15	Lovely Seat	0 1 25 D.	−23·5	24·9
	14 20	Bow Fell	0 0 35	−22	39·5
	14 35	Berkin	0 16 7	−20·5	186·2L.
	15 10	Blea Moor	1 6 40	−21	429·2
	15 30	Penhill (Wensleydale)	0 25 0	−16	

Calculation of the mean *differences of Level of the Stations.*

Whernside above Ingleborough.

	Feet.
By reciprocal observation	41·2
By obs. from Pen-y-gent	39·3
———— Noughtberry	38·5
———— Dod	38·5

Ar. mean 39·4; cor. mean 39·8

Ingleborough above Pen-y-gent, Wall.

By reciprocal obs.	91·7
By obs. from Whernside	89·8
———— Noughtberry	93·9
———— Dod	91·2

Arithm. and corr. mean 91·7

Ingleborough above Noughtberry Hill.

By reciprocal obs.	170·5
By obs. from Whernside	167·8
———— Pen-y-gent	168·3
———— Dod Fell	170·1

Ar. mean 169·2; cor. mean 168·9

Ingleborough above Dod Fell.

	Feet.
By reciprocal obs.	186·2
By obs. from Whernside	183·5
———— Noughtberry	186·6
———— Pen-y-gent	186·7

Ar. mean 185·8; cor. mean 185·7

Whernside above Pen-y-gent.

By reciprocal obs.	131·0
By obs. from Inglebro'.	132·9
———— Dod Fell	129·7
———— Noughtberry	132·4

Ar. mean 131·5; cor. mean 131·4

Whernside above Noughtberry Hill.

By reciprocal obs.	209·0
By obs. from Inglebro'	211·7
———— Pen-y-gent	207·6
———— Dod Fell	208·6

Ar. mean 209·2; cor. mean 209·1

Whernside above Dod Fell.

	Feet.
By reciprocal observation	224·7
By obs. from Inglebro'	227·4
——— Noughtberry	225·1
——— Pen-y-gent	226·0

Ar. mean 225·8; cor. mean 225·6

Pen-y-gent, Wall, above Dod Fell.

By obs. from Dod Fell	95·0
——— Ingleborough	94·5
——— Whernside	93·7
——— Noughtberry	92·7

Ar. mean 94·0; cor. mean 94·1

Noughtberry Hill above Dod Fell.

	Feet.
By reciprocal observation	16·1
By obs. from Ingleborough	15·7
——— Whernside	15·7
——— Pen-y-gent	18·4

Ar. mean 16·5; cor. mean 16·3

Heights of the Stations above the Irish Sea at low water.

	Feet.	Feet.
Ingleborough*	2374·6	
Whernside	2414·4	
Pen-y-gent, Wall	2283·0	(ground 2277·0).
Noughtberry Hill	2205·5	
Dod Fell†	2188·9	

Calculation of the Height of Whaw Fell.

	Feet.	Feet.
By obs. from Noughtberry Hill	1833·9	
——— Whernside	1832·2	
	———	Cor. mean 1833·6
By obs. at Whaw Fell of Whernside	1835·0	
——— Inglebro'	1836·7	
——— Pent-y-gent	1831·8	
——— Noughtberry	1832·9	
——— Dod Fell	1832·1	
	———	Cor. mean 1833·4
Height of Whaw Fell		1833·5

In the following table are given for each hill its height as determined from the stations at the head of the column;—the claims to accuracy of the different values of the altitude being considered in the calculation of the *mean* to be *reciprocally* as

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[*] See Phil. Mag. for 1823.

[†] Observations made some years from hills chiefly in Wharfdale, gave, Whernside 2412; Pen-y-gent, Wall 2281·4; Dod Fell 2189·8 feet.

the distance of the station to the hill. The last column contains the heights deduced from the observations at Whaw Fell.

Hills.	Height by Observation from Whernside.	Ingleborough.	Pen-y-gent.	Noughtberry.	Dod Fell.	Corrected Mean Height.	Height by Observations from Whaw Fell.
	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
Shunnor Fell	2350·2	2347·4	2352·1	2348·5	2351·0	2349·8	2347·2
Cotter Fell ...	......	......	2330·8	2327·9	2331·5	2329·7	......
Wildboar Fell	2322·4	......	2330·6	2325·1	2329·0	2326·3	......
Colm..........	2251·1	2252·0	2253·0	2253·9	2255·3	2252·4	......
Bow Fell......	2222·7	2226·2	2229.3	2226·0	2228·4	2226·1	2222·3
The Calf......	2217·5	......	2229·6	2220·0	......	2221·3	......
Lovely Seat...	2216·8	2211·8	2217·1	2211·2	2213·8	2214·0	2210·6
Graygreth ...	2058·2	2060·3	......	......	......	2059·2	......
Bear's Head	......	......	......	......	......	......	2017·5
Berkin.........	......	1998·1	......	1999·8	2002·7	2000·0	1999·1
Ten End	......	......	......	......	......	......	1915·9
Ryssell.........	......	......	......	......	......	......	1822·8
Snays Fell ...	......	......	......	......	......	......	1782·0
Blea Moor ...	......	1759·7	1762·8	1759·9	1759·7	1760·3	1758·7
Mean error...	−1·5	−0·9	+3·2	−0·8	+1·6		−2·7
Mean distance	43,892	49,632	73,462	36,051	44,745		32,930

At the Bear's Head the apparent elevation of Dod Fell, measured by the 4-inch theodolite, was 43′ 18″, whence the altitude of the former would be 2021 feet. At Ten End the Bear's Head was elevated 40′ 9″, giving 1911 feet for the height of Ten End. The heights of Ryssell and Snays Fell are as yet unconfirmed.

When the mean error of a number of heights determined from any particular station is considerable, it would appear to argue that the altitude of the station had been assumed in excess or defect according to the *sign* and by the *amount* of the error. But if the mean distance of the several hills to the station should greatly exceed the mean of their distances to the other stations, the error, as in the case of Pen-y-gent, may with greater probability be attributed to a false estimate of the refraction. Had we made use of $\frac{a}{18}$ *minus* a smaller quantity than 18″, the discrepancies would have disappeared.

On another occasion I shall beg to add the results of a few barometrical measurements, and furnish some geological notices.

Leeds, Dec. 26, 1827. JOHN NIXON.

XXXII. *On an Apparent Violation of the Law of Continuity.*
By P. M. ROGET, *M.D. F.R.S.*[*]

[Concluded from p. 121.]

THE severest test to which the validity of this mode of reasoning can be put, is to apply it to the purely mathematical relations of space and quantity, to which, if the law of continuity have any necessary existence in the nature of things, it ought to apply with the greatest rigour. We find, accordingly, that all the changes of magnitude in those quantities of which the value is dependent on that of certain other quantities, accompany corresponding changes in these latter quantities in a manner strictly conformable with the law of continuity. The same exact correspondence also obtains with regard to the geometric relations of space, such as lines, angles, surfaces and volumes. Thus, all the trigonometrical lines which are functions of the arcs of circles, undergo regular and continuous changes, corresponding to the regular and continuous increase or diminution of the arcs to which they are related. Some, as the sine and cosine, pass from every intermediate magnitude between nothing and a certain limit, which they do not exceed, but to which, when the arc increases uniformly, their last approximations are made with extreme slowness. Arrived at that limit, they again begin to decrease; but at first with extreme slowness, like bodies that are set in motion, from a state of rest, by the action of a moving force. Their progress of diminution is by degrees accelerated; and is again retarded only when they are preparing, as it were, to undergo some other change. By the time they arrive at any of their states, either of maximum or minimum, their rate or velocities of increase or diminution have gradually been extinguished, and reduced to nothing. Some of these lines, as the tangent for example, have no limits to their increase of magnitude, and pass through every possible linear dimension from nothing to infinity, while estimated in one certain direction, characterized by the positive sign. From positive infinity, the transition is made at once to negative infinity, and the subsequent changes consequent upon the further enlargement of the arc, consist in the gradual diminution of the negative tangent; that is, of the line estimated in a direction opposite to that which was assumed as the measure of the original, or positive tangent. This negative tangent passes again, by the continuation of the same process, into the primitive tangent, after having been again reduced to nothing.

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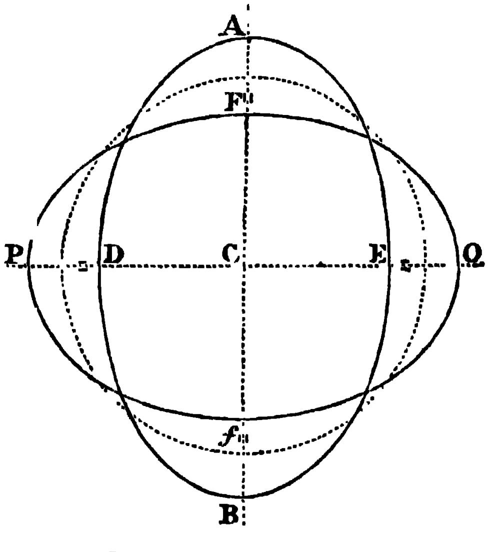
[*] The former portion of this article was inadvertently reprinted in our last Number, without having been previously submitted to the author; in consequence of which, several inaccuracies in the original (now noticed in p. 206,) were left uncorrected; and the division having been made in an improper place, the last paragraph of the former portion is here repeated.—EDIT.

By considering, in like manner, the generation and the course of curves of every kind, whether produced by the intersection of lines, moving according to certain laws, or of points whose motions are limited to certain conditions with relation to other lines or curves, we shall find that the same law of continuity obtains, both with respect to the changes of magnitude and the changes of direction. No mathematical quantity changes from positive to negative, or from negative to positive, without either becoming infinite, or being reduced to nothing; and it passes through every possible intermediate degree of magnitude, in its progress from the one to the other. The change from increase to diminution, or the reverse, is never abruptly made; but is always effected by a transition through a condition of momentary quiescence, which is the limit of opposite kinds of changes, and at which instant no actual change takes place.

Such being the universal law which is observed by lines generated according to any given geometric conditions, we should naturally be led, from analogy, to expect that the motions of points, having certain geometric relations to given curves, would also be governed by the same law. Let us take, for example, a small portion of any curve. This portion will have, what is termed, a certain radius of curvature, and the furthest extremity of this radius will be the centre of curvature. In proportion as the curvature is diminished, this radius will increase in length; and the centre of curvature will be removed to a greater distance. Its motion during this gradual change will be along a right line, perpendicular to the tangent of the curve at the point of contact. As the curve in the change we are supposing it to undergo, approaches more and more to a straight line, the centre will recede with great rapidity; till at length, when all curvature is lost, it may be regarded as removed to an infinite distance; and at this precise moment it is indifferent whether we consider it as existing on the one side or the other of the tangent, with which the curve has now coalesced. But we may suppose the same power, whatever it be, which has effected this gradual change of curvature, to continue its operation still further. A curvature in the opposite direction will now take place, and the centre of this curvature will be found on the opposite side to that in which it had at first been situated. Its motion, in proportion as the curve continues to follow the same uniform progress of change, will approach the line from a distance infinitely negative. All these successive changes of position in the centre of curvature are perfectly similar to those of the extremity of the tangent, during the increase of the arc of a circle from nothing to the entire circumference of the circle; and are, in both cases, perfectly accordant with the law of geometric continuity.

A case, however, has occurred to me, in which this apparently universal and necessary law seems to be directly violated. Let the ellipse ADBE be the curve which is to be subjected to the operation of the cause, tending, as in the former instance, to diminish its curvature; and let us trace the successive changes which will thereby be occasioned in the position of the two foci, F, *f*. A compressing force, for example, is applied in the direction of the transverse diameter, AC, BC; or, what will produce the same effect, a stretching force is applied in the direction of the conjugate, or shortest diameter, CD, CE. By the action of either of these forces, the form of the ellipse will be brought nearer to that of a circle, till it at length becomes a perfect circle, represented by the dotted circumference. But it assumes this form only for an instant, being merely the passage to another series of ellipses, which, taking their rise from this circle, at first differ but insensibly from it; but afterwards gradually assume every possible degree of compression, until they are ultimately reduced to a straight line at right angles with the transverse diameter of the first set of ellipses. The former represent circles that are elongated, the latter circles that are compressed in



in one particular direction; the two pass into each other by a continuous series, of which the perfect circle is merely a term of transition.

Let us now attend to the changes of position in the foci, corresponding to these transitions. During the gradual change of the first ellipse into the circle, the foci F, *f*, move along the diameter AB, approach nearer and nearer to the centre of the figure C, until they both coalesce in the centre. In the next series of changes, we find them again separating from each other; but instead of moving in the same line as before, we observe them starting off, each in a new direction, CP and CO, precisely at right angles to their former lines of motion, AC and BC; and this change of direction is made abruptly, *per saltum*, and by an angular motion. Are not these changes direct violations of the law of continuity?

As the question which I have now proposed may be thought deserving the attention of such of your philosophical readers as are stimulated by the appearance of paradox to renewed investigation; and as the attempts to solve it may give occasion to the exercise of ingenuity, I shall abstain from suggesting the considerations which might, perhaps, enable us to explain this apparent anomaly, consistently with the universality of the law of continuity.

Bernard-street, Russell-square,
November 1, 1825.

P. M. ROGET.

Errata in the last Number.

Page 118 line 23, *for* "operations," *read* "operation."
—— 118 — 23, *for* "greatest," *read* "gentlest."
—— 119 — 3, *for* "to," *read* "with."
—— 119 — 32, *for* "rectilineous," *read* "rectilineal."
—— 120 — 26, *for* "course pending," *read* "corresponding."

XXXIII. *Additional Discussion respecting the Ellipticity of the Earth as determined by Experiments made with the Pendulum.* By J. IVORY, *Esq. M.A. F.R.S.*[*]

IN deducing the figure of the earth from the observed lengths of the pendulum, I have always thought it necessary to leave out a few of the experiments that were inconsistent with the rest. The inconsistency is proved by comparing the pendulums on the same parallel, or nearly on the same parallel; in which latter case a small correction must be made for the difference of latitude. It is evident that, if the pendulums when thus compared are excessively irregular, we never can deduce from them, by any mode of investigation whatever, any conclusion respecting the figure of the earth, in which much confidence can be placed. On this ground I always rejected such of the experiments as were irreconcileable with the rest; and I thought this proceeding the more justifiable, both because the experiments set aside were always very few in comparison of the whole number, and because those that remained were brought within the compass of inequality to be expected in experimental quantities. The results obtained on this principle, are already before the public, and therefore need not be particularly mentioned. My present intention is to solve the problem, by taking in all the 40 experiments we at present possess, in the expectation, that by this means some light will be thrown on this subject, although my former mode of solution still appears to be founded on sound principles.

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[*] Communicated by the Author.

In order to abridge, I shall begin with laying down the general formulas of the mode of solution I follow. Put $39+\delta$ for the pendulum in English inches at any station; λ, for the latitude; $39+\Delta$, for the equatorial pendulum; and f, for the difference between the pendulums at the pole and the equator: then

$$\Delta + f \sin^2 \lambda = \delta.$$

Further, let

$$G = \Sigma\,(\delta),$$
$$H = \Sigma\,(\sin^2 \lambda),$$
$$K = \Sigma\,(\sin^4 \lambda),$$

the sums being extended to all the stations of which the number is n: then, by taking the sum of all the like equations at the n stations, we get,

$$\Delta + \frac{H}{n} f = \frac{G}{n}. \qquad \text{(A)}$$

Next, a being an approximate value of Δ, find an approximate value of f, namely b, by substituting a for Δ in Equat. (A); or, if b be an approximate value of f, find, by means of the same equation, a corresponding value of Δ, namely a: then, if we put,

$$\Delta = a + s$$
$$f = b + \tau,$$

we shall have,

$$s + \frac{H}{n} \tau = 0. \qquad \text{(B)}$$

Again: Multiply all the terms of the first equation by the coefficient of f: then

$$\Delta \sin^2 \lambda + f \sin^4 \lambda = \delta \sin^2 \lambda;$$

and, by substituting the values of Δ and f,

$$s \sin^2 \lambda + \tau \sin^4 \lambda = \sin^2 \lambda\,(\delta - a - b \sin^2 \lambda);$$

and, by adding the like equations at all the stations,

$$\mathrm{H}s + \mathrm{K}\tau = \Sigma\left(\sin^2\lambda\,(\delta - a - b\sin^2\lambda)\right);$$

and, by exterminating s,

$$\left(\mathrm{K} - \frac{\mathrm{H}^2}{n}\right)\tau = \Sigma\left(\sin^2\lambda\,(\delta - a - b\sin^2\lambda)\right). \qquad (\mathrm{C})$$

If $\sin^2\lambda$ had exactly the same value at all the stations, the coefficient of τ in this last formula would be evanescent. In reality this can never be the case: but if all the stations were in higher latitudes than 45°, the coefficient would be so small that a minute variation in the sum on the right-hand side, would produce a great alteration in the values of s and τ; and, considering the uncertainty of the experimental errors, the problem would be in a great degree indeterminate. Were we in possession of no other experiments than those north of Formentera, the figure of the earth would be very ill determined; because all the pendulums might be sufficiently well represented at the same time that the equatorial pendulum is made to undergo considerable variations. The practical solution of this problem requires many accurate experiments between the tropics for finding the exact length of the equatorial pendulum; and likewise many experiments from the tropics to the poles for ascertaining the value of the other element f with the requisite precision.

Now taking all the 40 experiments in the table of my former paper, I have found,

$$\mathrm{G} = 3{\cdot}91529$$
$$\mathrm{H} = 16{\cdot}25069$$
$$\mathrm{K} = 10{\cdot}47158:$$

hence, n being 40, by equat. (A)

$$\Delta + {\cdot}40627 f = {\cdot}09788.$$

In order to find an approximate value of Δ, the six anomalous experiments must be reduced to the equator on the same supposition as before; namely, $f = {\cdot}205$: Then

	Δ
Galapagos	·01717
Ascension	·01995
St. Thomas	·02073
Guam	·01912
Mowi	·02136
Isle of France	·02277
Sum	·12110
9 equatorial values of Δ in my former paper	·11967
	15) ·24077 (·01605 mean

By substituting ·01605 for Δ in the foregoing equation, we get $f = \cdot 2014$. We now have,

$$a = \cdot 01605, \qquad \Delta = a + s,$$
$$b = \cdot 2014, \qquad f = b + \tau:$$

and the equations (B) and (C) will give us,

$$s + \cdot 40627\,\tau = 0$$
$$3\cdot 869\,\tau = \left(\sin^2\lambda\,(\delta - a - b\sin^2\lambda)\right).$$

Computing now the sum on the right-hand side of the last equation, which sum consists of 40 terms, we shall find,

$$3\cdot 869\,\tau = +\cdot 00307;$$

wherefore, $\tau = +\cdot 00079$; $s = -\cdot 00032$;
$f = \cdot 2022$; $\Delta = \cdot 01573$:

And we hence obtain this general formula for the length of the pendulum, viz. $l = 39\cdot 01573 + \cdot 2022 \sin^2\lambda$,

$$\text{ellipticity} = \cdot 00865 - \frac{\cdot 2022}{39\cdot 01573} = \cdot 00346 = \frac{1}{289}.$$

This result coincides almost exactly with what Captain Sabine has deduced from his experiments. We have next to examine, how the formula agrees with the phænomena. Beginning with the six anomalous stations, there is still an excess of gravity at them all, as will appear by the following errors which must be applied to the observed pendulums to make them equal to the calculated lengths; viz.

Galapagos	−·00144	Guam	−·00355
St. Thomas	−·00500	Mowi	−·00599
Ascension	−·00427	Isle of France	−·00746.

The error at Galapagos is now very small, and, in all probability, smaller than the actual error of observation. At the other five stations the excesses of gravity are still great; and, with respect to the like errors in my table, they appear to be reduced nearly in the proportion of 3 to 4. But if some advantage be gained at the anomalous stations, there is a disadvantage at all the other stations within 50° of the equator on either side. Within these limits all the observed pendulums are uniformly short of the calculated ones, except at Port Jackson and Jamaica, where there is a very small difference of an opposite kind, which may be neglected; and even, with respect to these two stations, there is great reason to think that the observed pendulums are both too long. Nor are the deficiencies trifling, as will appear by the following list of errors, which must be applied to the observed pendulums to make them equal to the calculated lengths:

	errors.		errors,
Paramatta .	+ ·00212	Madras	+ ·00269
Rio Janeiro	+ ·00266	San Blas ..	+ ·00418
Bahia.....	+ ·00161	Figeac	+ ·00224
Maranham .	+ ·00399	Bourdeaux	+ ·00323
Rawak	+ ·00094	Clermont ..	+ ·00148
Trinidad ..	+ ·00376	Paris	+ ·00103

About 50° of latitude all the formulas very nearly agree with one another, and with observation. In receding more from the equator, the pendulums are shorter in the formula we are considering, than in the others; but the effect of this is not very perceptible except at the highest latitudes. At Port Bowen and Spitzbergen, the errors are −·00239 and −·00302.

It appears then that, by lengthening the equatorial pendulum, we approach a little nearer to the anomalous experiments, and recede from all the rest within 50° of the equator. The foregoing investigation proves that all the 40 observed pendulums can never be represented, without great discrepancies, by any one ellipticity. In my last formula all the pendulums, except the five anomalous ones, are placed on one and the same surface within the limits of the errors of observation. But no one pendulum within 50° of the equator answers to its proper place in the surface of which the ellipticity is $\frac{1}{289}$. This surface is merely an imaginary mean, the offspring of calculation. It can have nothing to do with what is called local attraction: for it makes an excess of gravity at five particular stations, and, as far as experience enables us to pronounce, a defect of gravity every where else within the limits mentioned.

Feb. 14. J. IVORY.

XXXIV. *Failure of* Lagrange's *Method of integrating Linear with Constant Coefficients*[*]. *By* JOHN HERAPATH, *Esq.*[†]

IF we suppose the roots r, r_1, r_2 of the equation

$$r^3 + Ar^2 + Br + C = 0$$

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[*] It should have been mentioned in my last paper, that the constants in (18) are not precisely the same as in (17), except the first two. In the other terms several factors in the denominators were suppressed and understood in the constants, for brevity and ease of printing.

[†] Communicated by the Author.

to be all equal, the three equations of condition, p. 23, which are asserted, p. 96, to give the "complete integral" of

$$\frac{d^3y}{dx^3} + \mathrm{A}\frac{d^2y}{dx^2} + \mathrm{B}\frac{dy}{dx} + \mathrm{C}y = \mathrm{X} \qquad \text{(a)}$$

by Lagrange's method will become

$$\begin{aligned} r^2e^{rx}\{dp + dp_1 + dp_2\} &= \mathrm{X} \\ dp + dp_1 + dp_2 &= 0 \\ dp + dp_1 + dp_2 &= 0, \end{aligned}$$

giving obviously X = 0. That is, the equations of condition, which are said to furnish the "complete integral" of (a), and which therefore ought to give all cases of the integral without limiting the value of X, fix in the above instance the value of this indefinite function to zero.

Any candid mind would not need a more decided proof of absurdity in the principles of Lagrange's method than the above; but as some may think it dealing a little too summarily with a process which has obtained so much celebrity, and may prefer an instance of failure in result to a theoretical exposition of absurdity in principle, I submit the following, taken not from any investigation of mine, but copied from a professed advocate of Lagrange in the last Number of this volume. We are informed, p. 96, that the "complete integral" of (a) is

$$\text{“}\; y = \frac{e^{rx}\{c + \int \mathrm{X}e^{-rx}dx\}}{r-r_1 \,.\, r-r_2}$$

$$+ \frac{e^{r_1x}\{c_1 + \int \mathrm{X}e^{-r_1x}dx\}}{r-r_1 \,.\, r_1-r_2} \qquad \text{(b)}$$

$$+ \frac{e^{r_2x}\{c_2 + \int \mathrm{X}e^{-r_2x}dx\}}{r-r_2 \,.\, r_1-r_2}\,.\text{”}$$

Now this being the "complete integral," must give the value of y whatever values be assigned to r, r_1, r_2 and X. Putting for simplicity X = 0, and then supposing $r = r_1$ the above equation becomes

$$y = c^{rx}\frac{c-c_1}{0\times(r-r_2)} + \frac{c_2e^{r_2x}}{(r-r_2)^2};$$

that is, the value of y is infinite unless the arbitrary constants c, c_1 are equal, or certain functions of r, r_1. But c, c_1 being in the strict sense of the word "arbitrary constants," are not necessarily functions of x or r, r_1; and therefore the infinite value of y remains. We however know from other principles that y may be finite.

Lest it be supposed that the failure of Lagrange's method may be remedied by putting the pretended "complete integral" under the form of our (17), so as to take away the zero divisors, and thus bring out the complete solution: I shall now examine this point. For the sake of simplicity I will take an equation of the second order only, the lowest to which Lagrange's method can be fairly applied; and which from its having but two roots, and therefore but one difference between them, is peculiarly adapted to exhibit perspicuously the justice or injustice of our observations.

If r, r_1 be the roots of the equivalent algebraic equation, the "complete integral" is

$$y = \frac{c e^{rx} - c_1 e^{r_1 x} + e^{rx} \int X e^{-rx} dx - e^{r_1 x} \int X e^{-r_1 x} dx}{r - r_1}.$$

And if we put $X = 0$

$$y = \frac{c e^{rx} - c_1 e^{r_1 x}}{r - r_1} = c e^{r_1 x} \int e^{(r - r_1)x} dx + c_1 e^{rx} \int e^{(r_1 - r)x} dx$$

must be the "complete integral" of

$$\frac{d^2 y}{dx^2} + A \frac{dy}{dx} + By = 0.$$

But when $r = r_1$ the latter expression for the "complete integral" is

$$y = (c + c_1) x e^{rx}$$

which is evidently not the "complete integral," were it only that $c + c_1$ makes but one arbitrary constant. The integral by our formula (17) is

$$y = \{c_1 + cx\} e^{rx}$$

in which two arbitrary constants appear.

J. HERAPATH.

P.S. Since writing the above, I have examined with more attention Lagrange's method, and I find that the solution (b), which is professed to be taken from Lacroix's great work, as the "complete integral" by Lagrange's method, is not strictly the integral which this method gives. The result of Lagrange's contains less of absurdity than the above, but nevertheless fails to give the complete solution. I have not found where Lagrange published his method, nor what examples of it he gave. If that of Lacroix be one, it is a curious circumstance that he should fail both in method and deduction.

XXXV. *On Systems and Methods in Natural History.* By J. E. Bicheno, *Esq., F.R.S., Sec. L.S., &c.**

I PROPOSE to myself on the present occasion to make some observations on Systems and Methods in Natural History; a subject of great importance at all times, but more especially so at present, when new views of arrangement and nomenclature are proposed, and to some extent adopted. Let me not be understood, however, in the general observations which follow, to be opposed to any particular system; my object being to discuss the first principles of arrangement, and to leave others to judge how far they are applicable to the views adopted by any individual systematist.

It has appeared to me that the difficulties of the subject have not been duly appreciated; and the time cannot be unprofitably occupied, if I accomplish no more than to enable us to estimate them. It might even be suspected, from the readiness with which new systems are adopted, that they have a peculiar attraction for ardent minds; as it has not unfrequently happened that young naturalists have found themselves prematurely embarrassed in a subject, which of all others requires not only an extensive acquaintance with the operations of the human mind, but long experience and various practice. The line of argument I propose to employ, must necessarily be somewhat abstract; yet I hope I shall be borne with, since the practical naturalist could make no accumulations to his science, and all his particulars would stand unconnected and discrepant throughout, without the aid of abstract reasoning. Besides, I am anxious to engage the attention of persons accustomed to turn their observations to the operations of the human mind, and to the instruments which it employs to perform its labours; feeling assured that, by obtaining the cooperation of this class of philosophers, we shall have great light thrown upon our subject; and that it will be one means of attracting the notice of those who delight in a large and liberal treatment of science. While they impart to us a philosophical solidity, in which I am apprehensive we are wanting, we may hope to communicate to them a reciprocal benefit, in some of those graces and charms to be derived from the study of Nature, and in which perhaps they may be deficient.

Without undervaluing the study of species, upon which a great deal of our knowledge is built, it cannot be denied that naturalists in general have been too often content with assigning them names, and a place in the systems they have adopted;

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* From the Transactions of the Linnean Society, vol. xv. Part ii. p. 471.

and this they have done without having an ulterior view to their structure and functions, and the relations subsisting amongst them. Much less have they kept in view the end of generalizing the particulars they are accumulating; but they continue to heap together a "*rudis indigestaque moles,*" until they are actually overwhelmed by their materials. To build up science skilfully, the combination should go on with the collecting, or the superstructure will exhibit neither use nor beauty.

Mr. Roscoe has clearly illustrated, the comparative merits of the artificial and natural arrangements in Botany in a former volume of the Transactions*; and has satisfactorily proved, in my estimation, that however admirable and comprehensive the system of Jussieu may be, yet it ought not to supersede the use of the Linnæan arrangement. The two great masters of botanical science propose different ends, and ought not to be regarded as rivals. The President of this Society has also constantly pressed upon the attention of the student the same important fact.

In some respects it is not to be regretted that the absolute sway which the name of Linnæus has had among English naturalists is somewhat abated: for although authority is an extremely useful bond of union, and has in this instance established among us a nomenclature which nothing short of homage to the founder could probably have made current, yet it has brought with it the ordinary evils attendant upon great names. The range of the pupil has been limited by that of the master; and it has been considered a species of heterodoxy to dissent from the established opinions. The danger to be now apprehended is, that those who adopt other arrangements will forget the advantages to be derived from what is old, in their love of that which is new.

In addition to the remarks made by Mr. Roscoe and the President, I would beg leave to suggest to those who adopt new systems,—and in adopting them think it advisable to break up the old orders and genera into many new ones,—that the artificial and natural systems aim at two very distinct objects, which are in some measure incompatible with each other. The one is to make us acquainted with individuals: and the other, founded upon an acquaintance with individuals, to combine them according to their characters, so as to abridge the labour of reasoning, and to enable us to ascend from particular to general truths.

In order to assist us in these investigations, we employ certain words in a peculiar sense. Thus the word *Species*, when used by naturalists, has a more confined signification than the same word, when employed in scholastic language. We have agreed that a species shall be that distinct form originally so created, and producing by certain laws of generation others like itself: whereas all that logicians have meant, is a number of objects bearing a certain resemblance to one another, and on that account denominated by a single appellation, which may be employed to express any one of them. This term is the creature of art, to help us up the first step of generalization. By its assistance we propose to reason upon all the individuals conforming to the law we have laid down, as safely as we can do of any one of them. There is this inconvenience attending the use of it by naturalists, that it assumes as a fact, that which in the present state of science is in many cases a fit subject of inquiry; namely, that species, according to our definition, do exist throughout nature. It is too convenient a term to be dispensed with, even as an assumption; only care should be taken that we do not accept the abstract term for the fact.

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* Trans. Linn. Soc. vol. xi. p. 50.

It might, for instance, be proposed as a legitimate question, whether the species of some familiar genera, such as *Rosa*, *Rubus*, *Saxifraga*, do not run into one another by imperceptible shades, unappreciable by human sense, in the same manner as certain genera melt and intermingle their characters, so as to render it impossible to circumscribe them. Indeed, the extent to which species-making has been carried in modern times, almost leads to this conclusion. Visible and palpable distinctions are in many cases no longer relied on; and there are many acute naturalists, who, without bringing the subject to the test of experiment, are content to rely on those empirical characters, which can only be perceived by long and familiar experience, and cannot be described by words. The truth is, that all sensible objects have characters which leave impressions upon the mind, without our being capable of embodying them in language. We are all aware of this when we speak of tastes, and tints, and the countenances of our friends. Every-body perceives them, yet nobody can communicate to his neighbour his perception of their differences. Thus botanists speak of certain species of plants differing in appearance, habit, touch, &c.; by which they often mean that they have some indescribable peculiarities about them, which point them out to the practised observer as distinct. A great number of such species may be detected in every modern Flora of a well investigated country; but whether they deserve to be ranked among those which are capable of definition, is a question of great doubt:—that the practice is an inconvenience, none will deny; and if it be much longer continued, will involve in inextricable difficulty all our well known species, make us dependent upon empirical and traditional evidence for our acquaintance with them, and render it impossible to derive instruction from books. In such cases the assumed law ought to be brought to the test of experiment, or the species should be rejected.

Many of our cultivated plants also tend to invalidate the law. Who can refer our *cerealia* and esculent vegetables, in many instances, to their true types? and how few of our old flowers are there, of which the astutest botanist can trace the origin! Domesticated animals afford a still more striking example; and man himself furnishes the most difficult problem of all.

These remarks and examples are, I apprehend, sufficient to show how difficult it is to adopt the term in its strict acceptation; and that however precisely the naturalist has attempted to employ it, he has not succeeded to the extent he has proposed; and that it can only be taken as correct in a vague and general sense, and as a convenient abstraction to relieve him at the first step from the necessity of becoming acquainted with every individual.

The next term of importance to the naturalist upon which the accuracy of his reasoning depends, is that division of his system which he denominates a *Genus*. This is an assemblage of individuals agreeing also in some common characters; but, unlike the word species, it is not previously defined. Thus much indeed has been thought requisite; that in botany these common characters should be taken from the parts of fructification, and in zoology from such parts as are indicative of structure and habits. "A genus should furnish a character, not a character form a genus." We are not here, as in the word species, precluded from inquiry by a previous definition. Though both words are terms of generalization, there is the same difference between them, as instruments of reasoning, as between a definition and a proposition in geometry.

The species includes all the characters which are in the genus, and those likewise which distinguish that species from others belonging to the same genus; and the more divisions we make, as order, family, class, it is intended that the names of the lower should become still the more comprehensive in their signification, but the less extensive in their application to individuals. Naturalists by this invention, which is not exclusively their own, have it in their power to contemplate and reason upon these separate characters, with all their consequences, as if they existed independently of species; as by the use of the word species they are enabled to look at their peculiar attributes independently of individuals. This faculty of the mind, which is one of the most curious that belongs to it, has given rise in all languages to a multitude of words of the same kind as the names of genera in Natural History; words, which do not express individual existences, but are abstractions of qualities and characters belonging to them *.

All general reasoning in morality, law, politics, and even mathematics, depends for its accuracy upon the proper use of generic and other abstract terms. In mathematics they admit of exact (or I would rather say more exact) previous definition; and hence arises the accuracy of deductions the most recondite and remote in that science. In the other sciences, which are of a speculative and contingent nature, these terms are employed not with the same precision, but seem to be the result of our necessities, borrowed from sensible objects and analogy, and frequently indeed from accidental coincidences. They derive their force rather from the character of the mind that employs them, than from any exact definition they may have received; and it seems impossible to make men use such words in a common acceptation. Hence it is, I apprehend, that knowledge of a speculative kind so soon finds its limits; and where at its outset it has promised such glorious results to mankind, as long as it floated in general propositions, the same subject eludes the grasp of the human faculties when it is attempted to be reduced to exactness, and leaves something always to be desired. We are constantly approximating to the truth, yet never reaching it.

It is sometimes asserted, but not correctly, that Natural History, by the aid of its terms, partakes of the nature of mathematical truth; or that it lies intermediate between that science and speculative knowledge. The situation of the naturalist is rather this. He finds himself placed amidst an infinite number of unknown particulars; and in order to facilitate an acquaintance with them, he at once, without regarding individuals with much minuteness, throws together a number of them, which he calls a species, according to an assumed hypothesis. These he attempts again to combine by certain external characters, and calls them a genus. By these means he is enabled to contemplate and treat of them, without being utterly bewildered in the labyrinth of unarranged individuals. Classification is his *filum Ariadneum*. It was but imperfectly understood by the ancients; and has enabled the moderns to arrive at conclusions with much more expedition than they,

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* I would avoid here, and leave the question to be decided by the reader, after he has consulted Locke and Berkeley, whether we have got ideas corresponding to these abstract terms, or whether they are mere signs, like x, y, and z, in algebra.

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and with equal safety. It does that at once which is constantly going on in ordinary language,—the modifications of it to express the classes of external objects. The invention of new terms suited to express new ideas in an abridged and compressed form, is a slow process, and in most cases is the result of convenience. There is no convention to attain the object, because nobody can arrest the subtile means that are employed. But the naturalist being without terms, or at most with so few that they are within his power, attempts to anticipate the slow process usually working in language, and forms at once his instruments of reasoning; and systems and methods can be regarded as no further useful, than as they are assimilated to the ordinary process of abridging the labour of thought adopted by mankind in other subjects of a like nature.

Naturalists err greatly who imagine they are employing terms possessing some new and distinct properties; whereas all they can do is to hold the subjects of natural history together in a loose manner by the use of the words *species*, *genus*, *order*, and *class;* thus presenting certain characters to the mind as separate objects of contemplation by means of abstract terms, of a similar though somewhat more precise import than those which are employed by the rest of mankind in treating general subjects. A stricter use may be made of these words by naturalists than by metaphysicians, because the business of the one is to examine characters and qualities more nicely than the subjects entertained by the other will admit of. Nevertheless, the one cannot employ these abstractions as instruments of reasoning in a different sense from the other. There is no magic about them in the hands of a naturalist more than there is in any of the thousand general terms in the mouths of the vulgar. "Rose" and "Grass" were generic names before the flood, and will continue to be so in spite of systems and methods. The naturalist has attempted only to carry this necessary operation of the mind somewhat further and with more precision, and has thus exposed himself to errors, which the vulgar have escaped. Thus, although there are but two modes of reasoning; namely, by the use of words expressive of an individual and its attributes, or by general words indicative of an aggregation of individuals with their common attributes; yet naturalists have used their terms in a different sense, and have invented additional ones, such as *order*, *tribe*, *cohort*, *family*, *class*, by which they attempt to express with more accuracy larger generalizations than they would do by employing a generic term, and as if they could settle the relative rank of the different groups whose existence they have assumed. Whereas the truth is, that in many instances a class may be equivalent to an order or a genus. These different gradations, thus strictly aimed at, are gratuitous assumptions with which Nature has nothing to do; and which frequently lead to the establishment of false hypotheses.

[To be continued.]

XXXVI. *Notices respecting New Books.*

The Steam-Engine: comprising an Account of its Invention and Progressive Improvement, with an Investigation of its Principles and the Proportions of its Parts for Efficiency and Strength; detailing its Application to Navigation, Mining, impelling Machinery, &c. with 20 Plates and numerous Wood-cuts. By Thomas Tredgold, *Civil Engineer, Member of the Inst. of Civ. En. &c.* Price 2*l.* 2*s.* 4to. Jos. Taylor.

A BRIEF analysis of the contents of this valuable work will give our readers some idea of its utility, as well as of the immense degree of labour necessary to collect, arrange, and reduce to their elementary principles, the various phænomena concerned in that master-piece of human contrivance, the Steam-engine. In so doing, we propose to follow, in some degree, the author's statement of its contents in his Preface; with the addition of many particulars he has not noticed in that short summary.

The work is in sections. The first contains a short history of the invention and improvement of the steam-engine, the inventors being noticed in the order of the dates of their first essays, commencing with the Marquis of Worcester, and closing with Woolf and Oliver Evans: a short sketch is added of the rapid progress of the application of steam power; and the section concludes with the remark "that the whole tends to prove that the steam-engine, in the highest state of perfection it has yet attained, is entirely of British origin. The remark extends to the discovery of physical principles, as well as of mechanical combinations. No new principle, nor no new combination of principles, has yet been derived from a foreign source, the most perfect of foreign steam-engines being professedly copied from British ones, and not unfrequently manufactured by British workmen."

The second section treats of the nature and properties of steam, its elastic force, expansive force, and motion; first stating the general principles of the equilibrium of heat, the phænomena attending a change of state, and a comparison of the best experiments on the quantity of heat required to convert bodies into vapour or steam, with the necessary formula for the comparison. The elastic force is next considered, with the principles which guided the author in his choice of a formula for a case where empirical means are necessarily employed: and here we remark for the first time a formula for the expansion and expansive force of water, free from the objection of becoming negative in either high or low temperatures. The connection between the boiling point of sea-water and the force of its steam is treated, and a simple formula for the force of vapours is shown to apply to those from various fluids, with the necessary

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correction of the constants.—The mechanical power of compressed gases is examined; and it is inferred, that the steam of water possesses greater advantages for use in an engine, than the vapour of any other liquid.—Rules are next given to find the volume the steam of a given quantity of water occupies at a given force and temperature; the laws of the mixture of steam and air, as in the air-pump of an engine; and the laws of the motion of steam in the passages, &c. of engines, with the loss by cooling; and a curious inquiry respecting the temperature of condensation which affords the maximum of useful effect in the old atmospheric engines; illustrated by a reference to Mr. Watt's experiments, with a model of one. The section concludes with the investigation of the formula for the escape of steam at safety-valves.

The third section is on the generation and condensation of steam, and the apparatus required; first treating of combustion and combustibles, with an extensive table to show the relation between the heat afforded by bodies, and that which their constituent elements would give: then the conditions favourable to the process of combustion are considered, and the quantity of the gaseous products, with rules for the supply of air, and size of chimneys. The relation between the surface of boiler to be exposed to heat, and the quantity of steam to be generated; and of the capacity of the boiler to the effective power of the engine, are also exhibited, and compared with practice; and then rules given to fulfil the essential conditions. The different species of boilers, fire-places, safety-valves, and other apparatus, are then described; and the section closes with the principles of condensation, the quantity of water required to produce it, and a tabular view of the methods that may be used, referring to the dates of the first use of them in the steam-engine.

The fourth section commences with a popular illustration of the nature and modes of obtaining the power of steam, and their relative advantages; next, proceeding to show how to calculate the utmost effect steam can possibly afford to produce rectilinear motion; and afterwards its effect in rotary engines,—showing the loss of effect in the latter to be constant, and in some cases very considerable. The modes of applying the power of steam are classified, with their combinations; and then the laws which determine the relations of the parts for cylinder engines, so as to afford a maximum of useful effect with a given quantity of steam; the capacity of the air-pump, and the power lost in working it.

The fifth section treats of the proportions, power and constructions of noncondensing engines, usually called high-pressure engines, showing the causes of loss of power, and the means of estimating the quantity of loss; the best proportion for cutting off the stroke when the steam acts expansively; and the theory of combined cylinder expansive engines.

The sixth section treats of condensing engines. After a general view of their nature, and a classification of the methods of construction, the proportions, causes of loss, and the power of the atmospheric engine are investigated; and also in its combination with a separate condenser. The steam-pressure engines of Boulton and Watt, both single and double, is next considered, with the construction of them either for expansive action, or not, and all the causes of loss of power, and the proportions of the parts in detail; also the combined engines of Hornblower and Woolf are treated of in a similar manner.

The seventh section is occupied by the construction of the parts common to most engines; as valves, slides, pistons; with the mode of estimating their friction, methods of moving valves, &c. cranks, parallel motion apparatus, and practical rules for the strength of the parts of engines, with the conditions necessary for safety in boilers, and the mode of estimating the effect of irregular expansion on the strength of boilers of cast-iron,—closing with the methods of joining the parts of engines together.

The eighth section is on equalizing the action, regulating the power to the effect, measuring the quantity of useful effect, and managing the working of engines; describing, with investigation when required, the fly and counter weight to equalize the action, the throttle valve, Field's valve, the conical pendulum, the regulator and the cataract to regulate the power; and the gauges, indicator, and counter, to show the state of the parts, and how by friction the useful effect of any engine may be measured at a trifling expense; and lastly, the circumstances to be chiefly attended to in working the engine.

The ninth section is on the application of the steam-engine to various purposes; first, generally to raising water, then its particular cases, as the drainage of mines, the supply of towns, with a table of the supply to towns in ancient and modern times; secondly, to impelling machinery for manufacturing purposes; thirdly, its application to agricultural purposes; and lastly, its application to steam carriages.

The tenth section is on steam navigation, treating of the longitudinal and lateral stability of vessels, and the resistance of vessels; in the latter inquiry a new theory of resistance is adopted, which appears to give results nearly according with practice. The various modes of propelling vessels are noticed, and those by water-screws and paddle-wheels are investigated, and the modifications pointed out; then the strength of vessels, the power of sails and of currents, and their combination with steam power, followed by the necessary rules for arranging the proportions, power, and effect of vessels, and a tabular view of the vessels, executed at different times by the best manufacturers in this country.

These multifarious inquiries are followed by an extensive table of the properties of steam, and two tables of the proportions of engines for different powers, with the expenditure of water and fuel each requires. The letter-press is illustrated by wood-cuts in the pages, and the whole by twenty plates, each having its description opposite to it; and a copious index, and a full table of contents, renders reference to particulars easy; and, as a whole, this most useful work presents that combination of science with practice, which perhaps no one, except a person having the experience of a workman, as well as an extensive knowledge of science, could have written,—and such we know to be the case with the author*.

The Elements of Geometry, with Notes. By J. R. YOUNG.

The great excellence of Euclid is well known to consist in the completeness and perfect accuracy, as well as the general simplicity and elegance of his demonstrations, admitting no assumption, no step in the process of his reasoning that has not been previously established, and at the same time, with this requisite, usually adopting the shortest course for arriving at his conclusion. It is not, however, pretended that this admirable work, so long and so deservedly received as the text-book for instruction, is absolutely free from defects. Even the very rigour of his proofs is sometimes the source of so much intricacy and abstruseness, especially in the equally difficult and important branch of the mathematics, the doctrine of proportions, as to form a serious and discouraging obstacle to the progress of the learner. The want of success in every attempt hitherto made to lessen this difficulty, might seem to render the task hopeless. The aim of Mr. Young, however, in the work before us, has been, without impairing the completeness and satisfactory nature of his demonstrations, to contract and facilitate the labour of the student as much as possible by simplicity and conciseness, and also to add to the extent and accuracy of his geometrical knowledge. Nor do we hesitate to recommend his treatise, not certainly as superseding the use of Euclid, but as a useful auxiliary to that great work. His observations on the theory of parallel lines; his demonstration of the converse of every proposition where this is possible, and showing its failure, where it is not; the labour he has bestowed on the doctrine of proportions, as well as his corrections of many errors of preceding geometers, and supplying their defects, together with his minute attention to accuracy throughout,—may be justly considered as rendering his performance valuable, especially to the learner.

The notes are numerous, and most of them important. Among the various errors and defects of modern geometers detected and corrected in them, one instance especially is singular, and deserves to be noticed. Mr. Thomas Simpson, in his Elements of Geometry, has substituted in the place of the seventh proposition of Euclid's sixth book, one which Mr. Young has proved to be absolutely false. This is the more remarkable, as, though the work in which it occurs has been in the hands of geometers more than seventy years, this error has hitherto escaped detection. Even Dr. Robert Simson, the editor of Euclid, though entertaining no very friendly feeling towards his contemporary, suffered it to pass unnoticed. A similar error occurs also in Mr. Leslie's Geometry, Prop. xiv. B. vi. last edition.

As the celebrated propositions of Euclid, (B. i. Prop. 47.) the discovery of which is attributed to Pythagoras, admits of various demonstrations, Mr. Young has given three; the last of which in the notes is concise and simple, though that inserted in the text instead of Euclid's, is inferior to the latter, not only in elegance but in simplicity also.

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* His first Essay was a paper in the 46th vol. of the Phil. Mag. p. 15.

The first eight books of this work now published treat of lines situated in the same plane. The second Part, as announced by the author, will contain the geometry of planes and solids; with an Appendix, and Notes on the Symmetrical Polyedrons of Legendre.

XXXVII. *Proceedings of Learned Societies.*

LINNÆAN SOCIETY.

Feb. 5.—READ some account of the Botany of the provinces lately ceded by the Burmese to the Honourable the East India Company; with a description of two new genera of plants: in a letter to H. T. Colebrooke, Esq., F.R.S. & L.S. &c. By Nathaniel Wallich, M.D. F.L.S. F.R S. E. &c. Superintendent of the Botanic Garden at Calcutta. The author states that his botanical treasures are most extensive; the number of species having long ago surpassed 2000; and that he has never seen any vegetable production equal to his *Amherstia nobilis* when in full bloom. It surpasses all the Indian plants.

Amherstia. Diadelphia Decandria—Nat. Ord. Leguminosæ. The flowers of this splendid tree are disposed in pyramidal pendulous clusters 2 feet long, and 10 inches broad at the base. Leaves 1½ foot long, with 8 or 10 pair of oblong pointed pinnæ, which are from 8 to 10 inches long, and of a peculiarly delicate glaucous hue. The racemes are scarlet. The petals are furnished at the apex with a broad yellow spot, having a tubular calyx; and the genus is evidently allied to Heterostemon of Desfontaines.

Dr. Wallich has at length found the *Varnish*-tree of the Burmese, which he constitutes a new genus, and calls it *Melanorrhœa;* Polyandria Monogynia; Anacardiæ, Brown.—Also another singular plant, which he calls *Phytocrene gigantea,* allied to Araliaceæ. The trunk is as thick as a man's thigh, and when divided affords a large quantity of a limpid, tasteless, and very wholesome water.

Feb. 19.—Read a description of a curious Fungus belonging to the Gastromycous order, found near Wrexham by J. T. Bowman, Esq. F.L.S. on decaying oak branches stript of bark. In its earliest stages it is globular: afterwards, from the expansion of the filaments, the sporules are exposed, and the sporangium becomes rugged and broken; from the ripening of the seeds the peridium bursts, and the filaments being set at liberty acquire first a horizontal and then a more erect position, resembling the branches of a palm-tree.

At the above meetings were also read some portions of a paper by J. E. Bicheno, Esq. F.R.S. Sec. L.S. entitled "Remarks on the Flora of Great Britain, as connected with Geography and Geology."

The Author in this paper, instead of attempting to connect plants with particular temperatures, as most authors who have treated the subject have done before, endeavours to show the relation which vegetables have to Geographical and Geological structure. He regards England as the most favourable place to commence such remarks, because of the intimate knowledge we have of its stratification, and also of the stations of all our plants.

In order to assist our inquiries he thinks it necessary to reject all those species the introduction of which may be traced to artificial causes, regarding those alone as indigenous which are coeval with the soil. Agriculture, Medicine, and even Religion, have each swelled our list. *Coronopus didyma* is not found in England but at ports, and seems to be a particularly migratory species. It may be found, he says, in almost every latitude where ships frequent;—at Lisbon, Madeira, Rio Janeiro, the Cape, Sydney. He thinks the same disposition may be witnessed in many of the Cruciferæ, and adduces as instances, *Sisymbrium Irio*, and *Thlaspi campestre.* Whether the cause of such phænomena is to be sought for in the structure of the seed, or in the pabulum necessary to the growth of the plant, remains to be determined. The *Blatta orientalis* (common Cock-roach), and the Norway Rat, which has eaten up the old Black Rat, are instances from the animal world, showing a similar tendency.

In order to ascertain whether a species be indigenous, it is of importance to ascertain upon what stratum, or in what places, it terminates its range. Many southern plants he finds terminate their northern range upon old walls, such as *Sisymbrium Irio* and *tenuifolium*, *Dianthus Caryophyllus, Teucrium Chamædrys*, &c. Others again abounding in the south, finish their course by attaching themselves to some of the warmer strata, as to mountain limestone, or new red sand-stone. *Hutchinsia petræa* grows in the Olive country so abundantly that it is gathered as a salad. With us it finds only a few localities, and those always on mountain limestone. *Campanula patula* finds its most northern accommodation in England on the new red sand stone. It has one other station with us on chalk, which is its place in Brittany.

The sea-coast also carries many plants further north, than inland situations can do. The Common Privet for instance, though so abundant in the south on the chalk and gravels, becomes rarer as we proceed to the north. In Scotland it affects the coast, and in Sweden is wholly confined to it. On the other hand, plants which belong to a more northern latitude and to more primitive countries, do not terminate their southern range on our warmer and richer soils. The author suspects *Daphne Mezereum* not to be English, because it is found only on the more recent strata.

The stratum producing the greatest number of rare plants with us is the mountain limestone; and although but a small portion of this rock should appear above the surface surrounded by other rocks of a different structure, it seems to have caught the rarer species. The district of Gower, Brean Down, St. Vincent's Rocks, Orme's Head, are examples.

Next to mountain limestone the Bury sand seems to be a favourite station of unusual productions. *Veronica triphyllos* and *verna, Vicia lathyroides, Tillæa muscosa, Scleranthus perennis,* &c. &c. grow there.

The richest soils are the least productive of rare species. Slaty countries, excepting the highest elevations, are also singularly deficient.

Many of our promontories are remarkable for the rarities they produce. *Erica vagans* terminates its northern range on the serpentine of the Lizard; *Draba aizoides*, in Gower.

The author then proposed to consider the vegetation which is produced by the different strata, commencing with the eastern side of the kingdom, or with the most recent deposits.

The diluvial soil of the Fens, which is composed of decayed vegetables resting on clay, is, when left to nature, soon covered with grasses, to the exclusion of every other plant. Scarcely a single rare species is to be found on it. The water in such soils, on the contrary, abounds in rarities. *Stratiotes aloides, Arundo calamagrostis, Teucrium Scordium, Lactuca Scariola, Senecio paludosus*, &c. &c. are of this number. The author remarked, that the first of these species, though wild in Berkshire, had not been observed to flower there. *Arundo calamagrostis* he regards as peculiarly a Fen plant with us, and that the localities assigned to it elsewhere belong to *A. epigejos*. This locality appears to be on its most southern limit. The most remarkable feature of Fen vegetation is the luxuriance and quantity of Cruciferæ.

Whatever phænomena geologists may discover to induce them to regard the Bury sand as the same with that of Bagshot-heath, the author remarks that their vegetable productions do not correspond; and though he does not assert that we ought to find a similar vegetation on strata of the like nature, yet it so frequently occurs that the rarer plants correspond where strata are alike, that he regards such differences as worth noticing.

The rare plants of the Bury sand are peculiar to itself, while the Bagshot sand is remarkably deficient in uncommon species; the only one of notoriety, he has remarked, being *Agrostis setacea*, which is abundant, and which may be traced upon the sands and gravels of the plastic clay, through Dorsetshire, Hampshire, Surrey, and even to the Commons in the neighbourhood of London, where its range terminates.

GEOLOGICAL SOCIETY.

Feb. 1.—The reading of Professor Sedgwick and Mr. Murchison's paper, begun at the last meeting, was concluded.

This paper consists of three divisions: 1st. A brief outline of the general structure of the Isle of Arran. 2d. An account of the section on the N.E. coast of the island. 3d. Concluding remarks explanatory of the probable causes, and geological epochs of the several phænomena. In the 1st division, the authors, considering that the subject has been amply elucidated by Jameson, MacCulloch, and Hendrick, confine themselves to such details as are necessary to make their subsequent description intelligible. In the 2d part, the strata on the N.E. coast are described in great detail, for the purpose of comparison with the corresponding members of the English Series; from whence it appears, that a succession of formations, analogous to the old red sandstone, carboniferous series, and new red sandstone, are exhibited twice over, in an anticlinal section.

The mineralogical centre of this section is at North Sannox, and the lower red conglomerate is there seen in several situations, rising to the height of about 1000 feet above the sea. 1. This formation is supposed to be identified with the old red sandstone; from its lowest members graduating into grauwacke; from its containing concretionary limestone not distinguishable from the cornstone of Herefordshire; and its being regularly overlaid by the carboniferous series. 2. The middle deposit of the section is clearly referable to the carboniferous series, by its mineralogical structure, by the organic remains in the calcareous beds, which are identical with those of the mountain-limestone; by its containing seams of coal, which have been worked; and by the plants in the shale being of the same species with many of those most abundant in the coal-measures of England. 3. The superior sandstone and conglomerate are of enormous thickness, rising into lofty and precipitous hills upon the coast. These are referred to the new red sandstone, from their position and internal characters; and this classification is confirmed particularly by the structure of the sandstone on the southern coasts of the island. This formation differs however from the new red sandstone of England, not only in being conformable to the beds on which it rests, but also by graduating into the superior parts of the carboniferous order.

In conclusion, the authors endeavour to show, that the great dislocations of the secondary deposits have been produced by an upheaving of the granite; and they state, in corroboration of this opinion, that where the breaks in the strata are greatest, there the granite makes the nearest approaches to them. It is further attempted to be proved, that the granite could not have been in a perfectly fluid state at the period of its elevation, from the fact of its existence in the form of mural and serrated precipices on the flanks of the secondary strata; being in this respect prominently distinguished from the trap of the southern regions of the island, which has, in numberless places, not only penetrated, but overflowed upon the new red sandstone.

Feb. 15.—The Anniversary Meeting of the Society was held this day; and the following Fellows were elected Officers and Council for the year ensuing:—*President:* William Henry Fitton, M.D.: *Vice Presidents:* Arthur Aikin, Esq. F.L.S.; Rev. W. Buckland, D.D. F.R.S. Professor of Mineralogy and Geology in the University of Oxford; Charles Lyell, Esq. F.R.S. & L.S.; Rev. A. Sedgwick, F.R.S. Woodwardian Prof. Camb.—*Secretaries:* W. J. Broderip, Esq. F.L.S.; R. I. Murchison, Esq. F.R.S. & L.S.—*Foreign Secretary:* Henry Heuland, Esq.—*Treasurer:* John Taylor, Esq. F.R.S.—*Council:* J. E. Bicheno, Esq. Sec. L.S.; John Bostock, M.D. F.R.S.; Rev. W. D. Conybeare, F.R.S; John Crawfurd, Esq. F.R.S.; Michael Faraday, Esq. F.R.S.; Davies Gilbert, Esq. M.P. Pres. R.S.; G. B. Greenough, Esq. F.R.S; J. F. W. Herschel, Esq. Sec. R.S.; Leonard Horner, Esq. F.R.S.; Ashhurst Majendie, Esq. F.R.S.; Rev. J. H. Randolph; N. A. Vigors, Esq. F.R.S.; Sir R. R. Vyvyan, Bart. M.P.; Henry Warburton, Esq. M.P. F.R.S.

ASTRONOMICAL SOCIETY OF LONDON.

Jan. 11, 1828.—There was read a paper entitled "Third Series of Observations with a 20-feet reflecting telescope;—containing a Catalogue of 384 new double and multiple stars, completing a first thousand of those objects detected in sweeps with that instrument;—together with Observations of some previously known." By J. F. W. Herschel, Esq., President of the Society.

This paper, as its title imports, is a continuation of the two papers previously communicated by the author on the same subject. The field of discovery in this department of astronomy, though narrowed by the great work recently published by Professor Struve, the author considers as not yet exhausted; since, on an average of the part of the heavens swept by him, not above one in four, of double stars sufficiently remarkable to attract attention in sweeping, have been catalogued by the eminent astronomer last named; not to mention the vast number of interesting close double stars, below the 9th magnitude, which a minuter examination than the nature of his sweeps permits would no doubt produce. The double stars of this Catalogue, he observes, are considerably more *select* than those of his two former ones; those whose distance exceeds 32″ being (except in particular cases) excluded, and the limit of distance being narrowed according to the faintness of the component stars.

The author prefaces his Catalogue with a comparison of the magnitudes habitually assigned to the stars by himself and Professor Struve; from which it appears that on the average, his magnitudes have a denomination about one unit lower than those of that astronomer;—a star (for example) which M. Struve would call of the 9th magnitude, being, in Mr. Herschel's nomenclature, of the 10th. The limit of vision in the Dorpat telescope, he presumes to lie about his average 14th magnitude, though such a determination must necessarily be liable to some latitude. This conclusion he deduces from a series of instances, in which small companions have been seen by him attached to large stars, within the limits of Professor Struve's 4th class, which have escaped the notice of the latter.

The author then states the principle on which he estimates magnitudes below the 6th, which is that of continual bisection of the light; and he cites some experiments, by which it appears that the light of an average star of the 1st magnitude is at least 150 times that of the 6th. He then adduces a series of observations of a considerable number of the closer stars, of M. Struve's Catalogue, by which it appears that the Slough telescope easily defines with its ordinary sweeping power, the generality of M. Struve's stars of the 1st class, and many of those marked by him as *vicinæ*, and even *pervicinæ*; but those which have the epithet *vicinissimæ*, he has not yet succeeded in separating with the highest power (240) usually applied,—which indeed was to be expected. In lieu of M. Struve's classification of double stars, which he considers as enlarging beyond due limits the number of those of the 1st class, he proposes the following system, which in fact very nearly approximates to that originally followed by Sir William Herschel.

Class I.	close	0″	and below	1″
	not close	1	and below	2
Class II.		2	and below	4
Class III.		4	and below	8
Class IV.		8	and below	16
Class V.		16	and below	32
Class VI.		32	and below	64

So that the limit of distance of stars of the *n*th class shall be $2^n \times 1''$.

The author then subjoins a list of stars common to his two former Catalogues, and to that of Professor Struve, 86 in number; after which he proceeds to describe some singular phænomena observed in the course of his examination of these objects, which explain certain discrepancies between the results of observations of their angles of position on different nights, and which tend to throw light on some obscure points in the theory of vision. He considers it as rendered very probable, by some of the facts adduced, that time is required for light to make an impression on the retina, as well as for the impression made to wear off; and that this time is the less, the brighter the object; and explains by this principle a remarkable degree of unsteadiness and fluctuation observed in the limb of the planet Mars, while small stars in the field remained perfectly tranquil, as well as certain other curious phænomena.

He then adds some observations on the contrasted colours so frequently observed in double stars, and regards them as (at least) in many cases referable to the laws of vision; in virtue of which, a strong light having an excess of the less refrangible rays, will cause a feebler one, in which no such excess exists, to appear of the complementary hue; instances of which, in artificial lights, are adduced. He notices especially the extremely intense red colour of a star of the 8th magnitude, R.A. $4^h\ 41^m$. N.P.D. 61° 47′ (1828.)

These prefatory remarks are terminated by some observations of the 5th star in *trapezio nebulæ Orionis*, pointed out by M. Struve. The author adduces evidence, which he considers as satisfactory, that no such star existed in that situation on the 13th March, 1826. It was observed, however, by M. Struve, to be conspicuous on the 11th Nov. of that year. It is now readily seen in the Slough telescope; and at the time of drawing up the present paper, it was so bright as not to be overlooked with the most ordinary degree of attention. He considers it therefore, if not as a NEW STAR, at least as a variable one of very singular character.

The Catalogue, which follows, is arranged in all respects like the preceding ones published in the Memoirs of this Society, and is followed by a list of about 200 double stars, for the most part found in the same sweeps with the others; but which, occurring in M. Struve's Catalogue, cannot now be regarded as new double stars. Their observed places and estimated angles of position, distances, and magnitudes, are however given, in order to afford ground of comparison between the two Catalogues, of which comparison the results are stated.

PROCEEDINGS AT THE FRIDAY EVENING MEETINGS OF THE ROYAL INSTITUTION OF GREAT BRITAIN.

Jan. 25.—The Members of this Institution commenced their meetings for the season, on this evening; Mr. Brande occupied the place at the lecture table, and gave an account of the discovery of the three most important vegeto-alkalies, Morphia, Cinchonia, and Quinia; also of their properties, the best methods of preparing them, the methods of detecting impurities in them; illustrating the whole by reference to experiments and specimens.

At the close of the evening he paid a just tribute to the memory of Mr. Daniel Moore, who, a firm friend to the Institution during his life, left it a thousand pounds at his death.

A quantity of the new element bromine was laid upon the library tables, with specimens of Brazilian manufactures, &c. &c.

Feb. 1.—An experimental illustration and explanation of the curious phænomena produced in certain circumstances by a current of air, steam, water, or of any other fluid, was given in the lecture room, by Mr. Faraday. The phænomena were first brought to the notice of scientific men by M. Clement, and consists, as our readers well know, in the apparent adhesion of a flat disc against an aperture in a plane surface, out of which the stream of fluid is passing.—Make a smooth round hole about the eighth of an inch in diameter, through the middle of a large sound bung, cut one of the flat surfaces of the bung smooth with a sharp knife, stick three or four pins upright into that surface, equidistant from each other, and about three quarters of an inch each from the central hole; and then drop between them a disc of paper or card one inch and a half in diameter, so as to lie loosely between the pins over the hole. No effort to blow the paper off by forcing air or the breath through the hole in the bung, will succeed; but the stronger the current, the more forcibly will the paper be pressed up against it. Mr. Faraday gave the same explanation of the effect as that given by M. Clement. He stated it to be a pure effect of the momentum of the air between the disc and the cork, and to have no necessary connection with the lateral currents of air which move on to join a stream passing in one direction:—these lateral currents were cut off in some of the experiments, and still the effect remained unabated. He equally denied the effects of friction as having any thing to do with the effect.

Numerous objects of interest were placed upon the library tables, amongst which were several from Ashantee, and especially the scull of an Ashantee slain in the battle of Aug. 1824, which had two occipital bones.

Feb. 8.—Mr. Ainger gave an illustrated account of the origin of Grecian architecture, and of the principles acknowledged in modern times as derived from it, and then applied those principles to certain parts of St. Paul's cathedral; at the same time strongly reprobating the species of tyranny which has resulted from judging a building of one kind by the rules and the taste which have been formed upon others altogether of a different nature.

Presents, curious specimens, and new machinery were laid upon the library tables as usual.

Feb. 15.—The subject of the evening was Resonance, or the reciprocation of musical sounds. It was explained and illustrated by Mr. Faraday; but he stated that his information was obtained from Mr. Wheatstone, to whom belonged also the new matter brought forward.

The vibration of one string when another in unison with it was struck, was referred to as an illustration of the nature of reciprocation. The honour of this discovery was given to Messrs. Noble and Pigott, pupils of Dr. Wallace. Reciprocation by undulations communicated through the air, or through solid bodies, was then illustrated, and the nature of stringed instruments explained.

After this, followed matter of a more original nature. It was stated that columns of air could reciprocate to vibrating bodies, when their vibrations could accord with those of the latter; in illustration of which, a flute was made to speak, simply by approaching a vibrating tuning-fork to its embouchoir. Other columns of air were also made to produce intense sound by reciprocating with tuning-forks which were themselves inaudible. An elementary model of an instrument to be constructed upon this principle was also exhibited.

Some musical instruments were then referred to, which had been brought by Sir Stamford Raffles from Java, and which had been lent to the Institution by Lady Raffles, for the evening's illustrations. They consisted of plates of sonorous metal, suspended by their nodal points over pipes of bamboo adjusted to reciprocate to the lowest note produced when the plate was struck. When the apertures of the tubes were covered, and the plates made to sound, the ordinary tones were produced; but upon removing the cover, the air in the tubes instantly reciprocated to the lower notes, and a series of fine rich sounds were produced. This instrument is called the Génder.

A further illustration of the reciprocation of sound was then given on the guimbard or Jews-harp, the different tones of which are found to depend upon the circumstance, that columns of air reciprocate not only when they vibrate in equal times with the original phonic, but also when their vibrations are any multiple of those of the phonic. This fact was illustrated by a syringa, and also by the beautiful performance of M. Eulenstein on the Jews-harp.

Some illustrations of the resonance of the columns of air in the cavities of the ear were then given,—and the subject concluded.

In the library were several beautiful engravings, by Robinson, Turrell, and others. A new perspective instrument by Mr. Turrell, called a Perspectograph, &c. &c.

XXXVIII. *Intelligence and Miscellaneous Articles.*

ANALYSIS OF SOME ALLOYS OF BISMUTH.

M. Laugier has analysed several of these compounds. To analyse an alloy of bismuth and lead, he dissolves it completely in dilute nitric acid, and then pours into the solution one of carbonate of ammonia,

monia, which at first precipitates both oxides in the state of carbonate; and when added in excess, redissolves the carbonate of bismuth. The carbonate of lead is to be washed in the filter with a solution of carbonate of ammonia, in order to dissolve any adhering carbonate of bismuth; the washing is to be finished with warm water, in order to dissolve all the carbonate of ammonia used in the washing.

The alkaline liquor containing the oxide of bismuth, is to be saturated by an acid, and then ammonia is to be added in excess; all the oxide of bismuth is precipitated, and after washing on a filter it is to be dried and weighed.

There is another method, which is perhaps more simple, but not so exact.—Boil the alkaline liquor and evaporate to dryness, wash it and filter in order to collect the oxide of bismuth. But this method is inconvenient, because the oxide of bismuth adheres to the vessel in which the solution is evaporated.

An alloy of equal weights of lead and bismuth gave by analysis 49·1 of the former metal, and 49·6 of the latter; the loss being 1·3 per cent. The nitrate of lead and oxide of bismuth separated by this process were both pure.

The fusible compound of 8 parts bismuth, 5 lead, and 3 tin, analysed with nitric acid, left the oxide of tin on the filter: the oxides of bismuth and lead were separated as above directed; 16 parts yielded bismuth 7·98, lead 4·95, tin 3; the loss of the operation amounting to $\frac{7}{1600}$ of the whole.—*Ann. de Chim.* xxxvi. *p.* 333.

EXAMINATION OF COPPER.

Some samples of copper which I examined some time since, contained a small portion of silver, without any other impurity which I could detect: lately, some other samples have been put into my hands, in order to determine the cause of their being complained of. The copper was dissolved in dilute nitric acid, used in excess; it contained no lead, and upon the addition of muriatic acid to a very dilute solution, slight precipitation took place, which I at first imagined was occasioned, as in the former case, by the presence of silver. I happened, however, to remember a fact mentioned in conversation some years since, and which I have never met with in any chemical work,—That a solution of bismuth so dilute or so acid that water would occasion no precipitation in it, is decomposed by the addition of common salt or muriatic acid; and this I found to be the case in the present instance: the precipitate was small in quantity, not amounting to one per cent for the copper employed, and it differed from chloride of silver in very readily passing through filtering paper. I found it necessary, in order to determine its quantity, to supersaturate with ammonia, by which the oxide of copper was dissolved, and the oxide of bismuth precipitated. R. P.

ON EFFLORESCENCE.

The following observations are by M. Gay Lussac. Many salts when exposed to the air, are well known to effloresce; that is, they fall to powder and lose their water of crystallization; and it is generally supposed that salts after efflorescence are perfectly anhydrous. Having been long convinced that this opinion is not correct, I have made some experiments upon the principal salts which are efflorescent in a high degree. Crystallized sulphate of soda, exposed to the air even when it is not very dry, readily loses all its water of crystallization. Phosphate of soda becomes readily opake without losing its form. After three months' exposure to the air, it contained on the 18th of July, 7·4 of the 12 proportions of water, which it contains in its usual state. Reduced to powder, and thinly spread upon paper, it contained on the 26th of July 5·65 proportions of water;—again exposed to the air during a hot and dry period, it contained on the 31st of July only 5·65 proportions;—afterwards exposed till the 21st of October, the weather having become colder and more damp, it was found to contain 7·2 proportions of water: some phosphate which had been calcined, absorbed in five days' exposure to the air, nearly half a proportion of water.

Carbonate of soda behaves on exposure very much as the phosphate: it becomes opake, loses much water without altering its form; but I have never found it anhydrous after exposure.

It results from these observations, that some salts completely lose their water of crystallization by efflorescence; but that others retain variable quantities, according to the hygrometric state of the air. I do not assert, however, that the water may not remain in definite proportions; it merely appears that in the phosphate and carbonate of soda, which retains a proportion of water of a certain number,—the seventh for example,—differs but little from that which unites the proportion, immediately above or below.—*Ann. de Chim.* xxxvi. 335.

NATIVE PLATINA.

A piece of native platina, weighing about 64 grains, has recently been found in the Russian mines at Hijne-Taguilski. Its shape was round, its surface granulated, and in some places it bore a metallic lustre. Its specific gravity being only 16, it must contain the various alloys met with in platina. It is singular that this specimen was met with in digging an argillaceous stratum.—*Monthly Mag. Feb.* 1828.

MANUFACTURE OF ULTRAMARINE.

M. Gay Lussac announced to the Academy that M. Tunel, inspector of gunpowder and saltpetre, had succeeded in the direct formation of ultramarine, and that what he obtains by his process is finer and more brilliant than the natural colour. It was by following the analysis made by M. Clement Désormes, that the inventor accomplished this desirable object. M. Tunel has already been able to supply the public with ultramarine at 25 francs the ounce; the colour having hitherto been sold for 50 or 60 francs the ounce. He hopes that he shall be able to sell it at a still more moderate price. M. Tunel has thought proper to keep his process secret for a certain time.—*Le Globe, Fev.* 9, 1828.

HEAT GIVEN OUT DURING COMBUSTION.

M. Despretz read at the Academy of Sciences, on the 15th and 22d of October last, a memoir on the heat given out during combustion. By means of a new method of observation, he found that hydrogen is the body, of which a given weight gives out most heat, and the metals least. The result will be opposite if we refer the results to the same weight of oxygen. It is remarkable that carbon, which in burning does not alter the volume of oxygen gas, produces three-fifths of the heat developed by the metals, iron, zinc, and tin, which reduce the oxygen to the solid state. Hence it is in the act of combination that we must seek for the principal cause of the development of heat, and not in the approach of particles.

In his second memoir, M. Despretz has shown that the quantity of heat developed by a certain quantity of a body which burns without changing the volume of the gas, is the same, whatever be the density of the gas.—*Le Globe.*

INFLAMMABLE GAS ARISING AFTER BORING FOR SALT.

In boring for salt at Rocky Hill, in Ohio, about a mile and a half from Lake Erie, after proceeding to the depth of 197 feet, the auger fell, and salt water spouted out for several hours. After the exhaustion of this water, great volumes of inflammable air issued through the aperture for a long time, and formed a cloud; and by ignition, occasioned by the fire in the shops of the workmen, consumed and destroyed every thing in the vicinity.—*Transactions of the Philosophical Society of New York.*

INFLAMMABLE GAS FROM SALT MINES EMPLOYED FOR PRODUCING LIGHT.

In the salt mine of Gottesgabe at Rheine, in the county of Tecklenbourg, there has issued for sixty years from one of the pits, (which has on this account been called the *Pit of the Wind,*) a continued current of inflammable gas. The same gas is produced in other parts of the mines. M. Rœders, the inspector of the salt mines, has used this gas for two years, not only as a light, but as fuel for all the purposes of cookery. He collects it in pits that are no longer worked, and conveys it in tubes to the house. It burns with a white and brilliant flame. Its density is about 0·66. It contains only traces of carbonic acid and sulphuretted hydrogen, and therefore should consist of carbonated hydrogen and olefiant gas.—*Brewster's Journal, Jan.* 1828.

NATURAL GAS LIGHTS AT FREDONEA.

This village, on the shores of Lake Erie, is lighted every night by inflammable gas from the burning springs, as they are called, in its vicinity. Captain Hall has visited this village, and will no doubt give us a good account of it on his return.—*Ibid.*

IODINE IN CADMIUM.

Iodine is found in the great zinc foundry at Königshute in Upper Silesia, in the cadmium which accompanies the zinc ores.—*Ibid.*

ANALYSIS OF THE GREEN IRON ORE AND ARSENIATE OF LEAD.

According to Dr. Karsten, a variety of this ore from the Hollester mines near Siegen, in Rhein Prussia, consists of

Oxide of iron	63·450
Phosphoric acid	27·717
Water	8·560
	99·727

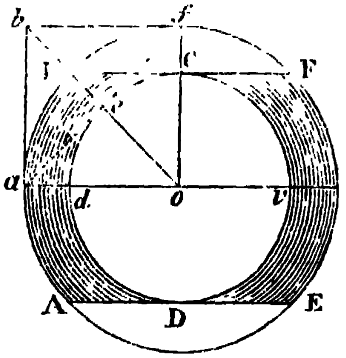
And according to the same authority, a variety of arseniate of lead from Herrhausen near Siegen, in Rhein Prussia, consists of

Oxide of lead	69·97
Muriatic acid	0·81
Arsenic acid	29·22
	100·00

The results of this analysis are not accordant with those obtained by Dr. Wohler.—*Poggendorff's Annalen*, vol. iv. p. 161.

GEOMETRICAL PROBLEM. BY MR. J. ROBOTHAM.

Required, a square equal in area to the space comprised between the arcs [AB, DC, and EF, DC,] of two concentric circles, and two parallel right lines, tangents to the interior circle. The area of the exterior circle being double that of the interior.



Let ABFE, and $dCvD$ be the two concentric circles. Bisect the quadrant aof by the line oB. Then by the nature of the problem, the area $aBde = deo = eoC$. To each of these equals add the area eBC, then will the area of the space $aBCd =$ the area of the triangle oBC. And since $oa = oB$, the square, described on oa, viz. $aofb = 4$ times the area of the triangle $oBC =$ the space AB, DC, and EF, DC. Which may be seen by drawing the diagonal af.

SCIENTIFIC BOOKS.

Just Published.

The 19th Number of Leybourn's Mathematical Repository; containing twenty mathematical questions, and their answers, selected from an extensive correspondence.—Horæ Arithmeticæ, by Mr. Horner.—On Porisms, by Mr. Galloway.—On Central Forces, by the Rev. Mr. Bromhead.—On Equations, by the Rev. Mr. Hawkes. —

—On Attraction, by Ch. Fred. Gauss.—Solutions to a curious Problem, by Mr. Lechmutz.— Cambridge Problems, 1821 — 1826. Together with a set of twenty questions, to be answered in Number XXI.

A Manual of Natural and Experimental Philosophy, beautifully illustrated, has just been published by C. F. Partington, and dedicated to the London Institution. It is well calculated to assist those who attend the Lectures, and not less so to give those who cannot attend a vast mass of useful information.

Observations on the Cruelty of employing Climbing-boys in sweeping Chimneys: and on the Practicability of effectually Cleansing Flues by Mechanical Means. With Extracts from the Evidence before the House of Commons: &c. &c.

Flora Londinensis. Nos. 35 and 36 of the New Series.— These Numbers, containing Twelve rare and highly interesting British Plants, complete the new edition of this splendid Work, which contains upwards of 650 accurate Delineations of Indigenous Plants.

A Manual of Electro-Dynamics, chiefly translated from the Manual d'Electricité Dynamique; or, A Treatise on the Mutual Action of Electric Conductors and Magnets of J. F. Demonferrand; with Notes, &c. &c. by James Cumming, M.A. 8vo.

Reports of Medical Cases, selected with a view of illustrating the Symptoms and Cure of Diseases by a reference to Morbid Anatomy; embracing Dropsy, Inflammation of the Lungs, Phthisis, and Fever. By Richard Bright, M.D. F.R.S. &c. 4to. with 16 coloured plates.

Popular Lectures on the Steam Engine, in which its Construction and Operation are familiarly explained; with an Historical Sketch of its Invention and Progressive Improvement. By the Rev. Dionysius Lardner, LL.D. 12mo.

Preparing for Publication.

Capt. John Ross, K. S. R. N., is about to publish a Treatise on Steam Navigation, Illustrated with Plates, comprehending a History of the Steam-Engine, its Principles, an Explanation of the Terms, and an Account and Examination of the Improvements applicable to Naval Purposes.—A Complete System of Naval Tactics peculiar to Steam Navigation as applicable to Naval Warfare and Commerce, including a comparison of this System with all others.—The Effects of such a System in National Defence.—Rules and Regulations calculated to prevent Accidents by Explosion and Collision.—An Alphabetical Arrangement of Technical Terms used in Steam Navigation; and a Chronological Table of the Progress of the Steam Engine, from the most ancient period of its invention.—The Description and Use of the Royal Clarence Sextant, an Instrument recently invented by the Author, and highly useful in Steam Navigation, for ascertaining the exact distance of Ships from Land, or any object in view: with Tables applying the principle to a Common Sextant.

An Essay on the Application of Mathematical Analysis to the

Theories of Electricity and Magnetism.—This Essay will commence with an Exposition of the General Principles common to both Theories; which will be followed by particular Applications of them to many cases not hitherto submitted to Calculation.

In the course of the present month will be published by W. Wood, 428 Strand, the 4th and concluding Part of Haworth's *Lepidoptera Britannica,* with a complete Index to all the species.

Also, the second edition of Wood's *Index Testaceologicus,* or Catalogue of English and Foreign Shells, illustrated with 2300 Figures.

LIST OF NEW PATENTS.

To J. Weiss, of the Strand, for improvements on instruments for bleeding horses and other animals.—Dated the 26th of January 1828.—6 months allowed to enrol specification.

To Augustus Applegath, of Crayford, Kent, for his improvements in block printing.—26th of January.—4 months.

To Donald Currie, of Regent-street, esquire, for a method of preserving grain and other vegetable and animal substances and ds. Communicated from abroad.—31st of January.—6 months.

Summary, for the Year 1827, *of the State of the Barometer, Thermometer, &c. in Kendal. By* S. Marshall, *Esq.*

1827.	Barometer.			Thermometer.			Quantity of Rain in Inches.	No. of rainy Days.	Prevalent Winds.
	Max.	Min.	Mean.	Max.	Min.	Mean.			
January	30·08	28·89	29·61	49°	9°	34°·59	8·630	14	SW.
February	30·40	28·89	29·51	53	14	33·92	2·698	5	N.
March	30·03	28·40	29·36	56	23	42·93	8·676	22	SW.
April	30·13	29·42	29·77	70	30	47·23	2·553	14	SW.
May	29·94	29·10	29·56	69	32	52·87	3·483	15	W.
June	30·14	29·27	29·69	74	45	56·98	4·264	17	W.&SW.
July	30·15	29·39	29·79	74	42	59·01	3·170	15	W.
August	30·18	29·10	29·81	68	46	56·61	5·214	12	NW.
Septemb.	30·20	29·14	29·78	68	41	55·55	3·329	16	SW.
October	30·18	28·92	29·54	63	34	51·95	3·009	17	SW.
Novemb.	30·05	29·09	29·78	55	21	42·30	2·615	9	W.
Decemb.	30·46	28·57	29·47	54	24	42·53	10·365	23	SW.
Average			29·63			48·03	58·006	179	

In comparing the preceding summary with that for 1826, it appears that the barometer has not reached the altitude which it then attained, 30·78, and the mean height for the year is one tenth of an inch less. The heat of the summer months has not equalled that of 1826, the greatest being 74°; whilst in 1826 the maximum was 85°, and the mean 47°·81. The superior mean temperature for 1827 may be accounted for by the weather's being more uniformly mild. From 26th of April to 21st of November, (or a period of nearly seven months) the thermometer was never so low as the freezing point; and from the former date to the end of the year, there have been but eleven days of frost. We have had thirty-two wet days more in this year than in 1826, and the quantity of rain is greater by 14·926 inches. The writer of these remarks has carefully registered observations on the weather in this town for upwards of five years. He subjoins a summary from 1823 to 1827, both years included. From observations made by the late John Gough, and by John Dalton of Manchester, he inferred the mean annual quantity of rain for Kendal was 51·8 inches. The average for the five years alluded to, will be found to be 57·310 inches. The difference may arise from two causes: —one, the difference of altitude in the places where the observations were made, above the level of the sea; but though that does not exceed many yards, yet even so small a difference will affect the amount of the mean in a series of years. The observations from which the former *mean* was calculated, were taken from twenty different years, though not twenty successive years. Assuming all the observations to be equally correct, the mean deduced from the twenty years is most likely to be the correct one. There are few places where rain-gauges are kept, that have so great a quantity of rain as at Kendal, though it is probable that in many places there are more rainy days within the same period. From a number of observations now in my possession, the annual mean quantity of rain which falls in England, may be stated at 35·2 inches.

Years.	Mean of Barometer.	Mean of Thermometer.	Inches of Rain.	No. of Rainy Days.	Prevalent Winds.
1822			62·726		
1823	29·56	° 45·00	62·749	198	
1824	29·76	46·88	62·762	187	SW.
1825	29·64	47·49	59·973	169	SW.
1826	29·73	47·81	43·060	147	SW.
1827	29·63	48·03	58·006	179	SW.
Mean	29·66	47·04	57·310	176	SW.

METEOROLOGICAL OBSERVATIONS FOR JANUARY 1828.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·40 Jan. 27. Wind N.W.—Min. 29·17 Jan. 13. Wind S.W.
Range of the mercury 1·23.
Mean barometrical pressure for the month .... 29·895
Spaces described by the rising and falling of the mercury............ 6·740
Greatest variation in 24 hours 0·530.—Number of changes 22.
Therm. Max. 56° Jan. 18 and 25. Wind SW.—Min. 29° Jan. 9. Wind NE.
Range 27°.—Mean temp. of exter. air 44°·95. For 31 days with ⊙ in ♑ 44·05
Max. var. in 24 hours 17°·00—Mean temp. of spring water at 8 A.M. 52°·57

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air several times during the month....... 100°
Greatest dryness of the air in the afternoon of the 27th 63
Range of the index.. 37
Mean at 2 P.M. 80°·8—Mean at 8 A.M. 87°·4—Mean at 8 P.M. 89·8
—— of three observations each day at 8, 2, and 8 o'clock......... 86·0
Evaporation for the month 0·60 inch.

Rain near ground 6·710 inch.—Rain 23 feet high 6·135 inch.
Prevaling Wind S.W.

Summary of the Weather.

A clear sky, 2; fine, with various modifications of clouds, 9; an overcast sky without rain, 11½; foggy 1; rain, 7½.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
16	8	30	1	8	9	19

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
	3½	3½	6	4	7	4	2½	31

General Observations.—The first part of this month to the 17th was very wet, and generally stormy, 6·385 inches of rain having fallen here; the latter part was dry, with a humid atmosphere. From the 2nd of December last to the 17th instant, upwards of a perpendicular foot of rain fell here, and during that period heavy rain frequently fell throughout England, but chiefly along the southern shores; also along the shores of Ireland and Scotland, in consequence of the S.W. winds and gales having prevailed two-thirds of the time, and having often been crossed by upper winds: the result was an overflowing of the rivers, and a general inundation of the low lands. Even the level roads and lands near the hilly districts in Hampshire, and the adjoining counties, lay under water several days about the middle of the month, which rendered them impassable to foot-passengers.

On the 1st instant, 1·89 inch of rain fell between the hours of 8 A.M. and 6 P.M., accompanied with a brisk gale from the S.E. in the morning, and a very hard gale from the North throughout the afternoon and night. That depth of rain in so short a time is with us unprecedented; but nearly the same depth fell here in 24 hours on the 29th of August 1821.—A more violent gale and rain in the afternoon, (as the latter frequently came down in torrents), have seldom been experienced: and in consequence of the then highly saturated state of the ground, the level roads and fields about the town and neighbourhood lay under water several hours, and carriages were prevented from travelling in the roads till the rush of water from the higher ground had subsided. On the morning of the 5th, icy efflorescences appeared on the inside of the glass windows; and at mid-day a bright parhelion was observed on the eastern side of the sun, 22½ degrees distant from his centre, From the 6th to the 10th the atmosphere presented a snowy appearance, and on the latter day one inch and a half of snow fell here, with a gale from the North-east. Early in the afternoon of the 11th a thick fog came on, but was soon dispersed by a change of wind to the West; and from a sudden rise of temperature the ice and snow were dissolved in four or five hours. While the thaw was thus rapidly going on, and immediately after the fog cleared away, a dense *stratus*, two or three feet in height from the ground, formed in the town and its vicinity, which viewed an hour before sunset from a height of twenty feet, had an unusually whitish appearance, like smoke from a tobacco-pipe. It was no doubt produced so suddenly, by the heat from the ground communicating with the lower atmosphere, whose temperature was only 40 degrees; while the heat of the ground, which had been kept down by the ice and snow, was 13 degrees higher. A wet fog prevailed throughout the day of the 12th: its temperature on the ground at 9 A.M. was 40 degrees; at three feet high, 43 degrees; and at nine and twelve feet high, 46 degrees. At midnight vivid lightning appeared in the W.S.W. horizon, preceded by two winds crossing each other, the upper one from that point, and the lower one from the South-east. Several black thunder-clouds also appeared at the same time at a great distance to the westward, which, with a rapid fall of the quicksilver in the barometer, indicated an approaching storm. The thunder-storm and hurricane are said to have come on at Plymouth soon after midnight, and to have lasted three hours with increasing violence, so that not only the ships and vessels at that port received considerable damage, but those in the Atlantic Ocean, over which the hurricane came, as since ascertained. At 4 o'clock the following morning a very heavy gale from the S.W. was felt here, accompanied with heavy rain, vivid lightning, and long peals of thunder, from half-past five till half-past six: and in three hours afterward the storm was very awful at Dover, and severely felt in the Straits; so that it was more or less violent along the whole southern coast. It is remarkable that the morning tide here, and at Plymouth, on this occasion, was very nearly as high as any of the following spring tides, notwithstanding it was a neap tide.

The mean temperature of the external air this month is unprecedentedly high; indeed the mean of March in several preceding years was much lower, and it certainly felt more like a spring than a winter month, which has been verified in some measure by the unusual appearance of spring flowers. This may be justly attributed to three co-operating circumstances, viz. the prevalence of warm winds, as may be seen by the above scale; the wet state of the ground, and the consequent humidity of the contiguous air; and the uncommonly high temperature of the ground, from there having been but little frost this winter to diminish its heat. The *maximum* temperature of the air has occurred four times by night instead of in the day.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are two parhelia, one lunar halo, two meteors, and eleven gales of wind, or days on which they have prevailed; namely, one from the North, five from the North-east, two from the South-east, and three from South-west.

REMARKS.

London.—Jan. 1. Fair. 2. Foggy. 3. Fair. 4. Rain. 5. Snow. 6—9. Cloudy. 10. Fine. 11. Snow. 12. Fog. 13—15. Cloudy. 16. Snow. 17, 18. Rain. 19—21. Fine. 22. Cloudy. 23. Fine. 24. Cloudy. 25. Rain. 27. Fine. 28. Foggy, 29. Rain. 30, 31. Fine.

Penzance.—Jan. 1. Heavy rain: fair. 2. Fair. 3. Rain: hail-showers. 4. Hail-showers. 5. Fair: rain. 6. Clear. 7. Rain: blowing strong. 8. Fair. 9. Fair: rain. 10. Rain. 11. Clear. 12. Cloudy: rain: stormy at night. 13. Rain. 14. Cloudy: rain. 15. Rain. 16. Clear: rain. 17. Rain. 18. Rain: clear. 19. Fair. 20, 21, Clear. 22. Cloudy: clear. 23. Clear: misty. 24. Rain. 25. Rain: clear. 26. Clear: 27, 28. Fair. 29, 30. Clear. 31. Rain.—Rain-gauge ground level.

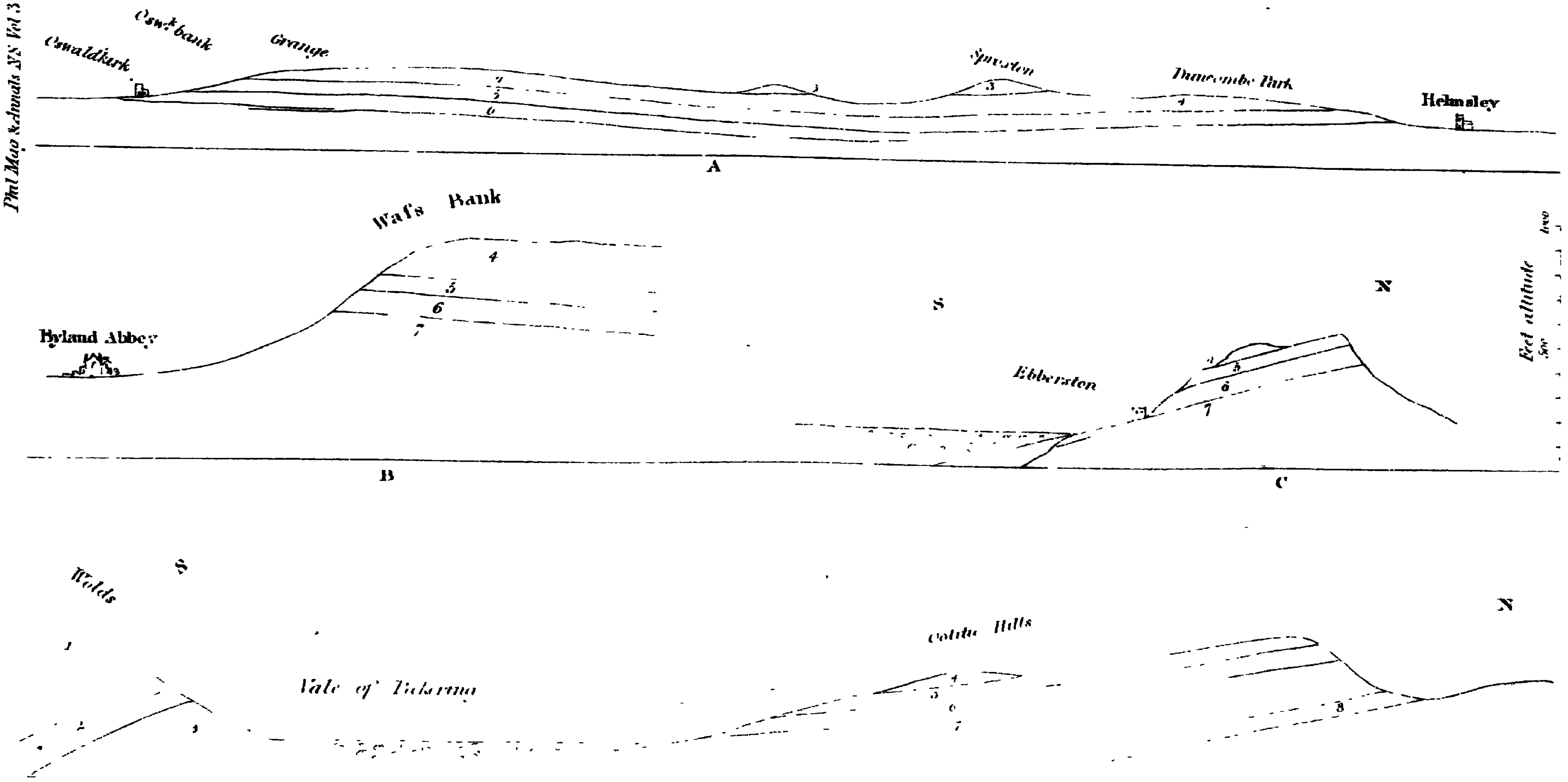
Boston.—Jan. 1. Rain. 2. Fine: rain P.M. 3. Fine. 4. Fine: rain P.M. 5—8. Cloudy. 9. Cloudy: snow P.M. 10. Fine. 11. Snow. 12. Fine: rain early A.M. 13. Rain. 14. Cloudy: rain P.M. 15, 16. Cloudy. 17. Rain and stormy. 18. Rain. 19—23. Fine. 24. Cloudy: rain A.M. 25—28. Fine. 29. Cloudy. 30. Fine. 31. Cloudy.

Owing to the Discontinuance of Mr. Howard's *Observations, our Meteorological Table has some deficiencies, which we hope to supply in our next.*

Meteoro-

Meteorological Observations by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1828.	Barometer.							Thermometer.							Wind.				Evapor.		Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost. 8½ A.M.	Lond.	Penz.	Gosp.	Bost.	Lond.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	9 A.M.	10 P.M.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.											
Jan. 1	29·48	29·60	29·60	29·26	29·63	29·23	29·22	47	36	50	47	52	37	43·5	NE.	NE.	SE.	calm		...		0·880	1·890	0·22
○ 2	29·77	29·41	29·80	29·50	29·83	29·48	29·53	50	45	48	41	51	45	36	N.	W.	SW.	calm		...		...	·400	0·13
3	29·56	29·60	29·80	29·50	29·75	29·42	29·03	50	34	50	44	48	36	43·5	NW.	NW.	NW.	NW.		0·05		0·725	...	...
4	29·73	29·52	29·70	29·70	29·80	29·60	29·97	44	33	46	42	46	33	34	W.	NW.	S.	NW.		...		...	·315	...
5	29·52	29·53	29·70	29·70	29·59	29·58	29·25	35	34	45	41	41	34	38	W.	W.	NW.	NW.		...		...	·020	·15
6	29·54	29·81	29·70	29·64	29·78	29·64	29·47	39	33	45	34	40	35	37	W.	W.	NE.	NW.		...		...	·020	...
7	29·86	29·74	29·50	29·50	29·83	29·75	29·76	36	33	45	40	42	39	34·5	SE.	SE.	SE.	calm		...		...	...	...
8	29·75	29·85	29·70	29·64	29·85	29·76	29·70	38	33	45	42	40	33	37	SE.	N.	E.	E.		...		1·020	·020	...
9	29·85	29·77	29·73	29·72	29·90	29·84	29·82	35	29	44	40	35	29	35	SE.	SE.	E.	E.		·06		...	...	...
☾ 10	29·57	29·52	29·56	29·40	29·64	29·52	29·47	34	28	52	38	37	31	16·5	N.	SE.	NE.	calm		...		0·725	·340	...
11	29·41	29·47	29·40	29·34	29·56	29·43	29·40	40	36	54	40	48	40	22	SE.	W.	E.	calm		...		...	·010	...
12	29·63	29·51	29·60	29·40	29·70	29·50	29·43	46	42	52	45	51	46	36	SW.	SW.	SF.	SW.		·04		...	·800	·37
13	29·45	29·45	29·50	29·20	29·53	29·17	28·85	47	38	48	45	50	42	43	W.	NW.	W.	calm		...		1·250	·525	·58
14	29·37	29·40	29·50	29·40	29·52	29·40	29·22	45	35	47	45	47	39	39	NW.	N.	SE.	calm		...		0·885	·155	·10
15	29·48	29·61	29·50	29·50	29·70	29·63	29·39	37	31	49	42	42	38	34	NE.	W.	NE.	E.		...		0·430	1·120	.05
16	29·68	29·68	29·52	29·50	29·76	29·63	29·57	40	38	52	44	50	46	32·5	NE.	S.	E.	E.		...		0·180	·380	...
● 17	29·68	29·93	29·74	29·60	29·95	29·84	29·70	50	49	54	48	53	50	35	SW.	W.	SE.	E.		...		0·400	·390	·50
18	30·04	30·06	30·00	29·90	30·14	30·07	29 66	56	46	55	50	56	45	49	SW.	W.	SW.	calm		·10		0·220	...	·47
19	30·13	30·13	30·06	30·04	30·22	30·20	29·70	55	47	54	48	55	47	47	SW.	W.	SW.	W.		...		...	...	...
20			30·10	30·08	30·28	30·26	29·75			53	49	52	42	47		W.	SW.	calm		...		...	...	...
21			30·10	30·08	30·21	30·19	29·77			55	48	54	43	43·5	SW.	W.	SE.	calm		·08		...	...	...
22			30·12	30·08	30·20	30·11	29·58			56	49	55	43	48	SW.	W.	SW.	W.		...		...	...	...
☽ 23			30·20	30·20	30·35	30·26	29·72			52	47	54	42	46·5	W.	W.	W.	calm		...		...	...	...
24			30 20	30·18	30·35	30·31	29·85			52	47	55	49	43	SW.	SW.	SW.	calm		·10		0·240	...	·03
25			30·20	30·18	30·19	30·10	29·70			52	47	56	42	48	SW.	W.	SW.	W.		...		0·160	·110	...
26			30·25	30·24	30·38	30·26	29·74			51	45	51	45	41·5	SW.	NW.	W.	W.		...		...	...	...
27			30·40	30·35	30·40	30·38	29·84			48	46	53	39	48		NW.	NW.	W.		·07		...	...	...
28			30·27	30·12	30·36	30·28	29·96			50	44	52	45	36	SW.	S.	S.	calm		...		...	...	...
29			30·06	30·00	30·17	30·13	29·81			53	45	52	41	42	W.	S.	S.	calm		...		...	...	...
30			30·00	29·94	30·05	29·98	29·70			50	46	50	42	40	SW.	SW.	SE.	calm		...		...	...	...
31			29·86	29·84	29·96	29·94	29·53			52	47	52	49	44	SW.	SW.	SW.	S.		·10		0·340	·215	...
Aver.:			30·40	29·20	30·40	29·17	30·22			55	34	56	29	39·3						0·60		7·455	6·710	2,60


Oswaldkirk
Osw.k bank
Grange
4
5
6
3
Sproxton
3
Duncombe Park
4
Helmsley
A
Waſs Bank
4
5
6
7
Byland Abbey
S
Ebberston
4
5
6
7
N
Feet altitude
500
1000
B
C
S
Wolds
1
2
1
Vale of Pickering
Oolite Hills
4
5
6
7
8
N

S Petit

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

APRIL 1828.

XXXIX. *A Letter to the Editors relating to the Ellipticity of the Earth as deduced from Experiments with the Pendulum.*

By J. Ivory, *M.A. F.R.S.*

Gentlemen,

IN examining the calculations in the articles relative to the ellipticity of the earth, inserted in the last Number of this Journal, I have detected a small arithmetical error, which, although of little moment, it may be proper to correct. The equation at the bottom of p. 170, viz.

$$2{\cdot}973\,\tau = -{\cdot}00061,$$

should be, $2{\cdot}973\,\tau = -{\cdot}00033$:

consequently, $\tau = -{\cdot}00011$, $s = +\,00005$,

$f = {\cdot}2056$, $\Delta = {\cdot}01335$;

and the formula for the length of the pendulum,

$$l = 39{\cdot}01335 + {\cdot}2056 \sin^2 \lambda.$$

But the error just corrected is so small as not to affect either the reasoning deduced from the formula at the top of p. 171, or any of the results obtained by it.

It is directed in p. 207, that the two approximate quantities a and b, be so taken as to satisfy equation (A): but it is obvious that, although this be convenient, it is not necessary in the method of computation I have followed. Nothing more is required than that a and b nearly satisfy equation (A); in which case the right-hand side of equation (B), will not be equal to zero, but to a small known quantity positive or negative,—a circumstance which makes no essential difference in finding the corrections s and τ.

From the calculations which have been made, we may infer that no great confidence can be placed in the ellipticity deduced from any partial combination of the experiments with the pendulum: for we have found that there is considerable uncertainty in the value of that element, even when we avail ourselves of all the experiments we at present possess. This matter will be placed in a clear light by setting before the reader, the values of Δ, or of the excess of the pendulum at the equator above 39 inches, which we have obtained by taking different means of the tropical experiments, viz.

$$\begin{array}{c} \Delta \\ \left.\begin{array}{r} \cdot01230 \\ \cdot01330 \\ \cdot01605 \end{array}\right\} \end{array} \text{mean of} \left\{\begin{array}{l} 6 \text{ experiments.} \\ 9 \\ 15 \end{array}\right.$$

The true value of the equatorial pendulum is therefore very uncertain. If we take the first mean of six tropical experiments, and neglect the other nine, we shall obtain, by combining the 31 remaining experiments, a formula for the length of the pendulum coinciding very nearly with that published in this Journal for October 1826, the ellipticity being about $\frac{1}{300}$. The other two means are the bases of the calculations in the two papers inserted in the last Number of this Journal. A strict scrutiny of the experiments would perhaps bear us out in adopting $\frac{1}{300}$ as the ellipticity that best agreed with the bulk of them; but any ellipticity between $\frac{1}{300}$ and $\frac{1}{296}$ will represent sufficiently well 34 out of the 40 experiments in our possession. The ellipticity $\frac{1}{289}$ does not represent with much accuracy, either the six anomalous experiments, or the remaining 34; and in the present state of our knowledge, we may consider it a thing nearly demonstrated, that this value is too great.

What is most remarkable in the experiments with the pendulum, is their excessive irregularity near the equator. This is well illustrated by the four values of Δ following, which are experimental quantities, not the results of calculation, viz.

	Longitude.	Δ
Maranham	44° 21′ W.	·01173
Rawak...............	131 1 E.	·01479
Galapagos	90 0 W.	·01717
St. Thomas.........	6 45 E.	·02074

The longitudes are set down, as all the stations are so near the equator, that they may be reckoned upon it. If the same irregularity shall be found to prevail upon all the parallels, it must be acknowledged that the figure of the earth, deduced from such incoherent data, will lose much of the interest and utility, which are usually attached to it. But no such irregularity has hitherto been experienced beyond the tropics; and even between the tropics there are only a few stations at which so great an excess of gravity has been observed as excludes them from belonging to the same surface as the other experiments. I apprehend it cannot be said at present that the great anomalies alluded to, are so well ascertained as to render any further inquiry unnecessary in regard to their exact quantity, or to the causes which produce them: and the determination of these points in preference to any other, seems to claim the attention of the experimenter; because it must fix in a great degree the complexion of the whole theory. Can we hope to determine an elliptical surface that will represent all the experiments within the limits of the probable errors of observation? Or, must we be content with a mean elliptical figure liable to great discrepancies? These seem at present to be the most interesting questions in this research. The least attention to the position of the anomalous stations on the surface of the globe, will prove how fruitless it would be, to suppose that the experiments can be better represented by a figure different from the elliptical spheroid. Far less can we expect, in the present state of our knowledge, to attain any useful purpose by pushing the theoretical solution of the figure of the earth to quantities of the second order; because the corrections thus introduced must be ultimately determined by the experiments themselves, the uncertainties of which greatly surpass the quantities to be found.

I remain, Gentlemen,
Your obedient servant,
March 12, 1828. J. Ivory.

XL. *Remarks on the Geology of the North Side of the Vale of Pickering.* *By* John Phillips, *F.G.S., Keeper of the Museum of the Yorkshire Philosophical Society.*[*]

[With an Engraving.]

THE principal object of this communication is to explain some of the peculiar appearances which are exhibited along the southern edge of the oolitic hills which margin the Vale of Pickering from Seamer to Helmsley.

My attention was first drawn to a part of this country by a visit to Kirkdale Cave, in company with Mr. Salmond and Mr. Smith, in March 1824. In August of the same year I had further opportunity of examining with the aid of a barometer, the whole line during my walk from York to Scarborough.

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[*] Read to the Yorkshire Philosophical Society, Jan. 1, 1828; and communicated by the Author.

In September 1825, I again traversed the line from Craike by Wass Bank to Helmsley; in September 1826, the Rev. W. V. Vernon and myself twice observed with attention the country about Ebberston and Snainton; and in October 1827, I very carefully reviewed with a barometer in my hand the whole line from Scarborough to Brandsby.

The roads from Craike and Brandsby to Helmsley, and from Helmsley to Scarborough, are lines remarkably well adapted for inquiring into the stratification of the country in question, and slight deviations to the right and left are easily made.

Some results of these several investigations I now beg to submit to the consideration of the Society.

Four strata or formations as usually enumerated, are seen along the north side of the Vale of Pickering.

Kimmeridge clay.
Coralline oolite.
Calcareous grit.
Oxford clay.

Of these the Kimmeridge clay, coralline oolite, and calcareous grit, may be considered as well identified by imbedded fossils with the strata bearing those names in the midland and southern parts of England. The Oxford clay is defined by its position between the well identified calcareous grit and Kelloways rock, and contains besides some ammonites which locally serve to characterize it.

Kimmeridge Clay.—It was in 1824 that I first had the pleasure of finding in the Kimmeridge clay, where it had been exposed by cutting the road at the east end of Kirby Moorside, plenty of characteristic pieces of *Ostrea deltoidea*, a fossil which Mr. Smith and myself had always considered to mark this stratum. The clay here is thin, contains minute layers of brown and white sandy lumps, in the upper part of which lies an ammonite like *A. plicomphalus* (Min. Conch.), and covers brown sandy stone incumbent on coralline oolite. The clay rises to the north, and forms the little insular hills which have recently afforded new examples of *Ostrea deltoidea*. From Kirby to Sinnington it retires to the south of the road, but beyond Sinnington forms a hill nearly a hundred and twenty feet high above the stream which there exposes the uppermost beds of the oolitic series beneath. From this point its course is wholly on the south of the road, but so much concealed by the alluvial and diluvial covering, that it would be a difficult matter to represent it on a map with even tolerable accuracy. It is seen on the coast at intervals from Filey to the Speeton Cliffs; but *Ostrea deltoidea* has never yet been found there. Unless at about one mile east of Helmsley, the Kimmeridge clay lies wholly to the south of the road from Kirby Moorside to Sproxton, one mile south of Helmsley. In this clay hill I found no opportunity of searching for fossils, but about a mile further west, where the road crosses a low insulated woody hill, I had the satisfaction of finding, in a broken clay bank, a layer of *Ostrea deltoidea* in tolerable perfection. This is the fifth example within my own knowledge of this remarkable fossil being found in the Kimmeridge clay of Yorkshire. Its true position here, as at Heddington and other places in the south of England, is very near the top of the oolitic formation which lies beneath; and I have no doubt it would be found in several other places, and in greater plenty, if it were carefully sought after. From the hill above mentioned, the Kimmeridge clay turns away toward Malton, and accompanies the eastern boundary of the coralline oolite.

The Oolitic Rocks rise from out of the Vale of Pickering to the north and to the west. Every one accustomed to practical geological investigation will at once recognize in the long slopes from Hambleton and Oswaldkirk, the Terrace of Rievaulx, and the sides of Newton dale, the characters of regular declination. There has been a general mistake respecting the order of succession of the members of the coralline oolite series in this tract of country. It has been generally believed that the uppermost beds of the series are coralline or shelly limestone, such as is seen in the quarries near Scarborough and Malton. But in truth, these oolitic beds are separated from the Kimmeridge clay by a considerable thickness of sandy calcareous and ferruginous beds, containing some fossils analogous to those which lie in the calcareous grit beneath the oolite.

This fact is evident on either the Gilling or Coxwold road to Helmsley. Oswaldkirk bank on the former road shows oolite under a considerable covering of brown sandstone, which continuing beyond Grange is twice covered by detached hills of Kimmeridge clay, but never once exposes the subjacent oolite till we descend to the town of Helmsley (see Plate V. section A). On the Coxwold road, after ascending nearly the whole height of the romantic Wass Bank, we notice the Oxford clay, surmounted by mural precipices of calcareous grit, and at the brow of the hill a quarry of coralline oolite covered by a hundred feet of brown and yellow sandstone. This forms the poor heathy moors to the north-west, and continuing N.E. towards Helmsley, presents to that town an *escarpment on the declining side* which exposes the coralline oolite (see section B).

Here then we first find indications of a denudation of a very interesting character: a denudation on the dipping edge of the strata which has produced here and there escarpments looking toward the formations which cover their continuous slopes. This principle will, I am satisfied, be admitted by all who carefully consider the facts; and it avoids the impropriety of supposing a dislocation without evidence. According as the denudation has extended more or less toward the "rise" of the strata, more or fewer beds of the oolitic series are exposed: where the denudation has produced the greatest effect, the lowest stratum exposed is Oxford clay.

A similar escarpment is found on the Rievaulx road from Helmsley as on the Oswaldkirk road; but long slopes of coralline oolite and brown sandstone over it cross the road to Bladlam. Hence to the valley of Kirkdale the same brown stone lies on all the higher parts of the road, and in a soft condition covers the oolite in the quarry at the famous cave. A mean of three measures very carefully made in 1824, gave me thirty-one feet for the height of the cave above the bed of the stream. (The statement of eighty feet in Dr. Buckland's account is probably a typographical error.) Nearly fifty feet of oolite are here exposed, and there are about twenty feet of sand and sandstone above. The brown stone continues below Manor-dale to Kirby Moorside, where it is covered by Kimmeridge clay.

In the stream which flows through the heathy common on the road from Kirby Moorside to Sinnington, geologists may observe in a very satisfactory manner the relation of this sandstone to the coralline oolite. The road-bridge over the stream stands on irregular sandstone layers, containing a few fragmented shells. Rising toward the north at a greater angle than the slope of the valley, fresh beds are exposed in succession as we proceed, and at length below six feet of yellow sand we find the uppermost very solid bed of oolite full of *Turritellæ*, *Melaniæ*, spines of *Cidaris* and *Trigonia costata*, as at Helmsley. A few beds lower lies a coarse variety of *Ostrea gregaria?* (Min. Conch.) At about half a mile from the road these observations may be repeated in the large limestone quarries, where the remarkable turbiniferous bed covers the other calcareous layers. Some *Turritellæ* also occur in the lower beds of the rock, which occupies the whole stream and sides of the valley, being covered by the heathy cap of sandstones. In the bank of the stream at Sinnington these sandstone beds are again exposed dipping south, and afford some fossils extremely similar to those which accompany the lower calcareous grit; particularly ammonites and pectines. Beyond Sinnington, the road crosses a hill of Kimmeridge clay one hundred and twelve feet above the stream; then descending

ing almost as many feet, it continues by Wretton and Aislaby on the long slopes of the same sandstone series to Pickering.

It thus appears that from Helmsley to Pickering the effects of denudation on the dipping edge of the oolitic series are scarcely at all observable; the beds declining regularly, and almost without interruption, into the Vale of Pickering. The denudation of Helmsley appears to be connected with the formation of the valleys about that town; but from Pickering eastward, a more extensive operation of similar causes has produced a long range of escarpments crossing the direction of the little existing valleys, but parallel to the general line of the great Vale of Pickering.

The valley descending to the south by the town of Pickering, presents good opportunities of observation. The strata on the western side are much lower than on the eastern, and slope south into the Vale of Pickering, without presenting any escarpment. On the eastern side calcareous grit is quarried beneath the limestone, a thin covering of the upper sandstones mixed with clay lies above that rock, and the whole series, sloping south into the Vale of Pickering, has been subjected to denudation parallel to the range and on the declining side. The effect of this denudation becomes extremely evident as we proceed further toward Scarborough. Before reaching the village of Thornton we cross a hill which appears to me to be formed of the Oxford clay. At Allerston, springs issue from the top of it, beneath the calcareous grit which is found on all the little risings to Ebberston, while the clay fills the valleys. At Ebberston, as at Allerston, springs issue from beneath the calcareous grit, and flow southward over the Oxford clay, in which stratum Mr. Vernon and myself found *Mya V-scripta*, and a peculiar ammonite as at Scarborough (see section C). At Parson's Houses, between Ebberston and Allerston, a well was sunk twenty-one yards through the clay of Ebberston to a brown freestone rock, which I suppose to be Kelloways stone.

Above the line of springs at Ebberston, calcareous grit in its usual characters with calcedonized ammonites occupies a good height in the hill, and is surmounted as usual by the coralline oolite, here full of *Ostreæ* in the lower part, and of *Melaniæ* and *Turritellæ* at the top. This escarpment, showing calcareous grit and coralline oolite, is very distinct above and north of the village of Snainton; and I think may be traced, though less evidently, by Brompton and Wykeham to the old Tower at West Ayton. After this, the long slopes of calcareous grit, descending from Falsgrave Moor and Oliver's Mount, are covered by parallel beds of oolite at Seamer, sloping down into the Vale of Pickering without any denudated edge.

Between these sloping beds of the oolitic range as they continue to Filey Brig, and the projection of chalk from Hunmanby, the Vale of Pickering is much narrowed; and where it opens into the sea exposes such a vast quantity of diluvium as to cover almost all the beds of oolite, and effectually to hide the junction of this series with the Kimmeridge clay. This stratum is seen irregularly in the cliffs peeping out from under its huge load of diluvium, and at length near Speeton is exposed in very decided characters, lying immediately beneath the chalk.

I have no doubt that an impartial examination of this line of country will lead to the adoption of my opinion, that the pseudo-escarpments of the oolitic rocks on the north side of the Vale of Pickering are caused by denudation nearly parallel to their ranges, and that no general dislocation is at all concerned in the appearances. Even a new *zigzag fault* would add nothing to the explanation, the facts remain just as before: sometimes the dip of the strata is uninterrupted till the Kimmeridge clay, as at Sproxton and Kirby Moorside, lies on the oolitic rocks; in other cases the declining surface is interrupted, and a pseudo-escarpment results, exhibiting coralline oolite alone as at Helmsley, or calcareous grit beneath it as at Pickering, or even Oxford clay beneath that as at Ebberston. The strata re-commence no doubt on the south of this last place; but they are not seen, because from the breadth of the denudation the opposite edges are sunk much below the general level of the vale, and covered by diluvial and alluvial accumulations. These accumulations prevent the Kimmeridge clay from being seen in the flat central part of the vale; but on its southern side where the ground rises to the Wolds this stratum may be very well traced. (See Plate IV. section D.)

The inferences from these results connect themselves closely with general views on denudation and diluvial action, but it seems improper to enter into a discussion of them on such an insulated example. This is not the only instance which may be brought forward when the details have been more carefully studied. Till then, I must content myself with referring to the accompanying sections in illustration of these remarks, and have only further to observe that they were all drawn on the spot.—One general explanation will apply to them all:

Plate V. 1. White chalk.
2. Red chalk.
3. Kimmeridge clay.
4. Upper calcareous grit.
5. Coralline oolite.
6. Lower calcareous grit.
7. Oxford clay.

8. Kelloways rock, which appears on the northern edge of the oolitic hills, but is not seen at the surface in the Vale of Pickering.

Dec. 1827.

XLI. *A new Theory of the Resistance of Fluids, compared with the best Experiments. By Mr.* Thomas Tredgold, *Civil Engineer; Hon. M. Inst. Civ. En., and of the Liter. and Phil. Society of Newcastle-upon-Tyne.*[*]

[With an Engraving.]

1. THE following is an account of some inquiries respecting the resistance of fluids, which were undertaken to enable me to render more effectual assistance to those who consult me on subjects where an accurate knowledge of this resistance is of importance: a further motive to proceed in the research was the extensive application of which it is susceptible among the wants of a commercial nation. With a like sense of its utility it was long ago remarked by Dubuat, "that to determine the resistance which a body in motion experiences in a fluid at rest, is one of the most important problems of general mechanics;" and since that time it has acquired still greater interest, in consequence of the application of a new species of power to propel vessels at sea.

2. Since I commenced the investigation, the French Academy of Sciences have offered a premium for the best series of experiments on the resistance of fluids; and certainly a good series of experiments under a great variation of conditions would be valuable; but when they are not made for the express purpose of determining particular truths, or data, they become of little value in assisting the progress of scientific investigation. The object of an experiment, like that of an equation, is the determination of something unknown; the same principles apply to both cases. The fine discoveries which made chemistry a science, resulted from experiment conducted by the methodical system of the mathematicians, though not in the same technical form.

I have felt the defects of the miscellaneous system of experiment, in attempting to correct and confirm my own inquiries; but have compared them with the best I am acquainted with.

3. When a body is moved forward in a fluid, the parts of the fluid before the body must be displaced; the force required to produce this effect is the measure of the *direct resistance.*

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[*] Read to the Lit. and Phil. Society of Newcastle-upon-Tyne; and communicated by the Author.

4. The vacancy left by the motion of the body is filled by the motion of the fluid; but, with a loss of the force necessary to maintain an equilibrium among the parts of the fluid, equivalent to the force producing this motion in the fluid. The increase of resistance from this cause has been termed the *minus pressure of the fluid.*

5. The sum of the direct resistance, and that due to the deficiency of equilibrium, called the minus pressure, is the total resistance when the friction is neglected.

6. If fluids were devoid of friction they would be perfectly elastic, differing only in the degree of compression produced by a given force; hence, in considering the effect of collision on a fluid, it must be esteemed an elastic body. The coherence and friction of natural fluids render their elasticity in a slight degree imperfect, but not sufficiently so to interfere with the application of rules founded on its perfect elasticity in most of the cases occurring in practice. The total want of elasticity is incompatible with the very nature of a fluid.

7. If a body moving with an uniform velocity in a straight line AB, Plate IV. fig. 1. impinge on a filament of a fluid in which it moves, with a part of its surface inclined in any manner to the direction of the motion, the fluid will be reflected, and the direction of the resultant of the impingeing and reflecting forces will be perpendicular to the surface, and equal to the pressure of a column of the fluid capable of producing the actual velocity of the body. For the fluid being elastic, the restoration of figure produces the reflecting force, and the force in a direction perpendicular to the surface is only half that which would arise from the collision of a non-elastic body.

8. *Of the direct Resistance.*—Let v be the difference between the velocity of the fluid and that of the body, when estimated in the direction of the motion; $g =$ the space described in a second by the velocity due to the force of gravity *in vacuo* $= 32\frac{1}{6}$ feet; $a =$ the angle the portion of the surface, on which the filament of fluid strikes, makes with the direction of the motion; and $h =$ the height of the column of fluid equal to the direct resistance. The velocity of the body is to that of the surface in a direction perpendicular to itself; or in the direction of the resultant as $v : v \sin a$; and $\frac{v^2 \sin^2 a}{2g} =$ the column of fluid whose weight would be equivalent to the perpendicular pressure. This pressure reduced to the direction of the motion is $h = \frac{v^2 \sin^3 a}{2g}$, the forces perpendicular to the direction of the motion being supposed to mutually balance one another.

9. *Of the Minus Pressure.*—It is obvious, from the property of figures having parallel sides, that the quantity of fluid required to fill the vacancy left behind the body when it moves, is a constant quantity, whatever be the form of the body; therefore the deficiency of pressure will vary only with the velocity the fluid must acquire to follow the retiring surface. But if c be the angle that surface makes with the direction of the motion, the velocity will be $v \sin c$; consequently, the motion of the fluid being established, the resistance from the deficiency will be $\frac{v^2 \sin^2 c}{4g}$.

Combining the two resistances, we have the height of the column $= \frac{v^2}{4g}(2 \sin^3 a + \sin^2 c) + F = H$; where F is the friction. When the body is a cube or a cylinder, with the ends perpendicular to the direction of the motion, the angles become 90°, and neglecting the friction $\frac{3v^2}{4g} = H$.

The height due to the resistance being to the height due to the velocity as 3 : 2.

10. If the height of a column that would balance the friction be x feet, and H be the height producing the motion, then $H - x = \frac{v^2}{4g}(2 \sin^3 a + \sin^2 c)$; or $x = H - \frac{v^2}{4g}(2 \sin^3 a + \sin^2 c)$.

Assuming that the friction varies as the square of the velocity, and as the surface of the body; and putting $f =$ the height of a column of water whose weight is equal to the friction of one foot of surface when the velocity is one foot per second; then $\frac{f}{s}$ will be the height when distributed over the whole section of the moving body, when s is the area of that section; therefore, if lp be the area of the surface, $\frac{flpv^2}{s} = x$.

11. There is also the friction of the fluid itself to be considered when the velocity is considerable; and without being able to assign a satisfactory reason for it, if we consider the length to be increased by a quantity Av, where A is a coefficient depending on the nature of the fluid, the result agrees nearly with experiment, and the value of x including this effect is

$$x = \frac{fpv^2(l + Av)}{s}; \text{ and consequently,}$$

$$v^2\left(\frac{2 \sin^3 a + \sin^2 c}{4g} + \frac{fp(l + Av)}{s}\right) = H.$$

Before attempting to determine the constant quantities from experiment, it will be useful to consider the different cases corresponding to the experiments to be compared.

12. If a plane of given area, and equal thickness, be set at different angles to the direction of the motion, the resistance will be expressed by $\frac{v^2}{4g}\left\{2(\sin^2 a + \cos^2 a) + \sin a + \cos a\right\} = \text{H}$ when the effect of friction is neglected. (See Plate IV. fig. 3.)

When the thickness is inconsiderable, the effect of the edge may be neglected, and then $\frac{v^2}{4g}(2\sin^2 a + \sin a) = \text{H}$.

13. In the case of a parallelepiped, with a wedge-formed prow attached, we have $\sin c = 1$; (see fig. 2.) and,

$$\frac{v^2}{4g}(2\sin^3 a + 1) = \text{H}.$$

Including the friction $\frac{v^2}{4g}\left(2\sin^3 a + 1 + \frac{4gfp\left(l + \frac{b}{4\sin a}\right)}{s}\right) = \text{H}$.

The same equations will apply when a cylinder terminates at one end in a cone.

14. The resistances of curved surfaces may also be computed: for example, let it be the resistance of a sphere;—put $p = 3{\cdot}14159$, and $y =$ the variable radius of the base, and $2py\dot{y} =$ the fluxion of its area. Then the figure being a circle, and x the abscissa measured on the direction of the body's motion (fig. 4.) $\frac{r-x}{r} = \sin a$; and $y\dot{y} = (r-x)\dot{x}$; consequently,

$$\frac{2pv^2}{4g}\left(\frac{\overline{2r-x}^{\,4.}\,x}{r^3} + \frac{\overline{r-x}^{\,3.}\,x}{r^2}\right) = \text{H} \times 2py\dot{y}.$$

The fluents are,

$$\frac{2pv^2}{4g}\left\{\frac{2\left(r^5-(r-x)^5\right)}{5r^3} + \frac{r^4-(r-x)^4}{4r^2}\right\} = \text{H}py^2.$$

And, when $x = r$,

$$\frac{v^2pr^2}{4g}\left(\frac{4}{5} + \frac{1}{2}\right) = \frac{1{\cdot}3\,v^2pr^2}{4g} = \text{H}pr^2;\ \text{or}\ \frac{1{\cdot}3\,v^2}{4g} = \text{H}.$$

The resistance of a sphere is to the resistance of a cylinder of the same diameter as 1·3 : 3, or as 1 : 2·308; or as ·43$\dot{3}$: 1.

15. If a cylinder have a hemispherical end or prow, then $\sin c$ becomes unity, and substituting this value of it in the equation we have,

$$\frac{v^2}{4g}\left(\frac{4}{5} + 1\right) = \frac{1{\cdot}8\,v^2}{4g} = \text{H}.$$

16. And, if the motion be reversed, or the flat end go forward,

$$\frac{v^2}{4g}\left(2 + \frac{1}{2}\right) = \frac{2{\cdot}5\,v^2}{4g} = \text{H}.$$

17. According to this investigation we have therefore the following order of resistance.

A cylinder with flat ends	3	or 1·00
A cylinder with the hind part a hemisphere	2·5	or 0·833̇
A cylinder with the fore part a hemisphere	1·8	or 0·60
A sphere	1·3	or 0·433

These ratios are likely to be altered a little by the effect of friction; and if the cylinder be reduced in length till it becomes a thin plate (fig. 9. and 10.), a still greater alteration is caused by the interference of the two motions of the fluid. The effect is easily observed by moving differently-formed bodies in water.

18. When a cylinder moves in a direction perpendicular to its axis (fig. 7. and 8.), making d its length, we have $2d\dot{x} =$ the fluxion of its area, and $\frac{y}{r} = \sin a = \sin c$; hence,

$$\frac{v^2}{4g}\left(\frac{4dy^4\dot{y}}{r^3(r^2-y^2)^{\frac{1}{2}}} + \frac{2dy^3\dot{y}}{r^2(r^2-y^2)^{\frac{1}{2}}}\right) = \mathrm{H}\times 2d\dot{x}.$$

The fluents are, when z is the arc of the curve,

$$\frac{2dv^2}{4g}\left(\frac{3r^3z-(r-x).(3r^2y+2y^3)}{4r^3} + \frac{2r^3-2(r-x)^3-3y^2(r-x)}{3r^2}\right)$$
$$= \mathrm{H}\times 2dx.$$

In this form the equation applies to the curved ends of canal boats having flat bottoms, the curves being usually portions of circles; and an approximate equation in the following form is easily applied.

Let $mr =$ half the breadth of the boat, r being the radius of curvature; then

$$\frac{2dv^2}{4g}\left\{\frac{3z-r(1-m).(3-4m)\sqrt{2m}}{4}+\frac{r(2-2(1-m)((1-m)^2+3m))}{3}\right\}=$$

$2d\mathrm{H}x$. In the ordinary boats $m = {\cdot}125$, and therefore,

$$\frac{2dv^2}{4g}\left(\frac{3z-1{\cdot}1629r}{4}\right) = 2l\mathrm{H}x.$$

From these equations I have been enabled to compare some experiments made by Mr. Bevan on the power required to draw canal boats; which will be detailed after treating of the friction of bodies moving in fluids.

When a perfect cylinder is the form of the body, then $r = x$ and $p = 3{\cdot}14159r$, we have,

$$\frac{v^2}{4g}\left(\frac{3p}{8}+\frac{2}{3}\right) = \frac{1{\cdot}8448v^2}{4g} = \mathrm{H}.$$

19. *Of the Friction of Fluids.*—A series of experiments on

the friction of water were made by Col. Beaufoy*, from whence it appears that the friction of water is very nearly as the square of the velocity; and about ·0032 pounds for one foot of surface moving at the rate of one foot per second. It is somewhat greater than this in slow motions; most likely from the effect of cohesion being sensible in small velocities. The experiments made by the Society for the Improvement of Naval Architecture do not appear to be so nearly in the ratio of the square of the velocity, but at the velocity of four nautical miles per hour the resistance is exactly ·0032 pounds (see Dr. Young's Nat. Phil. ii. p. 229). The head of water equivalent to this resistance is $\frac{0{\cdot}0032}{62{\cdot}5} = 0{\cdot}0000512 = f$.

20. If the resistance from the friction of the fluid itself be neglected, the general equation for water will be

$$v^2 \left\{ \frac{2 \sin^3 a + \sin^2 c}{128{\cdot}76} + \frac{0{\cdot}0000512\, p l}{s} \right\} = \mathrm{H};$$

or, $\frac{v^2}{128{\cdot}76} \left(2 \sin^3 a + \sin^2 c + 0{\cdot}0066 \frac{p l}{s} \right) = \mathrm{H}.$

When $\sin a = \sin c = \sin 90°$, the resistance from friction is to the resistance from pressure as $3 : 0{\cdot}0066 \frac{p l}{s}$.

21. The value of the coefficient A, for water is of very little use, neither are there good experiments for determining it; in the resistance of air, however, it becomes of importance. The equation for air, when the length of the body may be neglected, is

$$\frac{v^2}{4g} \left(\sin^3 a + \sin^2 c + \frac{4 g f \mathrm{A} p v}{s} \right) = \mathrm{H}.$$

And if n be the resistance of the body compared with a plane of equal section when the friction is neglected, the resistance of the plane being unity, the best experiments give $4 g f \mathrm{A} = 0{\cdot}000000332\, n$. The results of experiments, however, differ considerably; and it is difficult to fix on a datum which it never was their object to determine.

22. *Comparison with Experiments.*—The experiments of Bossut† were made with a parallelepiped four feet in length, two feet in breadth, and two feet draught of water. It was first tried alone, and afterwards with wedge-shaped prows of different degrees of acuteness (see fig. 2.); the time of describing ninety-six feet was observed, when the velocity had become uniform, except in the experiments with the most acute angles

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* Dr. Thomson's Annals of Philosophy, vol. vi. p. 281, 1815.

† *Traité D'Hydrodynamique*, tom. ii. p. 394 - 411.

in which the time of describing seventy-two feet was observed. Five trials were made with each change of form, except with the most acute prows, which were tried only thrice, and in the latter the results were not so regular as the others. The following table contains the mean resistances as Bossut has collected them, the resistance of the parallelepiped being denoted by 10000.

The equation applying to this case is $\frac{v^2}{128\cdot76}\, 2\sin^3 a + 1 + 0\cdot0066\left(\frac{p\left(l+\frac{1}{2\sin a}\right)}{s}\right)$; and neglecting the difference between the measures, the resistance must be 10000 when $\sin a = 1$, consequently $0\cdot3285\left(2\sin^3 a + 1 + \cdot0445\left(8+\frac{1}{\sin a}\right)\right)$ is the expression for other angles.

Angle with the Direction of the Motion.	Resistance collected from Observation.	Resistance by Calculation.		Old Theory neglecting Friction *.
		without friction.	with friction.	
90°	10000	10000	10000	10000
84	9893	9891	9893	9890
78	9578	9571	9578	9568
72	9084	9067	9083	9045
66	8446	8417	8442	8346
60	7710	7663	7700	7500
54	6925	6863	6913	6545
48	6148	6068	6131	5523
42	5433	5330	5407	4478
36	4800	4687	4777	3455
30	4404	4170	4272	2500
24	4240	3781	3897	1654
18	4142	3530	3662	955
12	4063	3393	3552	432
6	3999	3341	3578	109

The numbers in the fourth column ought to represent the resistances; and as the difference between these and Bossut's mean results are not greater than the differences among the trials with the same form, in any case we may, I think, con-

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* See Bossut's *Hydrodynamique*, tom. ii. p. 411, or Robison's Mechanical Philosophy, vol. ii. p. 295. Vince's Hydrostatics, sect. iii. Dr. Hutton's Course of Mathematics for the MilitaryAcademy, vol. ii. Problem xix. p. 353.

clude that the formula is sufficiently near for any practical purpose, as far as this species of body is concerned.

23. Some experiments were made in a very different manner by Mr. Vince*, and besides being chiefly made with thin planes, they were made by a rotary machine, which rendered it necessary to determine the centre of resistance. This he has done inaccurately; but neglecting its effect on the results, the experiments give the ratio of the height due to the resistance to be to the height due to the velocity as 3 : 2. This is the same as the ratio we have derived from theory (Art. 9.).

The experiments with a thin plane set at different angles may be compared with the equation $\frac{v^2}{4g}(2\sin^2 a + \sin a) = \mathrm{H}$; (Art. 12.) which making the resistance at 90° equal 1000, will vary as $\underline{1000\,(2\sin^2 a + \sin a)}$

Angle with the Direction.	Ratio by Experiment.	Ratio by Calculation.	Differences.
90°	1000	1000	0
80	963	975	+12
70	915	902	−13
60	820	788	−32
50	660	646	−14
40	506	489	−17
30	330	333	+ 3
20	157	192	+35
10	48	78	+30

Mr. Vince also tried the resistance of hemispheres moving in water; with the base foremost, and with the spherical side foremost, the ratio was 8339 to 3400. The resistance of a cylinder of the same diameter he found to be 7998; hence making the latter unity we have By experiment; Cylinder 1; base of hemisphere 1·05; round side ·427.

By calculation; Cylinder 1; base of hem. ·835, round side ·6.

24. The experiments of Dubuat† on the pressure of water on bodies moving in it tend to confirm the relation of 2 to 1 between the direct and minus pressure of a column capable of generating the velocity. He has shown that the pressure is different on different parts of a plane surface, and this we might expect; but the whole effect is not altered by the distribution

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* Phil. Trans. Abrid., vol. xviii. p. 250.

† *Principes d'Hydraulique*, tom. ii. partie 3.

of the resisting forces, which will vary in proportion to the facility with which the fluid can escape by reflection from the different parts of the surface.

25. The experiments made by the Society for the Improvement of Naval Architecture*, differ considerably from others, but they are most readily compared in a tabular form. The resistances were taken at a velocity of five nautical miles per hour, which is equivalent to 8·35 feet per second.

Form of the Body.	Resistance by Experiment.	Ratios.	Calculated Resistance.
	Pounds.		Pounds.
Thin square plane................	80·76	1·08	100·7
Cube................................	79·34	1·07	100·7
Thin round plane................	80·64	1·08	100·7
Cylinder............................	74·69	1·00	100·7
Cylinder and hemispherical end	56·04	·75	83·9
The same reversed	22·28	·30	60·4
Cylinder terminating in a hemisphere at both ends	18·53	·25	43·6
Sphere	25·24	·34	43·6

I have no doubt that there is an error somewhere in the mode of trial which gave the resistance stated in the account of these experiments, for they do not agree with others.

26. Col. Beaufoy made some experiments on the resistance of water, which appear to have been conducted in a similar manner. He found the resistance of a square foot, moving at the rate of one foot per second, to be 1·2949 pound; according to my mode of calculation, it should be 1·45 pound.

27. Borda, from an experiment made in sea-water, makes the resistance of a foot of surface 1⅞ pound Fr., at a velocity of about one foot per second; and Bouguer, as quoted by Robison, makes the resistance 1·44 pound Fr. My mode of calculation gives 1·785 pound Fr. for the resistance of sea-water, to a surface one foot square moving at the rate of one foot (French) per second.

28. Mr. Bevan has given me an account of some trials he made to ascertain the resistance of a canal boat on the Grand Junction Canal. The length of the boat was 69·57 feet; its width, 6·83 feet; floating depth in the water, 0·89 foot; weight, 9¾ tons; radius of curvature four times the breadth; and the area of surface in contact with the water, 540 feet. (See fig. 7.)

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* Buchanan on Propelling Vessels by Steam. 1816.

The equation rendered applicable to this case is $v^2(\cdot 242\, d(3z - 1\cdot 1629\, r) + 0\cdot 0032\, p\, l)$ = the resistance in pounds; and as $d = \cdot 89$; $z = 13\cdot 8$; $r = 27\cdot 32$, and $p\, l = 540$, it becomes $3\cdot 79\, v^2$ = the resistance in pounds.

Velocity in Feet per Second.	Resistance in Pounds by Experiment.	Calculated Resistance.
Feet.	Pounds.	Pounds.
1·31	6·1	6·49
1·98	14	14·8
2·93	28	32·5
4·3	56	70·0

Considering the complex form, the resistance is expressed very well by the equation, and quite as nearly as a general rule could be expected to give it; and every one is aware of the advantage of being able to anticipate practical effects so nearly.

29. The most important experiments on the resistance of air appear to be those conducted by Dr. Hutton*, for the improvement of the theory and practice of Gunnery. In order to ascertain the effect of giving different inclinations to the same surface, he fixed a rectangular plane 32 inches in area, at different angles to the direction of its motion; the velocity was twelve feet per second.

Angles with the Direction.	Resistance by Experiment.	Calculated Resistance.	Difference.
90°	1000	1000	0
80	994	975	−19
70	957	902	−55
60	868	788	−80
50	724	646	−78
40	533	489	−44
30	331	333	+ 2
20	158	192	+34
10	52	78	+26
5	19	34	+15

The resistance of the plane when set at an angle of 90° and moving at the rate of twelve feet per second, was 0·841 ounce. By my mode of calculation it comes out 0·896 ounce. In another set of trials with a triangular plane, the resistance corresponding to the same area and velocity was ·846 ounce. The mode adopted by Dr. Hutton to reduce for the edge of the plane is not quite correct, as it obviously has an unequal effect at the different angles. (Art. 12.)

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* Tracts, vol. iii. p. 202.

30. In comparing the resistance of bodies of different forms, Dr. Hutton obtained the following ratios

	Cone. Angle 25° 42'.		Sphere.	Cylinder.	Hemisphere.	
	Vertex.	Base.			Convex side.	Base.
Ratio by Experiment	126	291	124	285	119	288
	·44	1·02	·435	1·00	·42	1·01
By Calculation	·388	·729	·434	1·00	·6	·833

It will be remarked, that in air as in water, the hemisphere with the convex side foremost differs most widely from the result of calculation. The sphere is very near, and the near coincidence of the actual resistance, as well as the ratio, is worthy of notice.

The resistance of a ball two inches in diameter was found by experiment to be ·163 ounce at a velocity of twenty-five feet per second. By calculation I find its resistance ·1655 ounce at that velocity,—a difference of only $\frac{1}{63}$ in excess.

31. Col. Beaufoy* made some experiments on the resistance of air on bodies of different shapes, and published a table of the resistances. When the area of the base is one superficial foot the force is in ounces; and taking the line corresponding to a velocity of twelve feet per second, I have added the numbers my mode of computation give in these cases.

	Cone. Angle 45°.		Thin Plane.	Cylinder.	Wedge. Angle 45°.	
	Vertex.	Base.			Vertex.	Base.
Resistance by Experiment	3·61	4·586	4·834	4·668	3·005	4·69
Calculated Resistance	2·294	3·36	4·032	4·032	2·294	3·36
Differences...	1·316	1·226	·802	·636	·711	1·33

In these comparisons the calculated numbers are in defect, and the ratios are not very different.

32. Returning to Dr. Hutton's experiments †, I must next

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* Annals of Philosophy for 1815, vol. vi. p. 277.

† Tracts, vol. iii. p. 230, where the tentative formula of Dr. Hutton are given.

show how far the formula will apply to great variation of velocity.

The equation for a spherical ball reduced to the form applicable to this case is

$$\frac{v^2}{4g}\left(1{\cdot}3 + \frac{1{\cdot}3 \times 0{\cdot}000000332 \times 4v}{d}\right) = \mathrm{H};$$

and since a cubic foot of air weighs 1·2 ounce at the mean temperature and pressure, and the area of the section of the ball is $d^2 \times {\cdot}7854$ feet, it becomes

$$\frac{v^2 d^2}{105}\left(1 + \frac{0{\cdot}000001328\, v}{d}\right) = \text{the resistance in ounces.}$$

Velocity in Feet per Second.	Resistance to a Ball two Inches Diameter in Ounces.		
	By Experiment.	By Theory, neglecting Friction.	By Theory, with Friction.
5	0·006	·0066	·00661
10	0·026	·02647	·026478
15	0·058	·0595	·05977
20	0·103	·106	·10664
25	0·163	·166	·16725
30	0·237	·238	·24016
40	0·427	·425	·43012
50	0·676	·665	·675
100	2·78	2·647	2·727
200	11·34	10·6	11·24
300	25·8	23·8	25·96
400	46·5	42·5	47·62
500	74·4	66·5	76·5
600	110·4	96·0	113·28
700	156·0	130·0	157·44
800	212·0	171·	211·96
900	280·3	215·	273·32
1000	362·1	265·00	345·
1100	456·9	321·	427·48
1200	564·4	383·	521·24
1300	683·3	450·	625·76
1400	811·5	520·	739·52
1500	947·1	595·	865·
1600	1086·9	680·	1007·68
1700	1228·4	769·	1162·
1800	1368·6	860·	1326·56
1900	1505·7	960·	1508·72
2000	1637·8	1060·00	1700·

In this series of comparisons there are only two places in which the difference amounts to $\frac{1}{10}$; and the one is in excess, the other in defect; and if the resistances were represented by the ordinates of two curves, these curves would cross five times at unequal distances in the range.

Explanation of the Plate.

Fig. 1. A body moving in the direction AB. The collision and line of reaction of a filament of the fluid from the oblique surface of its fore-part are indicated by faint lines, with dotted lines to represent the composition and reduction of the forces. The motion of the fluid after the body is also shown by lines of a light tint. The dotted rectangle is equal to the oblique parallelogram, but the motion of the fluid is less as the obliquity increases.

Fig. 2. Represents the plan of the body used by the French Academicians in their experiments; the head was altered to different angles, but the form of the after-part remained the same in all their experiments.

Fig. 3. is to show the effect of moving a plane surface in a fluid when placed obliquely in respect to the direction of its motion; and it is obvious that the edge of the plane must have a sensible effect on both the direct and minus-pressure.

Fig. 4. 5. and 6. are, a sphere; a cylinder, with the after-part a hemisphere; and a cylinder with the fore-part a hemisphere; having the characters used for calculation annexed to their respective lines.

From the flat end of fig. 5. the stroke and reaction being in the same direction, the fluid cannot move laterally till the central pressure so far exceed that on the more remote parts as to cause the proper quantity of lateral motion; or till a portion of the fluid accumulates before the body: but the portion accumulated being a fluid, the forces must produce the same effect as if each filament were struck and reflected by the solid plane.

Fig. 7. The plan of a body formed by segments of circles, with the characters annexed to the lines. When the radius is half the breadth, the form becomes a prism terminating in semicylinders at the ends (as fig. 8).

Fig. 9. and 10. are to show how the two motions of the fluid interfere when the fore- and after-part are separated by a thin edge only. In the hemisphere (fig. 9.), the resistance is increased by the fluid displaced by the fore-part preventing the fluid from following behind the body. In fig. 10. the reflected fluid from the fore-part in some degree facilitates the motion of the following fluid.

It will be evident to any one who examines the preceding paper, that it must have cost me a great deal of labour; and in consequence I was desirous of presenting it to the Royal Society. But finding that I must sacrifice all claim to new theoretical investigation in order to secure its appearance in the Transactions of that Society, I chose in preference to send it to Newcastle, and to take this most respectable channel for presenting it to the public, knowing that it will be extensively circulated among men of science, as well as that in these days it does not require the aid of authority to support the cause of truth; while, recollecting the state of hydrodynamical science as it appears in books written for the use of University students, we know that when authority has not truth to propagate, it does not hesitate to teach that which is known to be erroneous.

Having opened a new path in this difficult subject of the motion of fluids, it was not in my nature to stand still; the efflux of fluids, the impulse on bodies placed in a moving fluid, and various other inquiries followed. These, as my health permits, will be presented to the world.

THOMAS TREDGOLD.

XLII. *On* Mr. Herapath's *Second Attack on* Lagrange's *Method*.

LAGRANGE's method still charged with failure! In returning to this subject it was incumbent on Mr. Herapath, as a man of an ingenuous mind, to acknowledge the mistake he has already committed; of which he says not a word, although it was so clearly pointed out to him. Who, that felt the spirit of Science within him, could bear the thought of having made a charge of failure in consequence of a mistake of his own, without hastening to apologize to his living readers, and to the illustrious dead from whose fame he had detracted! The former argument, which was to overturn Lagrange's method, originated in the erroneous notion that every one of the three parts of the integral considered, must separately satisfy the differential equation; whereas it is the aggregate of all the three parts which satisfies that equation*. The matter is so very plain, that it is not easy to account for the mistake; unless indeed his last article help us to a solution of the difficulty.

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* At the bottom of p. 23, of this Journal for January, Mr. Herapath gives the value of p, which he has computed. Taking this value, it is obvious that his $p y_1$, is the same with the first part of the integral in p. 96 of this Journal for February. The other two parts of the same integral are of course identical with $p_1 y_2$ and $p_2 y_3$, all the parts being derived from one another by interchanging the roots. Mr. Herapath's expression of y, viz. $p y_1 + p_1 y_2 + p_2 y_3$, is therefore the very same with the integral in p. 96 of this Journal for February. This being established, however surprising it may appear, yet we must infer, from what he says of the integral just mentioned, and from the very confident postscript to his article, that he is not acquainted with what he has himself computed. The truth is, that care was taken not to change his expressions in the slightest degree. God knows what would have been the consequence, if, in explaining this gentleman's mistakes, any one had presumed to alter one tittle of his formulas!

He is now puzzled how to deduce the case of equal roots from the usual form of the complete fluent. There is some confusion in what he writes, arising from his not distinguishing between the mathematical definition of the complete integral, and a property belonging to it. An algebraic expression which satisfies the differential equation, and contains the requisite number of arbitrary constants, is the complete integral; and it possesses the property of comprehending every possible case, by varying the arbitrary quantities. It is certain that Mr. Herapath's integral of the third order, when we take in all its three parts, does satisfy the differential equation *; it also contains three arbitrary constants; it is therefore the complete integral: but notwithstanding, he contends that it is not general; for he makes it fail, particularly in the case of equal roots. Now this proceeds, not from a failure in any method or in any doctrine, but from a failure in Mr. Herapath, who does not reason correctly in adapting the general expression to the particular case.

Let us take his own example, p. 212, of the last Number of this Journal, viz.

$$y = \frac{ce^{rx} - c_1 e^{r_1 x} + e^{rx}\int \mathrm{X}e^{-rx}dx - e^{r_1 x}\int \mathrm{X}e^{-r_1 x}dx}{r - r_1}$$

When the two roots are real and unequal, or when they are imaginary, there is no difficulty; but the puzzle is, what must be done when the roots are equal, for then the integral is infinite. For the sake of simplicity, let $r_1 - r = \omega$, then $r_1 = r + \omega$; also put $c_1 = c + h\omega$: then, by substitution,

$$y = \frac{e^{rx+\omega x}(c + h\omega + \int \mathrm{X}e^{-rx-\omega x}dx) - e^{rx}(c + \int \mathrm{X}e^{-rx}dx)}{\omega}$$

This is only another form of the complete integral, the arbitrary constants being c and h; but it has the advantage of bringing out the correct value of y when the roots are equal, or $\omega = 0$. If we substitute the expansions of $e^{\omega x}$ and $e^{-\omega x}$, we shall obtain, supposing $\omega = 0$,

$$y = e^{rx}(h + cx + x\int e^{-rx}\mathrm{X}dx - \int e^{-rx}\mathrm{X}x\,dx);$$

but, $x\int e^{-rx}\mathrm{X}dx - \int e^{-rx}\mathrm{X}x\,dx = \int dx\int e^{-rx}\mathrm{X}dx$;

wherefore, $y = e^{rx}(h + cx + \int dx\int e^{-rx}\mathrm{X}dx)$,

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* See the calculation in this Journal for February last, p. 97.

which is the complete integral in its simplest form when the roots are equal. If we suppose $X = 0$, then,

$$y = e^{rx}(h + c x).$$

Thus, when we follow a proper method, it appears that the complete integral comprehends all the subordinate cases; which proves the incorrectness of Mr. Herapath's views.

Nothing more was intended by the notice inserted in this Journal for February last, p. 96, but to correct Mr. Herapath's mistake about Lagrange's method. It may now be proper to inform him that there is nothing new in his papers. Euler has integrated the equation of the third order in his *Calculus Integralis*, tom. ii. sect. ii. cap. 3. prob. 149; and at the beginning of the scholium to the problem, he observes: "In genere autem, nulla integralium reductione adhibita, integrale nostræ equationis ita exprimi potest;" and he then sets down the very same expression of y which is contained in Mr. Herapath's equation 19. Euler's words contain the exact description of Mr. Herapath's method, which finds the integral by successive integrations without attempting to reduce it to the form best adapted for use. In prob. 151. of the same chapter, Euler exhibits the unreduced integral of any order indefinitely, being the very same with Mr. Herapath's equation 17, and therefore containing the sum and substance of that gentleman's doctrine. Lastly, in prob. 152. cor. 4. it is observed that this form of the fluent is free from the difficulty about the equal roots, on account of the absence of zero divisors. These accumulated proofs show that there is nothing new in Mr. Herapath's papers, except the extraordinary mistakes they contain.

The history of this problem; the real difficulties attending it; the slips that were *at first* made by geometers of the highest eminence about the case of equal roots; the correction of these slips, and the artifices by which the solution has been completed;—all these are points about which it is fair to presume that Mr. Herapath is at present not well informed.

Some apology is due to the readers of this Journal, for the length of these remarks; but it seemed proper to explain the matter pretty fully; for, unless the writer of this article has been misinformed, Mr. Herapath's solution of this problem has, not much to the credit of British science, been blazed abroad as a great discovery. I now withdraw from any further intermeddling in this trite subject*.

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XLIII. *On*

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* It is probable that his Postscript relates to this frivolous point, viz. whether the arbitrary constant in p is to be written with the same denominator

XLIII. *On Systems and Methods in Natural History.* *By* J. E. Bicheno, *Esq.*, *F.R.S.*, *Sec. L.S.*, *&c.*

[Concluded from p. 219.]

IT was the opinion of Linnæus, and continues to be the opinion of some of his disciples, that genera are actually founded in nature as much as species. "Naturæ opus semper est species et genus." *Phil. Bot.* § 162. "Genus omne est naturale, in primordio tale creatum, hinc pro lubitu et secundum cujuscunque theoriam non proterve discindendum aut conglutinandum." *Ib.* § 159. So the excellent and elegant author of the "Introduction to Physiological and Systematic Botany," says, "A genus comprehends one or more species so essentially different in formation, nature, and often many adventitious qualities from other plants, as to constitute a distinct family or kind no less permanent, and founded in the immutable laws of the creation, than the different species of such a genus. Thus in the animal kingdom a horse, ass, and zebra, form three species of a very distinct genus, marked not only by its general habit or aspect, its uses and qualities, but also by essential characters in its teeth, hoofs, and internal constitution." It was the circumscribing these insulated assemblages of species that Linnæus regarded as the business of the accomplished naturalist.

Those therefore who use the word *genus* in the Linnæan sense, do not employ it with the same meaning as those who regard genera as merely conventional, and subject to be broken down to suit convenience. The latter would do well to employ some other term, else one great object will be lost at which we are aiming;—the keeping together under some one common head those small assemblages of species which in some instances are so obvious, and so important in enabling us to comprehend and discourse of the scheme of nature.

Whether such insulated groupings really exist, it is for the naturalist to determine, and this can be only inferred from a very extensive knowledge; but as long as we are witnesses to

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minator as the variable part, as he has himself written it; or without the denominator. Write it how he will, the same egregious blunder still remains; namely, his supposing that every part of the integral must separately satisfy the differential equation. His Postscript is not clear; but two things may be gathered from it: one, that he is possessed of a method for measuring the degrees of absurdity; the other, that he is not well assured what is, or what is not, Lagrange's method, although he has, twice in this Journal, accused it of failure. The truth is, that all his arguments are directed, not peculiarly against Lagrange's method, but against the complete integral, reduced to its simplest form, by whatever method it may have been obtained.

such striking modifications of form as we discover in the genus *Erica*, *Rosa*, *Eriocaulon*, &c., among plants, and in *Vespertilio*, *Strix*, *Scarabæus*, &c., among animals, it would be the height of folly to give up a term so expressive and at the same time so useful, or to transfer its received meaning to some other word which has not been used in the same sense.

As the success of the systematist depends so materially upon the proper use of these abstractions, I shall now proceed to show some distinctions which it is necessary to keep in view while we employ them. We aim, as I said before, at two distinct objects by the use of systems: we use the artificial for becoming acquainted with individuals, and the natural as the means of combining them, and enabling the student to comprehend and speak of the general truths relating to nature by a knowledge of a few particulars.

Division and separation is the end of the artificial system;—to establish agreements is the end of the natural. In one case we reason *à priori*; in the other *à posteriori*. The one is a descending, the other an ascending series. Linnæus understood this distinction when he remarked, "Ordines naturales valent de naturâ plantarum; artificiales in diagnosi plantarum."—"Cavendo in imitando naturam filum Ariadneum amittamus." Nevertheless it has appeared to me that many modern naturalists have not adopted these truths; and that it is the prevalent error of the day to attempt to generalize where they ought to analyse; while their arrangements, called natural, are almost all of them framed with a view to distinguish. Let me not be supposed by these remarks to wish to exclude from the natural system every attempt at diagnosis; for it is obvious, that as the business of the naturalist is to study all the characters, he can no more neglect differences than he can agreements. I only wish to point out the two dissimilar objects we have in view, that they may not be confounded.

M. Decandolle, for instance, whose labours as a systematist are invaluable, seems to overlook this distinction. In his *Regni Vegetabilis Systema Naturale*, he starts from things the least known, to reason on things best known. He begins his comprehensive work with a predicate of the stars; and, proceeding downwards to minerals, comes to plants. Here he employs a series of terms expressive of a natural gradation from the highest to the lowest group, attempting fresh combinations at every stage, and making a place for every thing. Thus he has *class*, *sub-class*, *cohort*, *order*, *tribe*, *genus*, *section*, *species*. The extraordinary number of these combinations diminishes their value as a work of natural arrangement. It is a difficulty of sufficient amount to establish a few well marked;

and when they are so multiplied, it may be suspected that many of them are arbitrary and artificial. This attempt at breaking down good orders and genera into many subordinate and loosely defined groups, and encumbering them with names, involves the subject in obscurity, and may well be questioned as contrary to his main design of presenting those comprehensive views which are afforded by a natural system.

Mr. Brown has adopted a different mode in his *Prodromus.* He has attempted to combine no further than his knowledge would warrant, not even employing the terms class or order as the names of his groups. As his object is chiefly synthesis, he keeps his diagnostic characters apart, thus leaving the mind less embarrassed when it is in pursuit of analysis. It must be admitted indeed, that his work cannot be employed with any success by the inexperienced, or even by those who have occupied themselves only in searching for species; but to have made it subservient to this purpose, would have been to have rendered it less beautiful and complete as a work of synthesis. His aphorisms and remarks not being reduced to exact method, "are," as Lord Bacon expresses it, "still in their growth, increasing in bulk and substance."

Now wherever the object of the systematist is to enable his reader to discover species, it is necessary to define at every step; and where natural characters do not present themselves, we must adopt artificial ones. For this purpose large classes are formed, many of which are necessarily artificial. These again are broken up into orders, mostly of an artificial character; and thus the naturalist is led step by step from more comprehensive definitions to less, from class to order, from order to genus, and from genus to species. In this descending series it will be observed that the essential feature is the facility that is afforded for definition. Hence the Linnæan system of botany has succeeded so well, because its author selected chiefly as the ground of his arrangement the number and proportion of parts most obvious and least liable to vary. His classes and orders are avowedly so many assumptions, which practice has shown to be convenient; but when we come to genera, the artificial system falls in with the natural, as Linnæus framed their characters upon resemblances founded in nature.

Now in the natural system this machinery of terms cannot be employed in the same manner. It is an ascending series from the less to the greater predicate. From genera we proceed upwards to orders, and orders we combine into classes. We become more and more general in our characters, instead of more and more definite. Here indeed we ought not to sacrifice, as in the artificial scheme, to convenience; and break up well-defined genera and orders because they contain a large number of species. If we find a large genus, for instance, as *Erica*, agreeing in some well-marked characters of structure, form, station, and properties, it appears contrary to the end proposed by the natural system, to divide and subdivide the species into small groups, and to give each of these the same value as is now possessed by the whole. This is frittering away characters which are essential to the use of a genus, and destroying our power over it when we proceed to generalise. The value of generic terms consists essentially in the distinct conceptions we have of them; but if we go on to multiply them, as is at present the fashion, we render it as impossible to circumscribe them, as it is to parcel out the colours of the rainbow; and instead of making Natural History familiar and popular, it will require the compass of a man's life to master the terms we employ. If indeed the object be to analyse, division may be very convenient, because the inquirer may be otherwise bewildered in the multitude of particulars. It does not follow from hence that the student of the natural system may not avail himself of subordinate groups by whatever characters they may furnish; only the giving them equivalent names, and making them co-ordinate, is destructive, as it appears to me, of his system as a means of general reasoning.

In no department of natural history are the inconveniences arising out of this confusion of analysis and synthesis more felt than in Entomology. The multitude of species included in this kingdom of nature is so great, that it requires the most skilful arrangement to enable the student to determine them: yet it is unquestionably the worst furnished with assistance in this way;—a defect which may be attributed chiefly, I apprehend, to the attempt which both we and our continental neighbours have made to combine the natural with the artificial system. We have aimed at analysis and synthesis at the same time. A comprehensive acquaintance with this infinitely varied tribe can alone enable us to synthesise with safety; and a long period must elapse before we can hope to embrace within our synthesis the whole of the insect world.

In the large views taken by means of the natural system, our business will for ever be the labour of separating what we shall know from that which is unknown. The profoundest knowledge will at last be but a fragment. Some groups of nature are so closely related, that they have been observed from time immemorial. "Whatsoever parteth the hoof and is cloven-footed, and cheweth the cud," comprehends a group of animals so obviously connected, that they must have received a generic appellation from the remotest period. As knowledge has increased, more and more families have been separated: still there is always a remainder of unknown things. Take any natural system, and see if this is not the case. Linnæus in his " Fragments of a Natural Method " professes only to separate from the mass those groups which he saw clearly. Again, his definition of vegetables indicates the same truth: "Vegetabilia comprehendunt Familias septem, *Fungos*, *Algas*, *Muscos*, *Filices*, *Gramina*, *Palmas*:" and then, to include the remainder, he adds, " et *Plantas*;" defining the last thus, " Plantæ dicuntur reliquæ, quæ priores intrare nequeunt familias." *Phil. Bot.* § 78. Take up Jussieu's *Genera Plantarum*: and besides his " Plantæ incertæ sedis," see how he is obliged to dispose at the end of many orders his " Genera affinia," and " Genera nondum satis determinata." This is true inductive philosophy; yet the same author may be suspected of departing from this mode of investigation when he attempts to edge in his remainder under artificial or sweeping characters, as he has done in *Eleagni* and *Junci*, and when, falling in with this modern innovation, he invents a multitude of new orders to embrace every known species of plant.

The mammiferous animals are arranged with more ease according to a natural system, in consequence of their number being comparatively small, and their forms strongly marked. Nevertheless the system of M. Cuvier, in the *Règne Animal*, clearly shows the vain attempt of finding a place for every thing. Nothing can be more satisfactory and beautiful than many of his orders and divisions; yet see how he is compelled to change his ground when he comes to the *Pachydermata*, and to huddle together species very remotely connected. His birds also exemplify the same fact, where his order *Passeres* is made to include all that his other orders will not hold. " Son caractère semble d'abord purement négatif, car il embrasse tous les oiseaux qui ne sont ni nageurs, ni échassiers, ni grimpeurs, ni rapaces, ni gallinacés." Thus it contains the Warblers, the Shrikes, the Goatsuckers, the Crows, the Creepers; birds of the most dissimilar habits, and living upon the most dissimilar food. The Chough is separated widely from the *Corvi*, and *Anthus* from *Alauda*. Now this is what we might expect from the nature of the subject; only it is desirable that the remainder of unknown things should be distinctly avowed, and not reduced to an exact place in the natural system. Jussieu's was the most philosophic mode, which was to place this residue at the end. Linnæus too was very correct when he pronounced his natural orders to be a " Fragment;" and those persons who imagine it to be necessary or advantageous to find a place for every thing, and to divide and split for the purpose of making such places, appear to lose sight of the chief object of the natural system, and to destroy its utility as an instrument of general reasoning.

The French writers in general are prone to combine in their systems the very distinct objects of individualizing and generalizing. They are for ever subdividing where the great aim should be to combine, and thus they detract from the utility of their arrangements for either purpose. It is they who have countenanced the use of *sub-classes*, *cohorts*, *tribes*, *stirpes*, *sub-genera*, and *sub-species;* and they also are the great contributors to the minute division of genera. Strictly speaking, in the natural system we should employ but few terms of the kind alluded to, and those of loose application. For instance, the word *sort* or *group* would as correctly express any natural assemblage of species, as *sub-class*, *race*, *tribe*, *cohort*, or *stirps ;* for what do we know of the relative value of the groups attempted to be pointed out by these expressions? And how can we say they are not co-ordinate or commensurate with each other? The great division of cotyledonous plants may, for aught we know, be only equivalent to the order of Grasses; and a genus in some cases seems as distinct as any class, as *Parnassia* and *Linnæa* among plants, and the *Ornithorhynchus* and *Hippopotamus* among animals. Indeed in the recent work of M. Latreille, *Familles Naturelles du Règne Animal*, he has arranged the monotrematous animals in a class by themselves, and has made two orders; in one case, consisting of a single species, the *Ornithorhynchus paradoxus*, and in the other, of two other species before considered as belonging to that genus. Thus it is, as M. Cuvier remarks, that these animals set at naught all our classification by their osteology and mode of bringing forth.

The adoption of these numerous terms, intended to express fixed ideas, must be looked on with suspicion. The terms *species* and *genus* are too well established by custom, and are so clearly the result of convenience, and moreover conform so closely to the ordinary use of these words, that their utility cannot be questioned; but those numerous subdivisions current among our neighbours, and sensibly increasing among ourselves, may well be doubted as unphilosophical language. To each of them is attempted to be assigned a definite value beforehand, and an impracticable degree of precision; and we deceive ourselves by fancying that we can deal with these delicate and fleeting instruments of thought differently from the rest of the world. But are we to attempt to fetter nature by our systems and terms? "Books should follow sciences, not sciences books," says the immortal Bacon; yet the adoption

tion of systems and technical expressions, which have received their definition beforehand, cannot be employed without the danger of perpetuating false hypotheses, and an apprehension on the part of the ignorant, that these inventions give us some power over nature not belonging to ordinary language.

The more correct mode would be to exclude from the natural method most of these terms, and to employ in their place some convertible words of looser import, as indeed M. Cuvier has to some extent done; such for instance, as group, section, division, to express those larger assemblages of approximations to assigned forms, which are rather predicated than proved; and in many cases to point them out by mere signs, such as are used in printing. Thus, for instance, the word section, or any similar word, might be employed to express the plants severally comprehended in the order *Gramineæ*, the class *Compositæ*, and the division *Monocotyledones;* and where the characters are less definite, the plants pointed at might be assembled under a simple asterisk.

One chief recommendation of the natural system over the artificial, is the liberty which it leaves to the mind. The one shuts it in to the narrowest scope of observation, while the other suffers it to range in search of all the properties belonging to created beings; their functions, their structure, relations and resemblances, affinities and analogies. It is speculative and general truth that the natural system enables us to pursue; and this will never submit to be bound by any fetters which the art of man can invent. Books after all are but a rude mode of holding knowledge together; and language but an imperfect vehicle to convey with precision the just relations of things. At best it bears the image of the earthy, while things themselves bear the image of the heavenly.

XLIV. *Examination of a gelatinous Substance found in a damp Meadow;—as a Contribution to the Knowledge of the Meteors called Shooting-Stars.* By *Dr.* R. BRANDES*.

MY friend Dr. Buchner communicated some time ago (in Kastner's *Archiv.* v. 182), a treatise on the substance of the meteors called Shooting-Stars, which Kastner has designated by the name of star-jelly. This substance, found in a damp meadow, was of a gelatinous appearance, and was supposed by Dr. Schultes to be *Tremella nostoc.* M. Buchner, however, having examined it, was of a different opinion, not having been able to discover in it any trace of an organic tissue.

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* From Schweigger's *Jahrbuch der Chemie,* N. R. Band xix. p. 389.

Indeed he does not decide whether its origin was terrestrial or not; yet he asserts that this substance could be neither a plant nor an animal, (at least not an entire one,) but perhaps the produce of an animal, an *excretion* resembling gum, mucus, &c.; and although he does not deny the possibility of such a body having descended from the atmosphere, he does not think it very probable. Nay, he does not much object even to the comparison of this slimy substance with the manna of the Israelites, which fell from heaven;—he thinks at least that this slimy mass might be as nourishing as oysters. However inclined I may feel to agree with my respected friend on the first two points; viz. that this mass might originate from the excretion of an animal, or a gelatinous meteor, the idea of comparing it with the manna of the Israelites seems to me untenable, there being too great a difference between both the nature of the substances and the places in which they have been found. I was much inclined to ascribe to the substance in question an atmospheric origin, in consequence of the want of organic structure, which M. Buchner ascribes to the mass he examined; in consequence also of the account given by R. Graves of a fiery meteor which had fallen in Massachusetts, in the United States, in a spot where the following morning a gelatinous substance was found*; in consequence, moreover, of my own observations on the existence of atoms of azotized substances in the atmosphere, at least in rain-water, (*Jahrb.* 1826, iii. 253); and ultimately from the account frequently repeated to me by a soldier of the contingent of Lippe, who had fought in the Peninsula, of his having frequently noticed in Spain, while on duty during cold nights, stars to shoot, and perceived in the morning in damp places, where he thought them to have fallen, white gelatinous masses, which were soon decomposed; an opinion which I also mentioned in my treatise on Rain-water.

M. Schwabe, an apothecary at Dessau, published some time after a treatise on the same subject (Kastner's *Archiv.* vii. p. 428), on his having had an opportunity of examining a substance which had been found in a damp meadow, which was likewise gelatinous and of a green colour. Mr. S. recognised this mass distinctly as *Nostoc commune* Vauch. (*Tremella nostoc*, Linn.), having discovered in it through the microscope the structure of this singular *nostoc*. As this mass agreed not only in its external form and place of discovery with that of Buchner, but also in its chemical composition, Mr. S. thought himself justified in assuming that both this substance and that of Buchner were of the same nature; since, owing to its peculiar texture, Mr. B. might have overlooked its organic structure. But on comparing more closely the descriptions given by the two naturalists of the substances observed by them, we discover several other differences, besides the one that Mr. Schwabe discovered,—a distinct organic structure; while Mr. Buchner saw none. The substance examined by Mr. S. was of a greenish colour; that of Mr. B. white, like swelled tragacanth. The former on being burned emitted, not an *animal* smell, but one quite peculiar, resembling burning *confervæ*, *rivulariæ*, and *chætophoræ*, and yielded a shining coal, which preserved the skinny form of the pieces employed; and being reduced to ashes, left a residuum of silica, with carbonate, muriate, and sulphate of potash, with traces of phosphate of lime and oxide of iron; but Buchner's substance, on being heated, swelled considerably, diffusing at the same time a strong *animal* smoke; caught fire at last, and left a coal that could not be reduced to ashes, and which contained carbonate of soda and phosphate of lime. However similar Mr. S. may consider his substance to that of Mr. B., the facts here stated seem to invalidate his conclusion as to their identity. I feel, therefore, great pleasure in being able to communicate an examination which I had an opportunity of making last autumn, and which may perhaps throw some light on the point in question.

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* Gilbert's *Annalen*, lxxi. 314.

A friend and fellow-townsman of mine, who has a large meadow near our city, which, lying in the lower part of our salt-valley, has been drained in a slight degree by dint of great labour, and the grass in it improved by the application of salt and ashes, for manure, found in this very meadow a gelatinous substance, and was told by a labourer that he had frequently seen similar ones there; a circumstance which had escaped my friend and me, though both often passing through it.

My friend having brought me this substance for examination, my mind immediately reverted to that examined by Mr. Buchner; and perceiving even outwardly several differences between them, I undertook a close examination of it, as follows.

The substance was of a very clear white colour, representing a strongly swelled mass, which Mr. B. very properly compares to swelled tragacanth; its bulk might be about 2½ cubic inches. On a closer view, I perceived that in several places it was covered with a very fine white skin, which seemed to have burst in the centre only; in such parts the inside had protruded in the shape of a very bulky gelatinous mass. The

bursting

bursting of the skin had undoubtedly been produced by the swelling of the mass on account of the moisture it had absorbed from the meadow, a tension which the thin membranous covering could not resist. In such parts too the membrane was so concealed by the jelly that it could scarcely be perceived. Nor had the jelly here any distinct form, or any traces of organization. But wherever the mass had remained entire, and although swelled was still inclosed, it showed a vermicular appearance of the thickness of a quill and thicker; whilst in those places where the membrane had burst, the inside projected in lumps of three-quarters of an inch in thickness. This vermicular appearance presented several small slightly indented divisions, and had entirely the figure of an intestine; the back was marked by a tender vessel of a darkish-brown colour, which spread with fine veins towards the front, losing themselves about the middle in small blackish points. By this vessel the back of the mass was entirely contracted, and much extended towards the surface, just like an intestine.

In a dry place the substance gradually shrunk, soon lost its white colour, turned to a brownish yellow hue, became very tough, so that it could be drawn into threads like glue, and dried at last into a horny mass.

Burned in a crucible of platinum it swelled, became gradually black, emitted a strong animal smell, like singed wool, and left 1·2 of grayish white ashes, which were scarcely affected by water for some time, but at length gave a slightly alkaline solution. In nitric acid the ashes were completely dissolved, and ammonia gave a precipitate of phosphate of lime.

Twenty grains of this substance were desiccated on a water-bath. It became hard and tough; and its weight was reduced to four grains. Moistened with water, it in a short time re-assumed its former size and white colour.

One hundred grains of the substance were boiled in three ounces of water; by this it swelled into a tremulous jelly, which was so bulky that it had thickened almost all the water; for the whole being deposited on a loose clean linen cloth, it congealed upon it; and after several hours but little of the liquid had oozed out, which was rendered turbid by protonitrate of mercury and acetate of lead, but not by superacetate of lead. A little of the substance was shaken with alcohol; this acted but little, but separated from it some of the water, and on its bulk being diminished, it likewise lost its transparency.

A solution of ammonia acted but slightly, whether in the cold or by heat; but a solution of caustic potash affected it perceptibly even while cold, and entirely absorbed it when heated: neutralized by any acid, the substance was again precipitated. The sulphuric, nitric, and muriatic acids also acted upon it while cold, and entirely dissolved it by heat. The nitric acid turned somewhat yellow, the sulphuric acid brown; the muriatic acid remained colourless.

From these experiments it is evident that this substance did not resemble *albumen*, but essentially agreed with *jelly*, and resembled the slime of springs. It consisted of

Gelatinous substance	18·8
Animal ditto	traces
Phosphate of lime and phosphate of soda, with an organic acidity	1·2
Water	80·0
	100·0

What now is the origin of this substance?

The evident existence of an organic structure will not allow any opinion of its being of an atmospheric formation, but shows on the contrary that its origin must be terrestrial and of an animal kind. Its striking resemblance to an intestine, led me at first to suppose that it might have been the intestine of a bird; but its containing a smooth jelly, and being inclosed in so fine a membrane, compared with the tougher skin of any intestine, which could not have swelled to the degree mentioned, the want of the usual contents of an intestine, &c. left ultimately no room for such a supposition. But the chemical resemblance of this substance to the spawn of frogs, led me to the idea of its being perhaps the spawn of some animal. It could not be that of a frog, but it might be the swelled spawn of a snail, such as are frequently found in damp meadows; as the *Limax rufus, agrestis, stagnalis, &c.* I compared the descriptions given in this respect in Cuvier's Comparative Anatomy; in Oken's Natural History, iii. chap. i. p. 309; in the same author's Natural History for Schools, p. 668; in Goldfuss' *Hanbuch der Zoologie,* i. p. 661, and other works;—in all of which, however, I found little information respecting the object of my research; viz. the spawn of snails. Oken, in his Natural History for Schools, however, remarks, when speaking of *Limax stagnalis,* that "its spawn was a small gelatinous cylinder, of one inch in length and one line in thickness, containing a dozen small yellow eggs; that these rows of eggs generally adhere to some aquatic plants, and at the end of a fortnight or three weeks the young snail crept out of it." Oken further mentions in his Natural History, concerning the *limaces:* "The excremental canal of the sexual bladder is short, ends in the vagina close to the ovarium, much longer as it should seem, in proportion to the penis, and the ovarium connects with it before it enters the vagina. There is, therefore, no doubt that this bladder forms a portion of the female parts, and furnishes perhaps the gelatinous substance for the eggs, especially as this bladder is found in all snails. Their contents, however is solid, soft like pomatum, and red-brown, which has led to the erroneous supposition of its being purple." (Compare also Cuvier on the same subject.) Although the egg-sticks of the *limax*, as mentioned, are very small, it is yet possible to suppose our substance to have been the spawn of a *Limax rufus*, or some other species, since its great bulk chiefly arose from water,—a reason too why its solid contents, compared with its volume, were so small, and those of the water so great; and I was enabled, as we have seen in the experiment with boiling water, to swell it to its actual extent. This supposition was further confirmed, on my finding in a portion of the substance which I had placed in a small cup before the window of my study for a few days, a little naked snail (*limax*) of about a quarter of an inch long. I think, therefore, I may positively assert that the white gelatinous substances which are occasionally found in damp meadows, and frequently pronounced to be the substance of shooting-stars, are not of atmospheric origin, but consist of the spawn of the above-mentioned snails, which, although small in its natural state, and therefore remaining unobserved, assumes, in damp places, by absorbing water, the large bulk and white gelatinous appearance, necessarily attracting the attention of persons who find them in their way; and finally, that its being found only in damp places is owing to the very nature of this spawn.

I doubt whether the real substance of a shooting-star has ever been found. Whoever has observed these meteors, must be convinced that one could not so readily notice the spot where they seem to fall in the darkness of night, as to be able to find out the supposed substance, and as it were be able to say that he has a fallen star in his hand. Before this can be clearly shown, it may yet have to be proved whether any opinion can be formed as to the nature of shooting-stars. Even the observation of the above-mentioned American meteor seems subject to doubt; and it is still a question whether the produce of a fiery meteor could be a gelatinous mass?

Our knowledge of meteors is indeed much increased by the inquiries of Professor Brandes of Breslaw; but still the nature of the substance seems to be enveloped in doubt.

It now only remains for me to consider the apparent differences which seem to exist between the observations of Messrs. Buchner and Schwabe, and my own; which may be satisfactorily done in consequence of the precision with which those gentlemen have written their accounts.

The substances they have respectively examined offer, as I have stated above, qualities so different in their nature, as to ascribe to each a different origin; so that I cannot agree with Mr. S. in supposing the substance examined by him to be of the same kind as Mr. B.'s, while I readily admit that the substance he has investigated was a real *tremella.* Mr. B.'s substance, on the contrary, fully agrees with that examined by me. Their chemical contents are quite the same; and the only essential difference between them seems to be, that the substance examined by him no longer exhibited any organic structure; whilst that observed by me still bore the evident traces of an animal production. But if we consider that this snail-spawn had lost all appearance of organic structure wherever the membrane had burst, and especially when it was much swelled with water, I suppose that Mr. B. found this substance so swelled, that it had burst the membrane in every part, so as to cover and obliterate the organic structure entirely. That the mass examined by Mr. B. was actually very much swelled, appears from the fact, that he found only 40·4 of solid substance; whilst the one examined by me still had 20·0. If then the identity of the two cannot be doubted, we must at once ascribe the same origin to both,—that of being the spawn of a snail.—Thus I think I have explained the nature of the substance called star-jelly, and have had the satisfaction of reconciling the differences between the observations of Messrs. B. and S.; having shown that the observations of both these meritorious naturalists were correct, but that the substances were entirely different in their nature.

XLV. *Description of a Percussion Rifle, igniting by a Spring instead of a Lock. By Lieut.-Col.* MILLER, *F.R.S.*[*]

THE stock of this rifle is made either of iron or bronze[†], hollow in the centre, and the barrel is made to screw into it at the breech. The spring acts horizontally, and is screwed to a plate, fixed to the left side of the small of the stock. A cross piece is attached to the fore part of the spring, which passes through the stock behind the breech, and projects a little on the left side. In the cross piece there is a notch, and a button at the end of it;—the trigger moves upon a pivot in the upper part of the stock, and is pressed forward by a spring behind it. The piece is cocked by grasping the small of the stock with the fingers of the right hand, and pressing the thumb against the button of the spring until the notch comes opposite the trigger. The cap is then put on the nipple, and the spring let down upon it, by again placing the thumb against the button, and pulling the trigger with the middle finger. In that position the spring is allowed to remain, until the piece is about to be used, when it is again cocked and fired off. Fig. 1. represents a side view of the gun; and fig. 2. the action of the spring. A piece of leather is put round the small of the stock, to make it more pleasant to the touch; and a box placed behind it, for holding caps and patches. The power of the spring may be increased or diminished at pleasure, by turning the screw.

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[*] Communicated by the Author.

[†] As manufactured by Nock.

This contrivance, it is conceived, produces fire more instantaneously than a common lock, from its having no friction. It is also less liable to get out of order; and a gun can be constructed on this principle, at about half the expense of that now generally in use.

A rifle of this construction was tried at Woolwich in August last, of which the following is the result.

Woolwich, August 1, 1827.

Report of the experiment carried on this day with a percussion rifle, igniting by a spring instead of a lock, proposed by Col. Miller. The rifle was fired by Col. Miller, and at the ranges of 400 and 100 yards, proposed by himself. The powder used was Curtis and Harvey's, brought by Col. Miller. The target was one of 9 feet, of two boards each an inch thick.

Target at 400 yards, balls 28 to the pound.

Round 1.	charge 1 drachm;	grazed 100 yards short.
2.		100 yards short.
3.		20 yards short.
4.		20 yards short.
5.		30 yards short.
6.	1½ drachm;	30 yards short.
7.	———	over all.
8.	———	struck target 4 feet 7 inches right of bull's eye, 1 foot 9 inches under; ball penetrated 1 inch, and lodged in target.
9.	—	grazed 20 yards short; struck target, and dropped; ball made an impression half its own diameter in depth.
10.	—	grazed 28 yards short.

Fig. 1. View of Gun, one-sixth the original size.

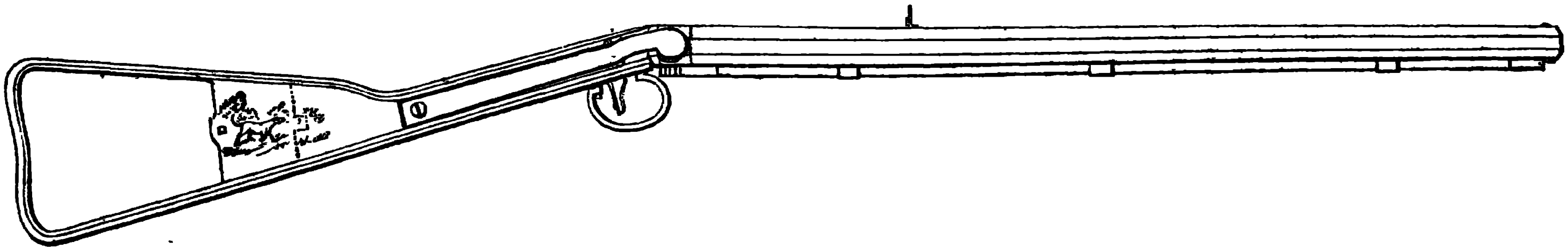
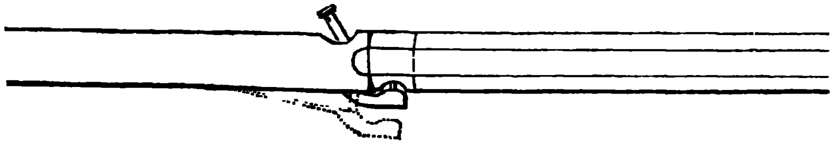


Fig. 2. Action of the Spring, one-fourth the original size.



Round 11.	charge 1¼ drachm;	over.
12.		grazed about 60 yards short.
13.		– grazed twice, and struck target.
14.		– over.
15.		– grazed about 20 yards short, struck target, and dropped; ball made an impression half its own diameter in depth.
16.		– grazed about 80 yards short; struck target, and dropped; ball made an impression one-fourth its own diameter in depth.
17.		– over.
18.		– grazed 20 yards short.
19.		– grazed to the right of target.
20.		– grazed 3 yards short.

Target at 100 yards distance.

Round 1.	charge 1 drachm,	through target (hung fire a little) 21 inches above, 7 left of bull's eye.
2.	————	through, ½ inch under, 1½ left of bull's eye.
3.	————	through, 9 inches under, 3 right of bull's eye.
4.	————	through, 1¾ inch under, 1½ left of bull's eye.
5.	————	through, 9 inches under, 5 left of bull's eye.
6.	————	through, 6 inches under, 8 left of bull's eye.

Penetrations of Col. Miller's rifle compared with the Government rifle, through elm boards, each ½ an inch thick, steeped in water, and placed in a frame ¾ of an inch apart from each other, at the distance of 60 feet.

Col. Miller with his own rifle, and Government powder. Weight of rifle, 7 lbs. 10 oz.; length of barrel 23 inches.

Round 1.	charge 1 drachm, flashed once, barrel just washed; through	7	boards, struck 8 smartly.
2.	————	6	—— split the 7th.
3.		6	
4.		6	— struck 7th.
5.		6	— struck 7th slightly.

Col. Miller, with his own powder.

Round 1. charge 1 drachm (Not taken, struck near a former shot).
2. ——————— through 7 boards, struck 8th slightly.
3. ——————— (The same as the first).
4. ——————— through 7 boards.
5. ——————————— 6 boards, struck 7th slightly.

Government rifle with Government rifle powder.

Weight of rifle 9¼ lbs.; length of barrel 30 inches; balls 20 to the pound.

Round 1. charge 3½ drachms (4 drachms including priming) through 8 boards, just touched the 9th.
2. ——————— 8 boards, split the 9th.
3. ——————— 8 boards, splintered the 9th.
4. ——————— 8 boards, splintered the 9th.
5. ——————— 8 boards, splintered the 9th.
6. ——————— 8 boards, split the 9th.

Col. Miller's rifle, 28 balls to the pound.

Round 1. charge 1¼ drachm, through 7 boards, struck the 8th.
2. ——— 1½ ——— through 6 boards, struck the 7th.

Government rifle, 20 balls to the pound.

Round 1. charge 1 drachm, through 5 boards, struck the 6th.
2. ——— 1¼ ——— ——— 5 boards, struck the 6th.
3. ——— 1½ ——— ——— 6 boards, struck the 7th.

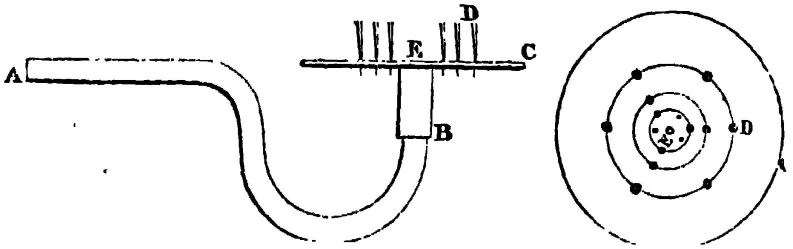
Here ends the Report.—From the preceding experiments, the great superiority of percussion over the flint-lock is very apparent, as the penetration of the percussion rifle is not very much inferior to that of the flint-lock, with a much heavier ball, and four times the quantity of powder. The penetration of the percussion rifle also does not appear to be increased by increasing the charge beyond a drachm; which tends to prove, if any proof were wanting, that moderate charges are the best, and that heavy ones only tend to destroy arms, and an unnecessary expenditure of ammunition, without producing any corresponding effect.

XLVI. *Account of an improved Form of Apparatus for exhibiting* M. Clement Désormes's *Experiments on Currents of Air, &c. By* Robert Younge, *Esq.*[*]

THIS improvement consists in the application of moveable pins on the plate, which enable the operator to vary the size of his discs; and it is presumed involves the explanation of these phænomena. The form of the glass tube permits the effect to be very distinctly seen, particularly when the current is powerful, the edge of the plate being on a level with the sight.

AB is a bent glass tube about six inches long; C is a circular copper plate, having a tube of the same metal attached underneath, which receives the end of the glass tube B.

D are upright moveable pins[†], passing through holes in the plate, which are represented in the circular drawing. At E the plate is perforated, to form a communication between the upper side of the plate and the glass tube[‡].



By commencing with a large disc, and gradually reducing its diameter, we may perceive corresponding alterations in the force of the resistance which it offers; and by striking circles of different diameters round the central opening, and leaving three or more smaller holes in each circle for inserting the pins, we may easily apply discs of any diameter; and *when these bear a certain proportion to the diameter of the central opening, they are invariably blown off.*

In No. 2. of the Journal of Science, p. 473, it is observed, "If the issue for the vapour be turned towards the earth, and the disc consequently tend to fall, as well by its own weight, as by the pressure of the vapour, still it will not descend." Might not the amount of this counter-attraction, or overcoming of gravity, be pretty accurately estimated by the application of a series of discs, of different weights, varying according to the diameter of the opening, and weighted in proportion to the power of the current?

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[*] Communicated by the Author.

[†] The sole use of these pins is to keep the discs over the central opening.

[‡] This apparatus was exhibited at the last meeting of the Sheffield Literary and Philosophical Society.

XLVII. *Of*

XLVII. *Of the Insect called* Oistros *by the Ancients, and of the true Species intended by them under this Appellation: in reply to the Observations of* W. S. MacLeay, *Esq., and the French Naturalists. To which is added, A Description of a new Species of* Cuterebra. *By* Bracy Clark, *F.L.S., and Foreign Member of the Royal Academy of Sciences of Paris.*[*]

IN the 14th volume of the Transactions of the Linnean Society, is a communication written by my friend W. S. MacLeay, Esq., intended to prove that the fly, intitled *Oistros* by the ancients, was not the insect so named by Linnæus, but that it probably belonged to the present Linnean genus *Tabanus.*

Being of a contrary opinion, I am led once more to address this learned Society, to lay before them the grounds on which it is founded, that naturalists may not incautiously and too hastily adopt the above conclusion, and that they may avoid the confusion which change of names and counter changes always produce in science. I am also led to this undertaking in order to vindicate Linnæus himself, our great master, and such distinguished naturalists as Vallisneri and Reaumur, with whose views on this subject I wholly concur. Nor is the justification of myself wanting as a motive to induce me to re-examine the subject, having formerly sent to this Society a dissertation of some extent on the genus *Œstrus*, unfolding some curious discoveries in the characters and natural habits of this singular race of insects [†].

Disputations about the meaning of the ancients, and identifying their descriptions with the modern species of natural history, would perhaps, in a general way, be better avoided in the valuable volumes of this Society, as leading to much desultory and unsatisfactory discussion: practical subjects and didactic facts would perhaps better maintain their reputation. As, however, the Society have in this instance already admitted the discussion, it is but fair and just to allow the reply in the same channel, that the impression, if erroneous, may be removed.

W.S.MacLeay, in the paper alluded to, insists that the οἶστρος of the ancients, and the *Brize* or *Breeze* of the old English poets, is not the *Œstrus* of the moderns; and he infers this from the anatomical characters which some of the ancient authors have left us of their insect. Now, besides the anatomical descriptions to be found in the works of philosophers, there is another mode of identifying the insect; and that is, by the description of the effects it produces upon cattle, and which are so singular, that they have afforded incidents to most rural poets, ancient and modern: and the truth seems to be, that the poets in describing these effects have been true to nature; while the philosophers, being presented with a wrong insect, have only involved the subject in error.

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[*] From the Transactions of the Linnean Society, vol. xv. Part ii. p. 402.

[†] Published in the 3rd volume of the Society's Transactions.

That it is an Italian insect we have the authority of Vallisneri of Padua, who appears to have been the first naturalist who bred the true *Œstrus Bovis* from the grubs found in the backs of the cattle; and for the first time, as far as we possess any record of the subject, saw with certainty the identical object that created, so much commotion among them. He applied correctly enough the passages of the ancients which he thought had allusion to this insect. Reaumur followed Vallisneri in these researches, and bred with great difficulty one imperfect specimen of the true *Œstrus Bovis.* Linnæus next followed; but not having ever seen the insect, and not daring to describe from figures merely, or the descriptions of others, he took the large Horse Bot for it,—the *Œstrus Equi* of my enumeration. This error is continued through all the editions of the *Systema Naturæ*, intending all the while, and referring to Vallisneri and Reaumur for, the true *Œstrus Bovis.* Thus, like some of the ancients, he also described a spotted-winged insect for the *Œstrus Bovis;* whereas the true insect has perfectly spotless wings. The true fly cannot be caught in the act of oviposition, from the violent running of the cattle, and the terror they are in at the approach of their enemy.

This makes it more than probable, nay, almost certain, that if Aristotle, Ælian or Pliny described an insect with spotted wings, or with a trunk or proboscis, &c., they knew nothing about the true *Œ. Bovis*, and had been deceived as to the real object of their research. It was indeed much more easy for them to have been presented with one of the numerous host of flies that infest the backs of cattle and lodge on them, than the true *Œ. Bovis.* Their fly may have been a *Tabanus* or an *Asilus*, a *Conops*, or a *Culex*, or any other with spotted wings; for as the true fly cannot be caught in the act of oviposition, it was next to impossible they should have discovered, or been made acquainted with, the true object of such disturbance. Indeed, during these commotions it would be dangerous to approach the cattle, or to remove any thing from their back; and if an insect was caught under any other circumstance, how could it be known that it was the genuine cause of this agitation?

It is in vain now to inquire what precise fly these ancient philosophers might have been presented with, as their testimonies

monies are various, and militate against each other; but none are descriptive of the true fly, which we now fully know. Surely such a conclusion is more natural and just, than to suppose these conflicting descriptions true, and that the poets and common observers were false witnesses.

I now proceed to give what Virgil says respecting the name of it among the ancients, and the tumult it occasions; and of which no sweat-sucking *Tabanus*, *Conops*, or modern *Asilus*, can in any way be the cause.

> "Est lucos Silari circa, ilicibusque virentem
> Plurimus Alburnum volitans, cui nomen Asilo
> Romanum est, Œstron Graii vertêre vocantes:
> Asper, acerba sonans: quo tota exterrita sylvis
> Diffugiunt armenta, furit mugitibus Æther
> Concussus, sylvæque, et sicci ripa Tanagri."
> *Georg.* lib. iii. v. 146.

From this admirable description, it is clearly manifest that *Asilus* was the Roman name for the fly which agitates the cattle; and it is equally clear that *Œstros* was the Greek name for it.

Not much weight is due to the observation, that Homer's insect was not the modern *Œstrus*, because he mentions the spring as the season of its appearance, since he also adds, in the same line, ὁτε τ' ἤματα μακρὰ πέλονται, "when the days are long;" nor that Shakespeare did not use the word *Brize* for the same insect, merely because he has assigned its appearance to the month of June, when it more often appears now in July. Indeed the alteration of style will account for this difference. But the same poet uses the word in another place, where the allusion is too distinct to be mistaken:

> "The herd hath more annoyance by the Brize,
> Than by the Tiger." *Troilus and Cressida.*

And again in an old Play, quoted by Archdeacon Nares in his Glossary, the following use of the word occurs,

> "I will put the Brize in 's tail, shall set him gadding presently."

Now if MacLeay or Latreille, who entertains a similar opinion, had ever been as much among cattle on the heaths, as my pursuits have led me, they would have long since obtained a practical acquaintance with the effects produced by these insects, and would not have been led to suppose that the *Tabani*, *Conopses*, or *Culices*, were the object of poetic description, or have made any mistake between the effects of one and the other. When the *Tabani* and *Conopses* have come and settled in great numbers on the back and sides of the animal, he would, as I have often witnessed, scarcely regard them. A toss of the head, perhaps, towards the part, if they sucked a little too vigorously; or, if they were still more importunate, a lash of the tail, was in general all the notice he would condescend to take of them. But if an *Œstrus* approached, the consternation was indescribable, and the agitation most remarkable; and the object attacked, however lazily he might be disposed from the heat of the weather, or a full belly, would become suddenly as agile as a young deer, and canter away, holding out his tail, and running with a sort of undulatory movement of the back (thereby endeavouring, perhaps, to disappoint the touch and designs of his enemy), till he had obtained his accustomed retreat in the water, or the fly had quitted him,—

> ....Tossing the foam
> They scorn the keeper's voice, and scour the plain,
> Through all the bright serenity of noon;
> While from their labouring breasts a hollow moan
> Proceeding, runs, low-bellowing, round the hills.—THOMSON.

Assuredly no *Tabanus* can produce any effects like these. Unable to account for this extraordinary agitation, I had formerly given way to the notion of some very painful infliction by the *Œstrus*: but I am now led to question this opinion, inasmuch as I can discover no instrument by which this effect can be produced. The shrill sharp sound, which Virgil describes, was, I dare say, not stated without some real ground; and a friend of mine actually informed me, that he was standing in a farm-yard one day near some cattle, when one of these flies entered and approached them, and that he distinctly heard this shrill sound. In confirmation of this account we may remark, that the wing-scale, covering the *halteres*, which has been supposed by Keller to be the organ of sound, is particularly large in this insect; but further than this we dare not assert, but leave the point for future investigation. We know from Linnæus's own account, that the *Œstrus Tarandi*, or Rein-deer Bot, very similar in all respects to the *Œ. Bovis*, makes no sound while depositing its egg; which again brings me into doubt upon this matter.

We next have to observe, in confirmation of the peculiar effects of these insects upon the animals they infest, that those of the *Œstrus* of the Rein-deer, are equally singular and remarkable; and this fact we have from the indefatigable researches of our immortal leader, Linnæus himself. He says, speaking of the *Œ. Tarandi*, in his Lapland Tour, that as he was in bed early one morning, he perceived a very ungrateful smell, and when day-light appeared, "there were standing about the cot a thousand of these Rein-deer, driven by old men, boys, dogs, and women, who milked these animals. They appeared to be under the apprehension of some invisible attack: the animals carried their heads aloft, their ears pricked up and extended, beating the ground, and kicking in the air with their feet, as though by enchantment. Then for a while they would be quiet; then, again, they were seen most furious, and this with so general and regular a movement, that no army would have surpassed their exercises in uniformity."

Linnæus further states, in the *Lachesis Lapponica*, respecting the effects produced by this sort of *Œstrus*, that in passing afterwards into the Lapland alps he observed a Rein-deer, which was loaded with his own package, frequently to stop short and become perfectly quiet and motionless as a pillar of stone, or one suddenly struck with catalepsy; the head held straight out, the ears upright, the eyes fixed; nor could he by any ill treatment be induced to proceed; but in a little while he would again resume his march. Where, I would ask, is the *Tabanus*, or *Conops*, that could produce effects like these? or what naturalist, at all acquainted with the operations of Nature herself, could confound the dissimilar effects produced by these several insects?

Linnæus further says, that in the Rein-deer fly he saw the egg held out "like a white mustard-seed" at the end of the abdomen, which, if true, fully confirms the supposition that there can be no infliction.

The *Œstrus hæmorrhoidalis* and *Œstrus Ovis*, in performing their office of ovi-deposit, are also equally irritating and peculiar, as I have shown in the paper above alluded to, in the 3rd volume of the Society's Transactions.

I avail myself of this opportunity in conclusion, to state, in addition to my former remarks on this genus, that it appears to me, as there is no *aculeus* or weapon of infliction at the end of the abdomen of the female of the *Œstrus Bovis*, that the egg is simply thrust down among the hair, till it meets the skin, and that then it is affixed to it by a glutinous liquor secreted at the same time; and that the egg being hatched, the young grub insinuates itself into, and finally through the skin, forming an abscess beneath it. In a somewhat similar manner it is that the ichneumon flies deposit their eggs on the sides of living caterpillars of the *Lepidoptera*, and hatching, perforate their skins, and entering within, live on the parenchyma or pulp of their bodies till matured and fully grown, when they make their way out again and change to the chrysalis.

I may also remark of the *Œstri*, that they appear to be wonderfully kept from such an increase as would be fatal to the animals they feed upon, by the difficulties and imminent hazards they are exposed to in the act of depositing their eggs. The teeth of the horse must destroy, one should imagine, nine-tenths of the *Œ. Equi*, *hæmorrhoidalis*, and *salutiferus*. The *Œstri* seem however, in the hands of Providence, to make a double recompense for the sufferings they occasion; first, by keeping the animals on the alert during hot weather, when they would be often too idly disposed for their welfare; while the few larvæ which succeed in getting into their bodies, appear to benefit them by their local irritations, stimulating the stomach to a quicker digestion of their watery food, and diverting diseases by their counter irritations of the skin and frontal cavities,—thus producing the effect of issues or vesicatories, which are powerful remedies in relieving and in preventing diseases.

I apprehend that I have now sufficiently shown that the *Œstrus* of the ancients could have been no *Tabanus*, and that it is clear Olivier, who appears to have originated this notion, and who was followed by Latreille, was mistaken.

A very extensive enumeration of this genus is seen in a late ingenious publication, the *Systematische Beschreibung* of J. W. Meigen. It is however in some instances not correct; for on carefully examining the *Œstrus lineatus* of this writer, introduced from Villers, it would appear to be that stumbling-block of systematists in entomology, the *Œ. Bovis* of my enumeration*, and not of Linnæus, as he states, who, as we have repeatedly said, described the *Œ. Equi* for this species. The *Œ. pictus* of this author, beautifully figured by Curtis in the British Entomology, *no.* xxvi. *t.* 106, I rather suspect to be the faucial bot of the Stag†.

As the species of the new genus *Cuterebra* were taken for *Œstri* till I separated them, and are closely allied to them in their habits, I have ventured at the close of this paper to communicate to the Society a new and undescribed species lately received from America, along with some other insects sent me by my nephew, Joseph Clark, from the Illinois.

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* The lines on the thorax, and the figure of Villers, undoubtedly confirm it. Meigen's *Œ. Bovis* is the *Œ. Bovis* of my enumeration, under which this should have come as a synonym.

† I may here observe, that a few days since, in visiting the British Musuem, I was shown the insect Dr. Leach has called *Œstrus Clarkii*, and find it only a variety, and scarcely that, of the *Œstrus veterinus* of my enumeration.

CUTEREBRA FONTINELLA.

C. thorace atro, lateribus albis; abdomine violaceo, ultimis segmentis albis, nigro-punctatis.

White-tailed Cuterebra, or Blue Rabbit Fly.

Habitat in Illinoe Americæ Septentrionalis, cuniculos infestans.

DESCR. *Cuterebrâ Cuniculi* dimidio minor; atra, subcylindrica, cum capite parùm latior. *Frons* insuper atra et circa oculos lucida, infra albida, pilosa, utrinque puncto elevato atro. *Oculi* picei. *Thorax* insuper ater, latè per medium et ad latera pilosus, albus, punctis tribus nigris utrinque notatus. *Alæ* obscurè nigro-fuscæ, sulcis valdè puculatæ et rugosæ, corpore longiores; ad basin squamulâ foliaceâ erectâ, magnâ: tympanum halterem tegens magnum, convexum, marginatum. *Abdomen* breve, atrum, lucidum, supernè violaceo resplendens; segmentis duobus postremis hirtis, albidis, punctisque variis atris elevatis, glabris. *Anus* utrinque quasi forcipe prehensorio armatus. *Pedes* atri.

XLVIII. *Notices respecting New Books.*

An Introduction to Geology: comprising the Elements of the Science in its present advanced State, and all the recent Discoveries. By ROBERT BAKEWELL. *The Third Edition, entirely recomposed, and greatly enlarged. With new Plates, Cuts, &c. Octavo. pp.* 540.

THE readers of the Philosophical Magazine, may probably remember, that after the first edition of Mr. Bakewell's Introduction to Geology appeared in 1813, the late Mr. John Farey, a practical geologist and accurate observer, published a very copious analysis of the work, in several Numbers of the Magazine, (see vol. xli. and vol. xlii.*) A second enlarged edition of the Introduction to Geology was soon after published; and it was translated into German by Mr. Fred. Müller, of Friburg. The work has now been several years out of print, and has been frequently called for. We shall extract from the author's Preface a brief account of what has been done in the third edition, in order to present a distinct view

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* Mr. Farey opposed with some warmth, the parts of Mr. Bakewell's work which were at variance with his own opinions on the Geology of Derbyshire; but he concludes his strictures with the following remarks: "I beg however distinctly to state, that I have yet perused no systematic work on Geology, which I think entitled to a general and careful reading, in any degree compared with Mr. Robert Bakewell's 'Introduction to Geology,' and which I sincerely wish may be read and studied by hundreds of persons, with no less care and attention, than I have bestowed upon it. Because I can assure them that it contains more facts concerning our planet, and fewer absurdities and whimsical assertions and theories, than any of the numerous systematic works which have preceded it, in our own language, or I believe in any other."—*Phil. Mag.* vol. xlii. p. 246.

of the progress of geology, and the important discoveries that have recently been made.

"The causes which have retarded the publication of a third edition it is unnecessary to mention: the delay has, I trust, been favourable to its appearance in a very improved state; as I have been collecting materials for it, during several years, having visited almost every situation of much geological interest in our own island, from the Land's End in Cornwall, to the Grampian Mountains in Scotland; and passed part of three years in examining the geology of Savoy, Switzerland, and France. There is scarcely a rock-formation described in the present volume, that I have not examined in its native situation, and compared with the descriptions of former geologists. I have also had opportunities of examining the collections, and of profiting by the communications, of some of the most eminent geologists on the Continent.

"While engaged in these pursuits, I have not been inattentive to the labours of other observers. So numerous and interesting are the discoveries made in geology during the last ten years, that in order to present a concise view of the science in its present advanced state, the 'Introduction to Geology' has been recomposed, and all the chapters are greatly enlarged.

"The following new Chapters have been added:—Chap. II. On Fossil Organic Remains. Chap. IV. On the Principles of Stratification. Chap. X. A Retrospective View of Geological Facts. Chap. XVIII. On the Destruction of Mountains; and on the Bones of Land Quadrupeds, found in Diluvial Depositions and in Caverns. Chap. XIX. On the Formation of Valleys; and on Deluges and Denudations.—The Plates are new, except Plate IV. and part of Plate VII."

"In the course of the present work, I have frequently attempted to elucidate the geology of England, by comparisons with situations I have examined on the Continent, in order to connect the geology of our own island, with that of France, Switzerland, and Savoy.

"By comprising the numerous facts and observations contained in the present volume, within the limits of an elementary work, from the desire to be concise, I may have run the risk of becoming obscure: this I have studiously endeavoured to avoid; my chief aim being to present the reader with a system of geology, which shall explain geological phænomena in a clear and intelligible manner, and as free from technical obscurity as the nature of the subject would admit. In order that the price may not exceed that of the last edition, this work is printed in a smaller type."

XLIX. *Proceedings of Learned Societies.*

LINNÆAN SOCIETY.

March 4. A COMMUNICATION from the Rev. Leonard Jenyns, M.A. F.L.S. was read, On the distinctive characters of two British species of *Plecotus* Geoff. supposed to have been confounded under the name of *Long-eared Bat.*

A new Bat found adhering to the bark of a pollard willow, and which the author names *brevimanus*, is discriminated in this Memoir from the *auritus*, which together with *barbastellus* make up Geoffroy's subgenus *Plecotus* of the Vespertilionidæ.

Spec. Char. *P. brevimanus*, Lesser long-eared Bat, " vellere suprà rufo-fusco, subtùs albescente; auriculis oblongis, capite haud duplò longioribus; trago ovato-lanceolato; caudâ antibrachium longitudine æquanti, apice acuto."

The difference in absolute size, in the relative proportions of the parts, in the colour, and in the apparent habits, seem to require the making it a distinct species.

March 18.—The chair having been taken by A. B. Lambert, Esq. V.P., Edward Forster, Esq. the Treasurer of the Society, communicated to the meeting the afflicting tidings which had arrived during the day, of the decease of Sir James Edward Smith, their eminent and much-beloved President,—an office to which he had been appointedby the annual and unanimous choice of the Society from its first establishment in 1788, till his death. The Society immediately adjourned.

GEOLOGICAL SOCIETY.

Feb. 15.—This day being the Anniversary, after the election of Officers and Council, as noticed in our last Number, a Report from the Auditors and Council was read and approved; after which the following Address was delivered from the chair by W. H. Fitton, M.D. F.R.S. &c. the President of the Society.

GENTLEMEN OF THE GEOLOGICAL SOCIETY.

You have just received from your Council a report on the condition of your finances; with a statement of the accessions to your number during the past year, and of the measures adopted for advancing the welfare of the Institution. It remains for me to lay before you a few remarks on the branch of knowledge which the Geological Society is intended to promote: and what I shall offer upon this subject will be confined, in a great measure, to the state of Geology in this country; since neither have my opportunities of acquiring information during the past year enabled me to give, nor does my duty appear to call for, a more extended view;—though such periodical reports in other hands, and on more suitable occasions, have been frequently attended with advantage.

We have had since our last Anniversary to regret the loss of one of our foreign members. Mr. Brocchi, whose death, according to the accounts we have received, took place in Egypt, whither he had been invited to discharge the duties of a mining Engineer, is distinguished in the recent scientific history of Italy by numerous contributions to the Geology of that country;—and his principal work "On the Fossil Conchology of the Subapennine Hills," abounds in valuable observations, and in proofs of accuracy and acuteness in the comparison of the fossil and existing species. His talents, however, were not merely those of an observer;—his general views were always wide and philosophic; and the style of his writings is considered by competent judges as remarkable for its purity and good taste. But those only, who have had the pleasure of being personally acquainted with Mr. Brocchi, could appreciate his patriotism and philanthropy, the variety of his acquirements, and the spirit and eloquence which rendered his conversation more than commonly instructive.

The printed "Proceedings" of the Society, and the portion of the Transactions published within the year, are the best records of our contributions to geological science during that period: and the volume now in progress will, I trust, be found to have contributed in no small degree to the advancement of inductive Geology. New monsters, it is true, have not been brought to light from the depths of our strata; nor has Zoology been enriched with new genera, by such rare coincidences of genius and good fortune as distinguish the last volume of our Transactions: but the Geology of England has been illustrated by various memoirs, on tracts not previously examined; and by more exact and extended researches on portions of our strata, the general relations of which had been before determined. Correct data have thus been recorded, to which inquirers in other countries may refer, for the purpose of comparison with their own.

I have to congratulate you upon the progress which has been made in the Trigonometrical survey of Ireland; a work designed with all the skill of modern science, and committed for the execution to such hands, and with such instruments, as to leave no doubt of the result. Maps alone, such as this survey will produce, are an acquisition of the first importance to our inquiries; since they form one of the chief and indispensable instruments of geological research:—but with these, in the present case, will be connected a series of observations more strictly geological, which cannot fail to throw great light upon the structure and composition of the country to which the operations extend. The Tract, which I now show you, has been drawn up by one of the principal officers engaged in the Irish survey*, and lithographized for the use of the subordinate surveyors; and it contains so clear and able a system of instruction for their guidance, illustrated by sectional sketches, as greatly to facilitate the task of geological investigation. The surveyors will thus accumulate a series of specimens, the precise places of which will have been recorded in maps upon a very large scale,—on which also the heights above the sea will be determined, in points almost innumerable; while sections are taken in well chosen situations, for the purpose of illustrating more effectually the order of the strata. The ultimate results of operations so well combined, must be equally honourable to those who are engaged in this vast work, and fertile in various and substantial advantage to physical science.

But while the survey of Ireland is in progress, it is to be hoped that that of England will not cease to advance; and that no great delay will take place in the publication of the maps which have been actually prepared by the Ordnance. To geologists who have travelled in England, I need not mention the benefits that our science has derived from the maps already engraved; nor dwell upon the misery of plunging, from a tract that we have traversed with the advantage of this guide, into regions where the survey leaves us, lost, as it were, and bewildered from the want of such assistance. The sheets of the Ordnance Survey which I now lay before you, represent a portion of the midland counties, coloured geologically by a gentleman whose activity and accuracy of research have made him minutely acquainted with the stratification of the district around him*; and the maps thus coloured, are probably as complete specimens of geological illustration as ever have been produced. The knowledge of this observer extends with equal precision several miles to the north of the tract here represented; but these sheets, you perceive, are bounded by a right line;—and beyond that line it has not been in his power to extend his colours, because no good map of the adjacent district is in existence. In this instance therefore, and no doubt in numberless other cases, the want of adequate maps may cause the final and irreparable loss of much geological information: And when it is considered, that Geology is but one of many departments of useful inquiry, to which good maps administer,—how much they contribute to the advancement of commerce, and to the comforts and conveniences of life,—it will be unnecessary to urge the enlightened and public spirited persons, to whose hands this great undertaking is committed, to finish with as much promptitude as possible what has been so admirably begun.

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* Captain Pringle, of the Royal Engineers.

The effective establishment in this country of a society for the cultivation of ZOOLOGY,—a source of just gratification to all who are interested in the progress of Natural History,—is an event connected very intimately with the advancement of our subject: for to the Geologist it is of great importance to obtain facility of access to cabinets and to living specimens, in elucidation of fossil remains; and to have the privilege of appealing, in doubtful cases, to competent authorities, in what relates to the animal kingdom. But the connection of Zoology with our science, is a field too wide to be discussed upon the present occasion; nor would my own acquaintance with the subject justify my dwelling upon it.

The numerous provincial institutions, which have been recently established for the promotion of useful knowledge, will also materially contribute to the diffusion of a taste for Geology; and will throw new light upon the structure and productions of their respective districts.

I wish that it had been in my power to speak with equal gratification, of the relation in which our subject stands to another principal department of Natural History; but the fossil remains of the vegetable kingdom do not appear to occupy, at present, a just share of the attention of Botanists in England: and hence it has happened, that of the numerous and interesting specimens of fossil plants continually brought to light from our strata, especially within the coal districts, the greater part has been sent for illustration to those naturalists on the continent, whose publications upon this branch of inquiry, are so creditably known to science. Ought we not then to imitate the example of those, for whose labours we have so much reason to be grateful; and to reflect, that—if the botanical characters of fossil specimens be obscure, and the investigation of them at present unsatisfactory,—the subject is still comparatively new, and the difficulties such as perseverance and the multiplication of specimens must every day diminish: whilst the views to be derived from the connection of vegetable remains with geology, are scarcely inferior in interest to those already disclosed by the fossil remains of animals? The distribution of plants upon the former surfaces of the globe,—its relation to the epochs of geological deposition,—the variations it may have undergone from change of climate, either by alteration of internal temperature, or of elevation above the sea;—the former existence of vegetation in the more complex forms, in tracts where scarcely any traces of it exist at present,—are subjects which give rise to some of the most important general questions connected with the history of the globe;—and that require for their due consideration such an acquaintance with the characters of fossil vegetable remains, as none but the most skilful and experienced botanists can be expected to possess.

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* Mr. Lonsdale of Bath.

On the Geology of foreign countries, the last year has not been unproductive. A valuable paper on the structure of Jamaica, has been published in our Transactions, by one of the most skilful of our practical observers. We have received a very important contribution of specimens from Captain Franklin and Dr. Richardson, under whose direction the expedition to the northern coast of America has been conducted with so much ability and success;—and a memoir by the latter, on the structure and components of those regions, will soon be read at one of our meetings. Captain Parry also, and Captain Foster, have presented us with a valuable collection of specimens from Spitzbergen, obtained during their late expedition to the north. Captain King, who has enriched our cabinets with specimens from the coasts of Australia, and done so much for other departments of natural history, has recently sent home a collection of rocks obtained during the earlier part of his survey in the Straits of Magellan; and further collections, accompanied with new information, may still be expected from the same indefatigable observer. We have reason also to hope that Geology will not be neglected during the expeditions soon about to sail,—of Captain Boteler, for the survey of the western coast of Africa, and of Captain Foster, for the purpose of determining the longitude of important stations on the shores of the Atlantic.

There is the greater reason to rejoice in the contributions thus given, or to be expected, from the Naval department of the public service, since it has not unfrequently been the reproach of this country, that—possessing colonies, which have dispersed the natives of Britain in every region of the habitable globe, and commerce, that maintains continual intercourse with them,—the benefits conferred by England on the natural history of distant countries, have fallen very far short of what the intelligence and activity of our national character might have afforded. Let us hope, however, that brighter days are opening upon us; and that those who are employed in the various departments of our foreign service, will universally feel, that where such frequent opportunities of advancing useful knowledge are likely to occur, an acquaintance with branches of science not immediately essential to professional duty, is strictly accordant with the dignity of the naval and military character.

Among the donations of foreign specimens to our cabinets, there is one of very peculiar interest;—the rich collection of fossil bones and shells presented to us by Mr. Crawfurd from Ava: which has the greater value, as it is one of the first collections of this description, that has made its way into England from our extensive empire in the East, or the adjoining territories. These specimens afford some very striking novelties, both to the Geologist and Zoologist; an account of which, I trust, will soon be laid before you by competent describers.

The last year has produced some valuable publications on the Geology of Volcanoes, which, though not emanating immediately from this Society, are the work of our own members. We are indebted to Dr. Daubeny for a judicious volume, in which he has combined what had been previously published on volcanoes, with much valuable observation of his own. The productions of Mr. Scrope, though his speculative views are not free from objection, are full of originality and talent.—To that especially, which describes the extraordinary volcanic region in the centre of France, illustrated with such effect as to render the task of comparison with other districts easy and inviting, I should have had pleasure in alluding more fully; if an eloquent account of it, in one of our leading journals, were not familiar to us all *: and this, also I believe I do not err in ascribing to an active member of our Institution.

In the speculative department of Geology, nothing has been of late more remarkable, with reference to its history in this country, than the universal adoption of a modified Volcanic theory, and the complete subsidence, or almost oblivion, of the Wernerian and Neptunian hypotheses;—so that what, but a few years since, it was by some considered as hardihood to propose in the form of conjecture, seems now to be established nearly with the evidence of fact. It is no longer denied, that volcanic power has been active during all the revolutions which the surface of the globe has undergone, and has probably been itself the cause of many of them;—and that our continents have not merely been shaken by some mighty subterraneous force, but that strata, originally horizontal, have thus been raised, shattered, and contorted, and traversed, perhaps repeatedly, by veins of fluid matter;—operations which have produced phænomena, so nearly resembling those of recent volcanic agency, that to have so long disputed the identity of their cause, is one of the most remarkable proofs in the annals of philosophic history, of the power of hypothesis in distorting or concealing truth. Whatever, therefore, be the fate of the Huttonian theory in general, it must be admitted, that many of its leading pro-

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* Quarterly Review, Vol. xxxv. page 447, &c.

positions have been confirmed in a manner which the inventor could not have foreseen.

The most striking modern support of these correcter views, is due to Von Buch and Humboldt, and to the facts and inferences derived by Dr. MacCulloch from the country which gave birth to Hutton, and to his illustrator, Mr. Playfair, and in which were made the experiments of Sir James Hall. More recently, a series of facts observed by Professor Henslow, in the Isle of Anglesea*, has proved, in the most satisfactory manner, the connection of veins of trap with very high temperature; since the change produced upon the strata, through which the substances now occupying the veins were injected, has approached so nearly to fluidity, as to admit of their crystallization, in forms different from any which the components of the rocks, if they had not been thus acted on, would have afforded. Sir Humphry Davy's experiments on the fluids contained within cavities in crystals †, are another striking and unexpected confirmation of Hutton's views: and our own Transactions, besides various incidental pieces of evidence derived from this country, supply the testimony of an unprejudiced witness to an earthquake on the coast of Chili‡, which brings almost before the eyes of the reader, the movement and permanent elevation of the land.

Having alluded to Mr. Playfair's support of the volcanic theory, it would be unjust to the memory of that distinguished man, not to mention, that his geological writings have had, indirectly, an effect in accelerating the progress of our subject, the benefit of which we experience at this moment, and probably shall long continue to feel; and which, perhaps, outweighs in value the partial success of the speculations for which he so strenuously contended. He clothed our subject with the dignity of an eloquence most happily adapted to philosophic inquiry, and redeemed the geologist from association with that class of naturalists who lose sight of general laws, and are occupied incessantly with details;—placing him, where he ought to stand, beside the mathematician, the astronomer, and the chemist; and permanently raising our science into an elevated department of inductive inquiry. His mild and tolerant character threw an assuaging influence upon the waves of a controversy, which in his time considerations entirely foreign to science had exasperated into unusual violence: and if, fortunately, there is no longer any trace of this asperity, the change must, in a great degree, be ascribed to the tone of Mr. Playfair's writings, enforced by the manly and consistent tenour of his blameless life.

I cannot, for your sake, regret that the presence of some of those who have had a large share in the foundation of this Institution, prohibits my alluding to their continued and unremitting efforts in support of it.—And the same cause prevents my dwelling on the effects produced, at both our Universities, by the geological instructions delivered there; which have given to the subject an impulse perhaps without example in the history of those institutions, and gone far to render natural science a permanent department of general education.

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* Transactions of the Cambridge Philosophical Society, vol. i. page 406.

† Philosophical Transactions, 1822, page 367, &c.

‡ Geological Transactions, second series, vol. i. page 413.

But there is one of our number, whom professional and domestic occupations retain so much in a remote quarter of the country, that we have seldom the gratification of his presence amongst us, though his writings are in all our hands: and it is a duty,—not to Mr. Conybeare, but to the subject, and to ourselves,—to say, that among the more recent causes which have accelerated the progress of Geology in England, the publication of the "Outlines of England and Wales," by him and Mr. Phillips, has had an effect, to which nothing since the institution of this Society, and the diffusion of the geological maps of England, can be compared. It is with peculiar pleasure that this statement can be made in this place; since a large proportion of that work has been derived from our own Transactions, and the authors have long been distinguished members of our Society. Of course their publication is not free from defects and inequalities,—inevitable perhaps in a first edition, composed for the greater part during its progress through the press:—but, regarding it as the first general sketch of a country so complex as our own, it may be said without fear of contradiction, that no equal portion of the earth's surface has ever been more ably illustrated;—nor any geological work produced, which bears more strongly impressed upon it the stamp of original talent for natural science.

The object, however, of our Institution, to adopt the language of the charter, is "to investigate the mineral structure of the EARTH;"—not to confine ourselves to the British Islands only, (and even they are best illustrated by comparison,) but to extend our researches if possible, to every part of the globe;—to record the geological phænomena of the most distant countries, as well as of our own,—and from the whole, derive the laws that have regulated the structure of this planet, and still influence the changes which are in progress upon it. It is our good fortune, and the fact is intimately connected with the commercial wealth of our country, that it affords a greater variety of strata and of geological appearances, than most other portions of the civilized world of such limited extent; while the range and variety of our coasts unveil the geological anatomy of England, with an obviousness and convincing facility to the observer, that have greatly accelerated our inquiries. The Geology of England, therefore,—which, with a view only to commercial advantage, and to the comforts and conveniences of life, would have well deserved all the labour that has been bestowed upon it,—acquires a new and more dignified interest, when we reflect that this island is in a great measure an epitome of the globe; and that the observer, who makes himself familiar with our strata, and the fossil remains which they include, has not only prepared himself for similar inquiries in other quarters, but is already, as it were, acquainted by anticipation with what he must expect to find there. If, therefore, I were called upon to state in what manner those who have leisure, health, and talent for such inquiries, can most effectually advance the bounds of our science, and increase the reputation which England has begun to acquire

quire in this department of natural knowledge,—I should say, that it would be,—First, by rendering themselves accurately familiar with the geological phænomena of our own country; and then,—by taking abroad with them the knowledge thus acquired, and comparing the phænomena with those of distant regions; since it is only from the multiplication of such comparisons, that sound general views can be derived.

But even within the British Islands, there still are tracts, and of no small extent, which are comparatively, and a great part of them absolutely, unknown. More than one half of Ireland is in this condition: for the publications of Conybeare and Buckland, Stephens, Weaver, Griffith, and Dr. Berger, comprehend nearly all that has been done in that country. But this subject, as I have already mentioned, has passed into such hands, as will, no doubt, accomplish every thing that can be desired.

In the North and North-west of England, the labours of Otley*, Smith, Professor Sedgwick, and some other inquirers, have already ascertained the principal relations of one of the most important districts; but very little has yet been published upon it. And on the mountainous tracts in Wales, the ancient and very interesting essay of Owen †, and the valuable papers of Mr. Aikin and Professor Henslow, with that of Mr. De la Beche on Pembrokeshire, and of Mr. Martin on the Coal Basin of Glamorganshire ‡,—a tract on which Mr. Conybeare is occupied at present, comprehend nearly every thing that deserves to be mentioned here.

In Scotland also, notwithstanding the graphic and copious illustrations of Dr. MacCulloch, and the mineralogical skill and perseverance of other eminent naturalists who have applied themselves to the Geology of their native country,—no geological map has yet appeared; and a great part of that rich and varied region remains to be explored. But the Society will have pleasure in observing, in the last portion of their Transactions §, that an effective comparison of the more recent strata of Scotland with our English formations has already begun. The memoir of Mr. Murchison on the Brora Coalfield is an excellent specimen of what may be effected in this department of inquiry; and a paper produced at the last meeting by the joint labours of Professor Sedgwick and Mr. Murchison, leaves no doubt that the remaining memoirs which are to be expected from those gentlemen, will throw great light on the comparative geology of that distant portion of our island.

The value, however, of the researches and identification at Brora, goes much further than the mere comparison of a remote tract, with the stratification of England: they confirm a suggestion of Dr. Buckland and Mr. Lyell, that the coal formation of that neighbourhood was in reality the equivalent of a portion of our Oolitic strata; and demonstrate the remarkable fact, that the same fossils which in England occur in oolitic limestone, exist there in strata of quartzose sandstone and of shale! The whole series indeed, of the phænomena developed by recent examination in Scotland and the north of England, gives rise to the most interesting speculations on the questions of geological identity, and of the relative value in geology of mineralogical and zoological characters,—which has been so ably treated by Brongniart and other continental writers:—questions, which it is necessary to keep continually in view, and that acquire fresh interest and importance in proportion as we extend our researches to the remoter districts of the world.

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* The work here referred to, is a brief but valuable notice, "On the succession of rocks in the district of the Lakes," published in the Lonsdale Magazine, or Provincial Repository, for October 1820:—Vol. I. No. x. pp. 433, &c.

† Dated in 1570:—See Cambrian Register, for 1796, and Geol. Trans. N. S. Vol. I. page 312.

‡ Philosophical Transactions, 1806. page 342.

§ Second Series, vol. ii. p. 293.

To those amongst us who are confined to England, the most useful task perhaps would be, when we have mastered the general relations of our series, to take up some one portion of the subject,—a group of strata, or even a single stratum, or any one of the numberless questions connected with their zoological and mineralogical relations,—and to publish in the form of *Monographs* the results of our inquiries. For it may be stated with confidence, that there is not any one of our strata, however familiarly it may be supposed to be already known, that would not, if thus treated, reward the most elaborate and minute examination.

But those who are deprived of the privilege of travelling even in England, must not suppose that they can be of no service as geologists; or if they belong to our body, that they are thus released from their obligation to be active in our cause: and there are two descriptions of persons,—the resident clergy, and members of the medical profession in the country,—to whom what I am about to say may be more particularly deserving of attention. Such persons, if they have not yet acquired a taste for natural science, can hardly conceive the interest which the face of the country in their vicinity would gain, however unpromising it may appear, by their having such inquiries before them; how much the monotony of life in a remote or thinly inhabited district would thus be relieved; nor how much benefit they might confer on the natural history of their country. Even of those who have made some progress in geological studies, many, I apprehend, are prevented from investigating attentively the tracts where they reside, or from communicating their knowledge, by a belief that the Geology of England itself is sufficiently known already; and that the district, with the phænomena of which they are themselves familiar, would have no interest or novelty for the world at large:—whereas it may be asserted (and it were easy to produce examples from modern researches in some of the counties near London), that there is no district that will not furnish sufficient interest and novelty to an attentive inquirer, not merely to repay his own exertions, but to instruct the most learned, and enlarge the bounds of our science.

To landed proprietors also, it can hardly be known, without some tinge of geological information, how nearly our subject is connected with Agriculture,—with an acquaintance with the nature and correctives of the soil, the supply of water, and facility of effectual drainage, and numberless facts essential to the perfection of rural œconomy; the discovery and supply of stone, for building and the construction of roads, the choice of the line of roads and of canals, and the facility of their execution. All these are but a few of the topics that come strictly within the province of the geologist: and which are so essential to the prosperous management of landed property, that a geological map may perhaps with truth be considered as not less necessary to the country gentleman, than the topographical plan of his estates.

I am fully aware, that much of what I have just said is obvious;—and even familiar, to the greater part of those who hear me:—But my object is to be useful; and I believe that some of those whom these remarks are likely to reach, are not sufficiently acquainted with the practical advantages derivable from our pursuits;—and that others are unconscious of the means within their own power for advancing them.

I shall conclude, Gentlemen, by congratulating you on the good feeling by which the proceedings of this Society have always been characterized; and on the self-command that renders both agreeable and instructive the conversations, (I will not call them discussions—much less debates,) with which it is now our practice to follow up the reading of memoirs at our table; and which have given to our evening meetings a character more like that of social intercourse in a private circle, than of the formal proceedings of a public body. This practice, I know, has been a subject of doubt, to many who wish well to our institution, and do not undervalue the personal character and disposition of our members. But, so long as our conversations are carried on with the urbanity by which they have hitherto been distinguished,—while it is the wish of those who share in them to give or to receive information, and not to shine,—and the object is not victory but truth,—there seems to be no reason to apprehend any very serious injury from the continuance of our geological warfare.

There is still another train of thought connected with our meetings, on which I confess I have sometimes delighted to dwell. The spirit in which they have been conducted has been so kind,—so little tainted with, or rather so perfectly free from, any admixture of the leaven with which from interest or ambition most of the pursuits of life are embittered;—and our duties here have been associated with so many offices of cordiality and friendship;—that when, in after life, the cares and chances of the world may have dispersed those whom I have now the happiness to see around me, I am fond to believe that the remembrance of these evenings will be called to mind with pleasure:—And I feel confident, that, as many of us already derive the chief part of our enjoyments from the friendships to which congenial pursuits have led, the Geological Society will continue to be no less effective, in the production of warm personal attachment, and of manly and ingenuous intercourse among its members, than it has been, in maintaining an active and energetic spirit of research.

March 7.—A Paper was read "On the Geological Relations and Internal Structure of the Magnesian Limestone, and the lower portions of the New Red Sandstone series, in their range through Nottinghamshire, Derbyshire, Yorkshire, and Durham."—By the Rev. A. Sedgwick, M.A. F.R.S. V.P.G.S. &c.

A sketch of the subjects contained in this paper was laid before the Society in 1826 (Nov. 17) :—They were resumed in a more systematic and detailed form during two meetings in 1827; and are now terminated by the observations read at the present meeting.

The contents of the Memoir are presented in the following order :

Part I.—§ 1. *Introduction.*—The new red sandstone is considered as one great complex formation, interposed between the coal measures and the lias ;—with two calcareous formations subordinate to it, one in the lower part of the series (the *magnesian limestone*), and another in the upper part (the *muschel-kalkstein*). The lower of the two calcareous formations is considered in detail; the upper has not yet been discovered among the British secondary deposits.

§ 2. *External characters of the country through which the Magnesian Limestone ranges.*—The form of the western escarpment is described, and is supposed to exhibit proofs of great denudations; and the general character of the soils resting upon the formation is noticed.

§ 3. *General distribution of the formation.*—The range of the escarpment is given in great detail; some errors of the geological maps are corrected; and in describing the eastern boundary, the enormous masses of diluvium in the county of Durham are briefly noticed.

§ 4. *Outliers.*—Sixteen outliers from the western escarpment are described; the most southern of which is at Conisborough. In addition to these, there are eight detached patches of magnesian limestone on the line of bearing, which are not considered as outliers. The most remarkable of these are seen in the range through Yorkshire.

§ 5. *Relations of the Magnesian Limestone to a succession of Coal Measures.*—In a general point of view these formations must be unconformable, because the overlying beds are extended far beyond the limits of the productive parts of the carboniferous order: and the fact is also proved by actual sections in several parts of Yorkshire and Durham. At the same time there are continuous tracts of country where the want of conformity does not appear, and where the overlying beds seem almost to graduate into the coal measures. Several details are given respecting ancient coal works, in which, in more than one hundred places, the coal had been extracted by *shafts sunk through the magnesian limestone:* and it is asserted that the quality of the coal is never injured by the presence of the overlying formations. Such injury is not only contrary to fact, but seems to be a physical impossibility.

§ 6. *On the Faults affecting the Magnesian Limestone and Coal strata, Trap dykes, &c.*—Examples are given of some great faults which traverse both the carboniferous and the superior formations: but it is remarked that many of the dislocations of the lower order of rocks do not affect the upper. Respecting the age of the trap dykes of the coal-fields, it is not possible to determine their epoch in comparison of the magnesian limestone, where they range up to the escarpment; and of such dykes there are only two examples; one of which does, and the other does not, pass through the beds of the overlying series.

Part II.—*Internal Structure and great Subdivisions of the Magnesian Limestone.*—Considered as a subordinate part of the *new red sandstone* series, this formation admits of five natural subdivisions, each of which is described in a separate section.

§ 1. *Lower Red-sandstone,* or *Rothe-todte-liegende.*—In Yorkshire this appears generally in the form of a coarse siliceous sandstone, of a reddish tinge. It is associated with incoherent sand, red micaceous shale, and sometimes with variegated marls. In Durham it is generally represented by a yellowish and nearly incoherent sand. In some places it cannot be distinguished from the gritstone beds of the coal measures: but as it commences in the edge of Derbyshire, and is almost co-extensive with the magnesian limestone as far as the mouth of the Tyne, it must on the whole be unconformable to the inferior order. It is, however, of very unequal thickness, and its upper beds are not always parallel to the strata of limestone which rest upon it. In Durham, being of loose texture and pervious to water, it throws the greatest difficulties in the way of mining operations carried on within the limits of the limestone.

§ 2. (a). *Variegated Marls, with irregular Beds of Compact and of Shell Limestone.*—This deposit is not either of great extent or thickness, and is confined to a small part of the escarpment in Nottinghamshire and Derbyshire. It is supposed to be contemporaneous with the following subdivision:

§ 2. (b). *Marl-slate, and Compact Limestone.*—This is much more extensively developed than the preceding formation; and though by no means coextensive with the yellow limestone, derives importance from its constancy of position and from its fossils. Several localities in the county of Durham are described; and among the beds of marl-slate of East Thickley, &c., two or three species of fern have been discovered; and seven or eight species of fish, four of which at least seem to be identical with fish of the *Copper-slate.*

§ 3. *Great central deposit of Yellow Limestone.*—It is subdivided into the following modifications, each of which is described in detail. (1) *Dolomite,* a simple crystalline rock.—(a) *Arenaceous dolomite,* coarse, nearly incoherent, often in minute rhombs.—(b) *Small-grained dolomite.* Many quarries of this variety are described as existing on the back of the deposit, and extending from the neighbourhood of Mansfield to Bramham Moor. The crystalline beds pass into others of mechanical structure, and in some extreme cases contain 20 or 30 per cent of siliceous sand.—(2) *Compact magnesian limestone.*—(3) *Laminated.*—(4) *Earthy.*—(5) *Masses of irregular concretionary structure.*—(6) *Beds or concretionary masses of crystalline limestone without magnesia.* Examples of these are derived from quarries near Ripon, Knaresborough, and Newton Kyme, &c.

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—(7) *Brecciated structure.* This modification abounds on the coast of Durham.—(8) *Small concretionary structure.*—(a) *Irregular.*—(b) *Regular or oolitic.*—(9) *Large globular concretionary structure.*—Of this, four principal modifications are described with minute detail. All these several subdivisions of structure are supposed to have been produced by great internal movements, after the mechanical deposition of the formation.

§ 4. *Lower Red Marl and Gypsum.*—This extends from the edge of Nottinghamshire to the banks of the Wharf; thins off at the two extremities; attains its greatest thickness (perhaps nearly 100 feet) on the right bank of the Ain;—but has not been discovered in Durham or the northern parts of Yorkshire.

§ 5. *Upper thin-bedded Gray Limestone.*—Near Ferry Bridge this contains very little magnesia. In other places it contains subordinate dolomitic beds. It commences at Carlton near Worksop, and ranges without interruption to the left bank of the Wharf. Further north it reappears in several places, under a modified form: and the highest beds on the coast of Durham may perhaps be referred to it; but the classifications are made obscure by the absence of the *lower red marl.*

§ 6. *Great Subdivisions of the new red Sandstone which are superior to the dolomitic series.*—In Nottinghamshire these consist of two principal deposits.—(a) *Upper red sandstone.*—(b) *Upper red marl and gypsum.*—The same subdivisions may be traced near the mouth of the Tees. In the central parts of Yorkshire they are obscured by diluvium.

By way of conclusion,—the deposits described in § 1 and § 2, are supposed to be the equivalents of the *rothe-todte-liegende,* the *kupfer schiefer,* and *zechstein.*—The ore described in § 3, 4, and 5, are in like manner supposed to be the equivalents of the *rauchwacke, asche,* foliated *stinkstein,* breccias, and gypsum, which compose the upper part of the Thuringerwald system. The coincidence, in order, mineralogical character, and organic remains, seems to be nearly perfect. In like manner the two divisions described in § 6. are taken as the respective equivalents of the *bunter sandstein* and *keuper*; and, the enormously thick deposits between the coal measures and the lias, with the exception of the *muschel-kalkstein,* are thus found to admit of the same natural subdivisions in England and in central Germany. Finally, the author speculates about the origin of the dolomitic deposits, and adopts in part the theory which derives them from the mechanical destruction of the rocks of the carboniferous order. He states however two facts (1st, the greater abundance of magnesia than could have been supplied by the dolomites of the carboniferous limestone; 2ndly, the fact that some beds contain a greater proportion of magnesia than is found in true dolomites), which seem to imply that the waters of the ocean had a power of separating carbonate of magnesia from the preexisting rocks, in a manner which is not explained by the mere mechanical hypothesis. Whatever may have been the origin of the whole system; its extent, regular subdivisions, and characteristic organic remains, seem to prove, that it originated in the long continued and consistent operation of powerful causes, acting simultaneously in distant parts of the earth.

PROCEEDINGS AT THE FRIDAY EVENING MEETINGS OF THE ROYAL INSTITUTION OF GREAT BRITAIN.

Feb. 22.—The progress of printing by machines, from the improvement of the press by Lord Stanhope, to the present time, was briefly traced by Mr. Cowper, who at the same time illustrated the various inventions by drawings, models, and many of the working parts of the present machines: after which a more particular account was given of the machines invented by Mr. Kœnig and perfected by himself and Mr. Applegath*, and of the one recently erected at The Times newspaper office. The extraordinary speed of this machine is such, that 4000 impressions on one side can be taken in one hour. The disposition of the form, inking apparatus, paper rollers, and conveying tapes, were explained upon models and sections.

The library contained a variety of interesting objects, and especially several productions of the fine arts.

Feb. 29.—Mr. S. Solly, jun. gave an illustrated account of the analogies and differences between the skeletons of man and birds. The accordance between the skeletons was first pointed out, and that of man referred to as a standard; and then the departures and differences in different animals, with the effects thereby produced, were explained.

March 7.—Supplementary remarks on the subject of February 15th,—*i. e.* the reciprocation of sound,—were given by Mr. Faraday. New cases of reciprocation were adduced, and illustrated; especially of one column of air by another, and of beats and chords by columns of air. The formation of the grave harmonic was referred to, and cases of its reciprocation mentioned: and the theory before laid down relative to the Jews-harp, experimentally proved by an apparatus consisting of a column of air in a tube which could be lengthened or shortened at pleasure, and before which a Jews-harp was firmly fixed and made to vibrate. This apparatus gave all the sounds which could be produced by the mouth, at the same time that the columns reciprocating to the tongue of the instrument could be measured.

Presents: Works of art, specimens of paper prepared in France from straw, and other objects, were laid upon the library table.

March 14.—Mr. Turrell, the engraver, gave an account of the latest improvements in etching upon steel. He illustrated the processes of laying a ground etching, rebiting, &c.; of ascertaining whether the plates were in a proper state; of preparing peculiar menstruums for biting in steel, and many others which constitute the latest improvements of the arts. He showed the application of etching upon steel to the manufacture of graving tools, which could be manufactured only in an inferior manner by any other process; and concluded by explaining the principles and use of a new instrument invented by him, and called a Perspectograph.

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* We believe that all printing-machines which have hitherto been produced since Mr. Kœnig's original invention, have been varied applications and combinations of his principles.—Edit.

Amongst the objects in the library was a very large specimen of native silver from Mexico.

ROYAL ACADEMY OF SCIENCES OF PARIS.

Oct. 1. 1827.—M, Julia Fontenelle exhibited a well preserved head of an inhabitant of New Holland.—M. Latreille gave a very favourable account of the monograph, presented by M. Bois-Duval, of the tribe of *Zygenides*, of the order *Lepidoptera*.—M. Geoffroy-Saint-Hilaire, made a very favourable report respecting a notice of a monstrous child, by M. Rambur, physician at Ingrandes.—M. Poisson read a memoir on sonorous bodies; and M. Cauchy stated that he had made similar researches.—MM. Milne Edwards and Audoin presented anatomical researches on the nervous system of the *Crustacea*. M. Cagniard de Latour communicated the results of several new experiments which he had made respecting the vibration of sonorous bodies.

Oct. 8.—M. Tournal communicated some details respecting the fossil bones of the caverns of the department of the Aude.—Dr. Thomas Young thanked the Academy for having recently elected him a Foreign Associate.—M. Rousseau, of Coucy-le-Château, sent a memoir on the improvement of forceps.—M. Parseval sent a supplement to his last Memoir on Analysis.—M. de Senne communicated a work on the operation of tracheotomy.—M. Magendie gave a favourable account of M. Breschet's memoir on false consecutive aneurism of the heart, and true aneurism of the arteries.—M. Mirbel made a favourable verbal report on the botanical part of M. Freycinet's Voyage edited by M. Gaudichaud.—M. Frederic Cuvier read an extract of a work on the development of the bristles of the hedgehog.—M. Cagniard continued his communication respecting his researches on sonorous vibrations

Oct. 15.—M. Julia Fontenelle sent a sealed packet.—M. Delafuge presented a manuscript entitled: *Nouvelle Jérusalem apocalyptique.* —M. Joseph Anastasi sent a memoir On practical mechanics.—The Academy afterwards heard; A memoir by M. Binet on the solution of indeterminate equations of the first degree.—Researches by M. Despretz On the heat disengaged during combustion.—A memoir by M. Gasparin, On European climates.—Researches by M. Desvoidy on the vertebral organization of the *crustacea*, the arachnida, and insects.—And a work by M. Delpech on the resection of the inferior maxillary bone.

Oct. 22.—M. Champie sent a manuscript containing some new propositions in geometry.—M. Serullas announced that he had formed a bromide of arsenic.—M. Cordier gave a very favourable verbal account of the Geological Essay on the Environs of Issoire, published by MM. Devèse and Bouillet.—M. Lacroix, in the name of the commission, read an analysis of M. Binet's memoir On the determination of the orbit of comets, which was not approved of.—M. Savart read an extract of a memoir On the elasticity from bodies.—M. Despretz communicated some new experiments on the heat disengaged by combustion.—M. Cagniard de Latour presented the continuation of his experiments on what he terms *les chocs solides des cordes.*—The Academy decided that M. Fresnel's place should be filled up.

Oct. 29.—M. Coste, captain in the artillery, announced the forwarding of a work containing several mechanical experiments.—M. Fossard sent a manuscript entitled: *Utilité de l'horlogerie.*—The Academy heard a favourable account given by M. Mirbel, of M. Despreaux's work, entitled *Essai sur les Lamenaires des côtes de la Normandie;* and a report by M. Cordier on the researches made by M. Marcel de Serres on the extinct volcanoes of the South of France.—The Academy decided that the place vacant by the death of M. Laplace should not be filled up.—M. Payen read a notice relating to a new crystallized borate of soda, and its employment in the arts.—The Section of Physics afterwards presented, in a secret committee, the following candidates for the vacant place: MM. Savart; Becquerel; Cagniard de Latour, Pouillet, Despretz.

Nov. 5.—M. Bernier sent a memoir on the means of descending to the bottom of water.—The Academy proceeded to a scrutiny of the ballot for the election of a member: of 49 votes, M. Savart had 29, M. Cagniard de Latour 9, M. Pouillet 6, M. Despretz 5.—M. Cordier gave an account of the memoir on thermal waters, presented by M. Gendrin. It results from this memoir, as might be expected, and has been already proved, that thermal waters and common water cool in the same manner. The contrary opinion, however widely spread, is therefore a prejudice.—M. Cauchy communicated some new fundamental propositions on the calculus of remainders.—M. Adolphus Brongniart read a memoir containing new observations on the spermatic granules of vegetables.—M. Girard stated that the memoir lately presented by M. Anastasi was unworthy of any examination.—M. Raspail communicated observations on the motion of the tentacula of the *vorticella.*

Nov. 12.—M. Marcel de Serres sent from Montpellier a sketch of a geognostic map of the department de l'Hérault.—M. de Freycinet read a letter from M. Gaymard, dated New Zealand, containing several details respecting the voyage of the Astrolabe.—M. Girard read a memoir respecting some standards of the ancient Egyptian cubit, lately discovered.—M. Boyer read a favourable account of the memoirs presented by M. Faure respecting the iris and artificial pupils.—M. Barry, physician of Limoges, sent a memoir concerning two cases of luxation of the cervical vertebræ with compression of the spinal marrow, which he stated he had reduced: The commissioners named by the Academy did not approve of it.—M. Coquebert gave a verbal account of the new *Annales de Sciences naturelles,* published at the Havana by M. Ramon de la Sagra.—M. Delpech read a notice respecting his labours relating to *Rhinoplastie.*

Nov. 19.—The President of the Council of Ministers informed the Academy, that a marble bust of M. Laplace would be executed for the Library of the Institute.—M. Jomard presented engravings of four measures of the Egyptian cubit.—M. Dupetit-Thouars gave a verbal report respecting the work, entitled *Disposition méthodique des mousses;* by M. Walker Arnott.—M. Bouillaud read Experimental researches on the functions of the posterior portion of the brain.—M. Willermé read the remainder of his Memoir on the distribution of conceptions and births, in relation to the seasons.—M. Sérullas read Experiments on the iodide of antimony, and laid upon the table some observations relating to the bromide of bismuth.

Nov. 26.—The Minister of the Interior sent a Notice of M. D'Hombres-Firmas respecting the fossil bones found in the environs of Alais.—M. Saint-Hilaire deposited the manuscript of a new work which he proposed to publish under the title of *Flore et Pommone françaises.*—M. Damoiseau, in the name of a Commission, gave an unfavourable account of the chronological tables by the Abbé Lachèvre.—M. Le Gendre verbally announced the recent ingenious labours of M. Jacobi of Konigsberg. This geometer has greatly perfected the important theory of elliptical functions.—M. Dupin, in the name of a Commission, read the first part of a report respecting M. Brisson's Essay on the general system of French navigation.—M. Cagniard de Latour read some New experimental and theoretical researches on the properties of sound.

December 3.—MM. Gauthier de Claubry et Person requested that a sealed note which they had deposited, should be given to the Commissioners, who were to give an account of the Memoir on madder by MM. Robiquet and Collin.—M. Malbouche announced that he had successfully practised the method for the cure of stammering, invented by Mrs. Leigh of New York.—At his request two Commissioners named by the Academy were to attend the experiments.—At the request of the Academy of Inscriptions, two members of the Academy of Sciences were added to the Commission named by them, for examining the Egyptian cubits lately discovered.—M. Duméril, in the name of a commission, reported respecting M. Chabrier's Memoir on the progressive motion of man and animals.—M. Gay-Lussac made a verbal report upon a pamphlet by M. Burridge, On the improvement of civil architecture.—M. Biot read a memoir On the figure of the earth.

OBITUARY:—SIR J. E. SMITH.

On Monday, the 17th of March, died at his house in Surrey Street, Norwich, his native city, aged 68, Sir James Edward Smith, M.D. F.R.S. Member of the Academies of Stockholm, Upsal, Turin, Lisbon, Philadelphia, New York, &c. &c. the Imperial Acad. Naturæ Curiosorum, and the Royal Academy of Sciences at Paris, Hon. Mem. of the Horticultural Society; and President of the Linnæan Society, which office he had held from the first establishment of the Society in 1788.

Of this eminent Naturalist and most excellent and amiable man, as time does not permit us in our present Number to give such a notice as is due to his station and his merits, we must fulfill that duty at a future time. We shall now only add, that he had laboured nearly up to the day of his decease (though often impeded by sickness), with unabated zeal and success in the advancement of his favourite science; the fourth volume of his "English Flora" having been published but a few days before his death. The former volumes of this masterly work have been noticed by us on their publication *. At the close of the volume which has just appeared, are the following remarks, which will now be read with melancholy interest by the friends and admirers of the much lamented author.

"Several circumstances have caused a long delay in the publication of the present volume, which, if their recurrence should not be prevented, may render the completion of the work, according to its original plan, very precarious. In the mean while, the number of volumes originally proposed is now finished, and the first twenty-three Classes are completed, as well as the first Order of the twenty-fourth, *Cryptogamia Filices,* the only one that required more study and emendation than it has hitherto received.

"Of the remaining Orders, the *Musci* have been detailed in the Latin *Flora Britannica* and *Compendium* of the author, as well as in his *English Botany;* and by other well-known writers, in two editions of the *Muscologia Britannica,* and the *Muscologiæ Hibernicæ Spicilegium.* The monograph of Dr. Hooker on British *Jungermanniæ,* which, with their allies, constitute the next Order to the *Musci,* diffuses a new light over the whole of that Order. The works of Mr. Dawson Turner on *Fuci,* and of Mr. Dillwyn on *Confervæ,* have gone far to exhaust the species of those tribes; an application of scientific principles to the settlement of their genera being all that is wanting. The *Lichen* family, under the controul of the great Acharius, assumes the dignity of an entire and well-arranged Order. The *Fungi,* better discriminated by Withering than by most popular writers, and well explained by the figures of the excellent and lamented Sowerby, are, in their minutest details, exquisitely illustrated by the *Cryptogamic Flora* of the ingenious Dr. Greville, and the accurate publications of Mr. Purton. These, marshalled by the aid of the learned Persoon and others, might possibly have proved less obscure than heretofore. This tribe indeed leads the botanist to the end of his clue, and leaves him in palpable darkness, where even Dillenius was bewildered.

"All these subjects, if not yet brought into perfect daylight, might well, by the help of those brilliant northern lights, Acharius, Fries, and Agardh, have been made more accessible to the student, and more instructive to systematic botanists, by one long accustomed to their contemplation in the wild scenes of Nature, and not unfurnished with remarks of his own. If our bodily powers could keep pace with our mental acquirements, the student of half a century would not shrink from the delightful task of being still a teacher; nor does he resign the hope of affording some future assistance to his fellow-labourers, though for the present, "a change of study," to use the expression of a great French writer, may be requisite "by way of relaxation and epose."

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* See Phil. Mag. vol. lxvii. p. 60.

L. *In-*

L. *Intelligence and Miscellaneous Articles.*

ANALYSIS OF ALCOHOL, ÆTHER, ETC.

MESSRS. Dumas and Boullay have recently analysed alcohol, and several æthereal preparations. The alcohol used was repeatedly rectified from chloride of calcium; its sp. gr. was 0·7925 at 64° Fahr.; it boiled at 169° at a medium pressure.

The analysis of alcohol agrees, the authors remark, with that deduced by M. Gay-Lussac, from the density of its vapour. The following are the results of the experiments compared with those of calculation.

	Experiment.	Calculation.
Carbon	52·37	52·28
Hydrogen	13·31	13·02
Oxygen	34·61	34·70
	100·29	100·00

Sulphuric æther was obtained perfectly free from alcohol by rectification from chloride of calcium, until the operation produced no alteration in its properties. Its sp. gr. was 0·713 at 68° Fahr.; it boiled at 93° Fahr. at a medium pressure. The experimental results compared with those of calculation were as follow:

	Experiment.	Calculation.
Carbon	65·05	64·96
Hydrogen	13·85	13·47
Oxygen	21·24	21·57
	100·14	100·00

There is a slight excess of hydrogen, but the authors consider sulphuric æther as consisting of a volume of olefiant gas and half a volume of the vapour of water.

The oil of wine examined was separated from pure æther by distillation: as it is not volatilized at a very low temperature, it remains almost entirely in the retort; a part is then distilled and afterwards rectified from chloride of calcium and a little potash. Its sp. gr. is 0·9174. It was found to consist of

	By Experiment.		By Calculation.
Carbon	88·36	88·80	88·94
Hydrogen	11·64	11·20	11·06
	100·00	100·00	100·00

Oil of wine is therefore a carburet of hydrogen, differing in the proportion of its constituents from all the previously known carburets. The calculated result is obtained by supposing it to be formed of four volumes of the vapour of carbon with three volumes of hydrogen gas. The authors observe that this composition necessarily results from the kind of reaction which gives rise to the formation of oil of wine, as elucidated by other experiments.

Sulphovinic acid was analysed in combination with barytes. The results are thus stated:

Sulphate of barytes....	53·30	54·00
Sulphurous acid	14·65	14·85
Carbon	11·32	10·33
Hydrogen............	1·46	1·39
Water	19·31	20·
	100·04	100·57

The composition of the oily matter, brought to 100, would give

Carbon............	88·37
Hydrogen	11·63
	100·00

It is therefore oil of wine. This being admitted, the sulphovinate of barytes is represented by an atom of hyposulphate, two atoms of oil of wine, and five of water; or

	By Experiment.	By Calculation.
Hyposulphate of barytes....	68·40	67·37
Oil of wine	12·25	12·27
Water	19·65	20·36
	100·30	100·00

The authors then show that similar results are obtained by analysing the sulphovinates of copper and lead.

The authors consider the theory of ætherification as rendered extremely simple by these analyses: the acid and the alcohol are divided into two parts, one of which produces oil of wine and hyposulphuric acid, and occasions the formation of a certain quantity of water; and the other portion of acid and alcohol furnishes by their action weakened acid and æther. It will be seen, according to this process (say the authors), what would be the nature of the action of peroxide of manganese or chromic acid in the formation of æther. They would lose a portion of their oxygen to form water and oil of wine, thus presenting the formation of hyposulphuric acid. M. Gay-Lussac has in fact stated that this acid is not produced in these kinds of reaction. The formation of hyposulphuric acid is not then necessarily connected with that of æther; it is also difficult to believe that the production of oil of wine is necessary to that of æther, the reactions producing them seeming so independent. If it be admitted, as stated by M. Desfosses, that fluoboric acid gives æther without oil of wine, it seems at least that the necessity is not general; and the authors conclude that the two phænomena have nothing in common.—*Annales de Chimie et de Physique*, Nov. 1827.

I shall probably offer some observations upon the paper from which the above is extracted in the next Number of the Annals, more especially with respect to the notice taken of the experiments of Mr. Faraday and Mr. Hennell. In the mean time I cannot help regretting the great use (if that term can be properly applied) which MM. Dumas and Boullay have made of symbols to represent chemical compounds. Can they, or can any body, believe that the following is a very intelligible mode of describing bisulphovinate of lead?

$$\dot{P}b + 4\dddot{S} + 8H^2C^2 + 3\dot{H}H = \dot{P}b + 2\overset{\cdot\cdot}{\dddot{S}}{}^2 + 4H^3C^4 + 5\dot{H}H.$$

R. P.

LIST OF NEW PATENTS.

To W. Nairn, of Dane-street, Edinburgh, for his improved method of propelling vessels through, or on the water by the aid of steam, or other mechanical force.—Dated the 5th of February 1828.—6 months allowed to enrol specification.

To C. Hitch, of Ware, Hertfordshire, for his improved wall for building.—21st of February.—2 months.

To G. Dickinson, of Buckland Mill, near Dover, for his improvements in making paper by machinery.—21st of February.—4 mon.

To Angelo Benedetto Ventura, of Cirencester-place, Fitzroy-square, for his improvements on the harp, lute, and Spanish guitar.—21st of February.—6 months.

To T. Otway, of Walsall, for an expedient for stopping horses when running away.—21st of February.—2 months.

To D. Bentley, of Pendleton, Lancashire, for an improved method and machinery for bleaching and finishing linen or cotton yarn and goods.—21st of February.—6 months.

To W. Brunton, of Leadenhall-street, for improvements on furnaces for the calcination, sublimation, or evaporation of ores, metals, &c.—21st of February.—2 months.

To J. Levers, of Nottingham, for improvements in the manufacture of bobbin net-lace.—3rd of March.—4 months.

To W. Pownall, of Manchester, for improvements in making healds for weaving.—6th of March.—4 months.

To B. H. Brook, of Huddersfield, for improvements in the construction and setting of ovens or retorts for carbonizing coal for gas-works.—6th of March.—6 months.

To Lieut. W. Roger, of Norfolk-street, Strand, for improvements on anchors.—13th of March.—6 months.

To R. G. Jones, of Brewer-street, Golden-square, for a method, communicated from abroad, of ornamenting china and other compositions, which he denominates Lethophanic, translucid, or opaque china.—13th of March.—2 months.

To G. Scholefield, of Leeds, mechanic, for improvements in or additions to looms for weaving woollen, linen, &c.—13th of March.—6 months.

To N. Gough, of Salford, Manchester, for an improved method of propelling carriages or vessels by steam, &c.—20th of March.—6 months.

To S. Clegg, of Chapel Walks, Liverpool, for improvements in steam-engines, and steam-boilers and generators.—20th of March.—6 months.

Results of a Meteorological Journal for the Year 1827, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

By WILLIAM BURNEY, *LL.D.*

Latitude 50° 47′ 20″ North—Longitude 1° 7′ West of Greenwich. In time 4′ 28″.

1827. Months.	Barometer.										Self-registering Thermometer.								Mean Temp. of Spring Water.	De Luc's Hygrometer.						
	Max.	Min.	Media.	Range.	No. of Changes.	Spaces described.	Greatest Variation in 24 hours.	Media at 8 A.M.	Media at 2 P.M.	Media at 8 P.M.	Max.	Min.	Media.	Mean Range.	Gt. Var. in 24 hours	Media at 2 P.M.	Media at 8 A.M.	Media at 8 P.M.		Max.	Min.	Mean Range of the Index.	Media at 2 P.M.	Media at 8 A.M.	Media at 8 P.M.	Media at 8,2,&8 o'cl.
	In.	In.	In.	In.		In.	In.	In.	In.	In.	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
Jan.	30·26	29·29	29·828	0·97	21	7·52	0·76	29·828	29·815	29·832	54	20	38·90	34	18	42·16	36·68	38·68	51·62	100	48	52	67·7	74·1	74·0	71·9
Feb.	30·44	29·49	29·983	0·95	16	5·07	0·48	29·977	29·978	29·988	56	14	36·62	42	21	41·11	33·86	36·39	50·12	98	45	53	58·6	66·3	66·1	63·7
Mar.	30·30	28·79	29·716	1·51	31	11·44	0·94	29·701	29·721	29·726	60	32	47·06	28	24	52·13	45·77	45·32	49·54	96	44	52	60·7	69·6	71·0	67·1
April	30·28	29·44	29·960	0·84	14	4·04	0·33	29·959	29·964	29·959	70	35	51·07	35	22	57·17	50·43	48·50	49·78	88	42	46	55·5	63·6	68·8	62·6
May	30·15	29·09	29·758	1·06	19	5·43	0·57	29·762	29·759	29·760	72	40	56·31	32	21	61·58	56·74	53·13	50·22	95	33	62	54·1	60·1	70·9	61·7
June	30·33	29·64	29·958	0·69	26	4·08	0·32	29·959	29·958	29·956	75	45	60·75	30	24	67·27	60·87	58·30	51·35	82	36	46	51·4	58·0	61·3	56·9
July	30·46	89·76	30·103	0·70	28	4·32	0·40	30·108	30·112	30·099	80	53	65·03	27	25	71·84	65·06	62·97	52·87	94	37	57	50·7	58·8	65·6	58·4
Aug.	30·41	29·38	30·009	1·03	21	4·62	0·36	30·008	30·014	30·009	77	48	62·58	29	23	68·52	61·42	60·74	54·35	84	38	46	49·6	57·4	61·5	56·2
Sept.	30·34	29·50	29·986	0·84	30	3·70	0·46	29·982	29·986	29·990	73	49	60·77	24	24	66·27	59·56	59·53	55·25	100	40	60	54·6	64·6	67·5	62·2
Oct.	30·36	29·07	29·731	1·29	15	7·09	0·58	29·727	29·727	29·735	68	35	55·56	33	21	59·35	54·23	54·26	55·71	100	49	51	66·2	75·8	81·0	74·3
Nov.	30·40	29·28	29·964	1·12	19	6·79	0·73	29·960	29·961	29·969	62	27	48·42	35	19	52·30	46·83	48·43	54·82	100	51	49	70·0	79·2	81·2	76·8
Dec.	30·58	29·07	29·810	1·51	26	8·90	0·56	29·813	29·811	29·810	57	31	47·82	26	17	50·81	45·74	48·00	53·87	100	55	45	78·2	83·0	85·5	82·2
Aver.	30·58	28·79	29·900	12·51	266	73·0	0·94	29·899	29·900	29·903	80	14	52·57	31·25	25	57·54	51·43	51·19	52·46	100	33	51·6	59·8	67·5	71·2	66·2

TABLE (continued.)

1827.	Scale of the Winds.									Modifications of Clouds.							Weather.						Atmospheric Phænomena.									Evaporation in Inches, &c.	Rain in Inches, &c.
	North.	North-East.	East.	South-East.	South.	South-West.	West.	North-West.	Total Number of Days.	Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostratus.	Nimbus.	A clear Sky.	Fair, with Clouds.	An overcast Sky.	Foggy.	Rain, &c.	Total Number of Days.	Anthelia.	Parhelia.	Paraselenæ.	Solar Halos.	Lunar Halos.	Rainbows.	Meteors.	Lightning.	Thunder.		
January ..	5	6½	½	1	1	3½	7½	6	31	18	9	30	...	15	15	19	4	11	11	½	4½	31	...	...	1	...	1	...	3	...	...	0·70	1·000
February	2½	13½	4	1	1	3½	½	2	28	8	6	24	...	14	4	8	4½	11½	10	...	2	28	...	...	...	4	...	...	4	...	...	0 90	0·820
March ...	2	2	...	1	3	10	6	7	31	21	10	31	...	19	26	22	3	12½	10	..	5½	31	...	6	...	9	2	1	2	1	...	1·95	3·145
April	4	5	3	5	4	1	4	4	30	13	9	28	4	17	18	14	4	13	8½	½	4	30	...	2	...	3	3	...	4	...	...	2·35	1·910
May	1	3½	3	2½	3	10	4½	3½	31	20	9	28	...	22	21	18	3	14½	7	½	6	31	...	5	...	4	...	1	3	3	3	3·05	2·125
June .. .	1½	5	½	1½	½	13½	1	6½	30	17	11	25	...	25	23	15	4	14	7	...	5	30	...	2	...	4	...	...	3	1	...	3·00	1·660
July	2½	2	2	2	3	11	6	2½	31	20	16	23	1	28	22	10	6	15	6	½	3½	31	...	...	...	2	1	...	6	1	1	3·70	1·115
August ..	8	7½	1	1	3	4½	2	4	31	21	12	29	...	25	25	13	5½	15½	6	...	4	31	...	...	...	...	...	...	14	1	2	3·85	2·060
September	2	8	1	3	4	6	2	4	30	18	10	28	3	24	26	18	3½	14	6	½	6	30	...	1	...	1	1	4	20	2	1	2·85	3·835
October ..	2	4½	5½	5	4	6	2	2	31	22	12	31	1	22	19	18	3	10½	10	2	5½	31	...	1	...	3	...	10	1	2	2	1·35	4·835
November	6	3½	3	2	4	1	2½	8	30	15	10	27	1	15	19	12	1	9	14	1	4	30	...	...	2	...	3	...	2	1	...	0·70	1·835
December	1½	½	1	1	4	10½	8	4½	31	15	8	28	...	14	12	23	3	9	7½	2	9½	31	...	1	...	...	1	1	4	1	1	0·60	5·625
Results for 1827.	38	61½	24½	26	34½	80½	46	54	365	208	122	332	10	240	230	190	45	149½	103	8	59½	365	...	18	3	30	12	17	66	13	10	25·00	29·965

ANNUAL RESULTS FOR 1827.

Barometer.

	Inches.
Greatest pressure of the atmosphere, Dec. 28th. Wind N.E.	30·580
Least ditto ditto March 4th. Wind S.W.	28·790
Range of the quicksilver	1·790
Annual mean pressure of the atmosphere	29·900
Mean pressure for 190 days with the moon in North decl.	29·872
——— for 164 days with the moon in South decl.	29·927
Annual mean pressure at 8 o'clock A.M.	29·899
——— at 2 o'clock P.M.	29·900
——— at 8 o'clock P.M.	29·903
Greatest range of the quicksilver in March and December	1·510
Least range of ditto in June	0·690
Greatest annual variation in 24 hours in March	0·940
Least of the greatest variations in 24 hours in June	0·320
Aggregate of the spaces described by the rising and falling of the quicksilver	73·000
Number of changes	266·

Self-registering Day and Night Thermometer.

	Degrees.
Greatest thermometrical heat, July 7th and 8th, Wind N.	80
——— cold, February 16th, Wind E.	14
Range of the thermometer between the extremes	66
Annual mean temperature of the external air	52·57
——— of do. at 8 A.M.	51·43
——— of do. at 8 P.M.	51·19
——— of do. at 2 P.M.	57·54
Greatest range in February	42·00
Least of the monthly ranges in September	24·00
Annual mean range	31·25
Greatest monthly variation in 24 hours in July	25·00
Least of the greatest variations in 24 hours in December	17·00
Annual mean temperature of spring water at 8 o'clock A.M.	52·46

De Luc's *Whalebone Hygrometer.*

	Degrees.
Greatest humidity of the atmosphere, several times in November and December	100
Greatest dryness of ditto, May 13th	33
Range of the index between the extremes	67
Annual mean state of the hygrometer at 8 o'clock A.M.	67·5
——— at 8 o'clock P.M.	71·2
——— at 2 o'clock P.M.	59·8
——— at 8, 2, and 8 o'clock	66·2
Greatest mean monthly humidity of the atmosphere in Dec.	82·2
——— dryness of ditto in August	56·2

Position

Position of the Winds.

	Days.
From North to North-east	38
—— North-east to East	61½
—— East to South-east	24½
—— South-east to South	26
—— South to South-west	34½
—— South-west to West	80½
—— West to North-west	46
North-west to North	54
	——365

Clouds, agreeably to the Nomenclature, or the Number of Days on which each Modification has appeared.

	Days.		Days.
Cirrus	208	Cumulus	240
Cirrocumulus	122	Cumulostratus	230
Cirrostratus	332	Nimbus	190
Stratus	10		

General State of the Weather.

	Days.
A transparent atmosphere without clouds	45
Fair, with various modifications of clouds	149½
An overcast sky without rain	103
Foggy	8
Rain, hail, sleet and snow	59½
	——365

Atmospheric Phænomena.

	No.
Parhelia, or mock-suns on the sides of the true sun	18
Paraselenæ, or mock-moons	3
Solar halos	30
Lunar halos	12
Rainbows	17
Meteors of various sizes	66
Lightning, days on which it happened	13
Thunder, ditto ditto	10

Evaporation.

	Inches.
Greatest monthly quantity in August	3·85
Least monthly quantity in December	0·60
Total amount for the year	25·00

Rain.

Greatest monthly depth in December	5·625
Least monthly depth in February	0·820
Total amount near the ground for the year	29·965
Total amount near 23 feet high for ditto	27·635

The instruments are the same, and were placed in the same situations as described in the February Number of this work, in the Results of the Meteorological Journal for Hampshire, for 1826.

BAROMETRICAL PRESSURE.—The mean pressure of the atmosphere

sphere on the mercurial column this year, is $\frac{81}{1000}$ of an inch lower than that of last year, and $\frac{3}{250}$ of an inch higher than the mean of the last twelve years. The yearly maximum pressure is not high, nor the minimum comparatively low.

The aggregate of the spaces described by the alternate rising and falling of the quicksilver, is 14·59 inches greater than in 1826.

The number of changes in the Barometer this year, viz. 266, coincides with that of 1826 and 1816, and is only four short of the annual average for the last twelve years. Although the aggregate of the spaces differs very much in different years, yet the yearly number of changes is pretty uniform; and the average time of each change during the last twelve years, is 32½ hours nearly. The annual uniformity of these changes, as influenced by the varying weight of the atmosphere on the column of quicksilver, might have first suggested to meteorologists the idea of the existence of atmospherical tides, as chiefly effected by the moon's attractive force on the spheroidal mass of the earth's atmosphere. But these changes not happening regularly, compared with the regularity of the tides of the ocean in connection with the moon's motion, as sometimes they are quick and at other times very slow, at least near the earth's surface, a train of imperceptible effects, besides the moon's supposed attraction, should therefore be taken into consideration to account fairly for the atmospherical tides, as the diurnal action of the sun's calorific rays upon the atmosphere; the ascending heat from the earth in the spring and summer months; the rarefaction of the atmosphere, particularly under the sun's vertical rays and about the equator; the consequent perturbations of the air, and dispersion of dry and moist winds, according to their position, to every known place on the earth; and the influence of the seasons upon the constitution of the atmosphere in every climate without the tropics of Cancer and Capricorn.

TEMPERATURE.—In the temperature of the atmosphere we generally feel more interest than in the pressure, from the effects it often produces in us when too much exposed to the extremes of either cold or heat. The heat we sustain beyond the temperature of the ground, is the effect of the solar rays on the earth and surrounding atmosphere, while the earth moves in that part of its orbit which coincides with the northern signs of the ecliptic; for while it moves through the space occupied by the southern signs, the mean temperature of the atmosphere in this latitude is almost invariably below the mean temperature of the ground, and especially during the time it moves through *Capricornus*, *Aquarius*, and *Pisces*. However, in respect to the temperature connected with the seasons, it is sometimes reversed for three or four weeks in the spring or autumn. A deviation of this sort occurred last February, the mean temperature of that month being 2¼ degrees below the mean of January. Two other deviations also occurred in 1826, as explained in our Meteorological Summary for Hampshire that year.

The mean temperature of the external air this year is about a degree below that of last; but it is nearly three quarters of a degree higher than the mean of the preceding twelve years.

The mean temperature of spring water at 8 A.M. is only $\frac{1}{100}$ of a degree less than the mean temperature of the external air, which seems to prove that the additional thermometrical heat near the ground in the summer, is balanced by the decrease of temperature in the opposite season, when the sun's diurnal arc with us is much shorter, and his rays more oblique.

The difference between the annual mean temperature of the air as taken daily at 8 A.M. and 8 P.M. is only ·24, or a quarter of a degree.

WIND.—The scale of the prevailing winds this year deviates very little from that of last. High winds prevailed in January, February, March, April, October, and December; but in the other months the winds were comparatively light. The number of strong gales, or the days on which they have prevailed this year, is as in the following scale:

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Gales.
10	11	3	3	8	39	8	7	89

Here the number from the S.W. is remarkable, as usual. The weather in January and February was cold; and in March, September, October, and December it was wet, but rather dry in the other eight months.

The amount of evaporation this year is considerably under the depth of rain, from the atmosphere near the ground having been more than usually humid in the summer months.

METEOROLOGICAL OBSERVATIONS FOR FEBRUARY 1828.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·44 Feb. 3. Wind N.W.—Min. 28·90 Feb. 21. Wind S.E.
Range of the index 1·54.
Mean barometrical pressure for the month .... 29·763
Spaces described by the rising and falling of the mercury............ 6·250
Greatest variation in 24 hours 0·570.—Number of changes 17.
Therm. Max. 61° Feb. 26. Wind SW.—Min. 28° Feb. 12. Wind N.
Range 33°.—Mean temp. of exter. air 45°·74. For 30 days with ⊙ in ♒ 45·32
Max. var. in 24 hours 17°·00—Mean temp. of spring water at 8 A.M. 51°·56

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the afternoon of the 25th............ 100°
Greatest dryness of the air in the afternoons of the 3rd & 15th ... 59
Range of the index.. 41
Mean at 2 P.M. 71°·6—Mean at 8 A.M. 82°·1—Mean at 8 P.M. 81·8
—— of three observations each day at 8, 2, and 8 o'clock......... 78·5
Evaporation for the month 0·75 inch.
Rain near ground 1·515 inch.—Rain 23 feet high 1·395 inch.
Prevailing Wind S.E.

Sum-

Summary of the Weather.

A clear sky, 2; fine, with various modifications of clouds, 9½; an overcast sky without rain, 12½; foggy ½; rain, 4½.—Total 29 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus
14	8	29	1	10	17	20

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
5	2	1	7	2	6	4	2	29

General Observations.—This month has been calm and generally mild, and although it has rained more or less on seventeen different days, yet the amount at the ground is very little more than one and a half inch in depth; therefore it has generally been light.

From the 10th to the 18th instant, the weather was rather cold; and on the 11th and 12th, sleet and granulous snow fell. In the morning of the 14th it snowed three hours, and the depth was about 2½ inches, which dissolved in this neighbourhood in two hours with a temperature from 37 to 41 degrees in the shade. In the evening of that day about 20 minutes before 7 o'clock, a very large meteor, in the form of a cone on its side, passed over towards the S.E. It appeared very low, perhaps not above 200 feet from the ground: its light was glaring for five or six seconds of time, and nearly the colour of the moon's reflected light, and its train sparkling. It passed through an apparent space of about 60 degrees before it disappeared behind some houses, and was seen by many persons in the town and vicinity.

In the evening of the 15th, a short time before Venus set, a mild light reflected in the atmosphere from that planet eastward, was distinctly traced to the first star of *Aries*, marked γ, a distance at that time of 34 degrees.

The *maximum* temperature of the external air occurred in the nights of the 3rd, 13th, and 24th. The *maximum* temperature of the present month is remarkable, it being four degrees higher than in any February for the last twelve years. It occurred on the 26th, and appeared to have been influenced after light rain, by two warm winds crossing at right angles from the S.E. and S.W., and a dense body of *cumulostratus* clouds with some attenuated parts, sufficient to admit the calorific rays to the earth's surface, which acted powerfully on the exterior thermometer. The mean temperature of the month is very high for the season. Much has been said in conversation on the mildness of the last winter, viz. December, January, and February, which in this latitude are understood and very generally taken as the three winter months; and it certainly has been the mildest during the last thirteen years. The mean temperature of these months for the last thirteen years, is 41·38 degrees; and for the last three months 46·16 degrees; therefore, the mean of the last winter is 4¾ degrees higher than the mean of that period. The next mildest winter occurred in the years 1821–2, the mean temperature of which was 45·56 degrees, that is *three-fifths* of a degree lower than the mean of the last winter. The spring has commenced, and the trees are progressively breaking into buds.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are one lunar and two solar halos, two meteors, one rainbow, and two gales of wind, one from the South-east, the other from South-west.

REMARKS.

REMARKS.

London.—Feb. 1. Fine. 2. Very fine. 3. Fine. 4. Slight fog: cloudy. 5. Cloudy. 6. Slight rain: cloudy. 7. Cloudy: rain at night. 8. Fine. 9. Hazy, with showers. 10. Clear and cold. 11. Snowy: cloudy. 12. Cloudy. 13. Fine. 14. Cloudy, with sleet. 15. Fine. 16, 17. Very fine. 18. Cloudy, with showers. 19—21. Fine. 22. Cloudy. 23. Showery. 24, 25. Cloudy. 26. Rain in morning: cloudy. 27. Fine. 28. Slight fog: fine. 29. Slight fog: cloudy.

Boston.—Feb. 1, 2. Fine. 3. Fine: rain A.M. 4. Cloudy: rain A.M. and .M. 5. Cloudy. 6. Cloudy: rain early A.M. 7. Dense fog: rain early A.M. . Fine: rain at night. 9. Cloudy: rain at night. 10, 11. Cloudy. 12. Snow. 13. Cloudy. 14. Stormy: heavy fall of snow. 15—17. Cloudy. 18. Fine: rain P.M. 19. Cloudy. 20. Fine: rain P.M. 21, 22. Fine. 23. Cloudy: rain A.M. 24. Cloudy. 25. Cloudy: rain A.M. 26, 27. Cloudy. 28. Foggy. 29. Cloudy.

Penzance.—Feb. 1. Clear: rain at night. 2. Misty: clear. 3. Clear. 4. Rain. 5. Misty: rain. 6. Fair: misty. 7. Rain: clear. 8. Misty: clear. 9. Clear. 10. Fair: rain. 11. Hail-showers. 12. Fair: rain. 13. Clear: rain at night. 14. Fair: hail and rain. 15. Clear. 16. Clear: fair. 17. Fair: rain. 18. Showers. 19. Fair: showers. 20, 21, Fair: rain. 22. Fair: showers. 23. Clear: hail-showers. 24, 25. Rain. 26. Misty: fair. 27. Fair. 28. Clear. 29. Clear: fair.—Rain-gauge ground level.

We give the following to supply the deficiencies in our last Number in the Meteorological Register for January.

London.				
	Barometer.		Thermometer.	
	9 A.M.	10 P.M.	Max.	Min.
Jan. 20	30·13	30·18	53	43
21	30·13	30·11	50	45
22	30·00	30·13	46	40
23	30·18	30·24	52	44
24	30·23	30.23	52	48
25	30·20	30·01	54	42
26	30·15	30·23	51	47
27	30·24	30·31	53	37
28	30·31	30·25	47	42
29	30·08	30·06	50	40
30	29·98	29·90	47	40
31	29·90	29·86	49	43

We are enabled in the present Number to supply the place of Mr. Howard's observations, by extracts from the accurate Register kept by Mr. W. B. Booth, at the Garden of the Horticultural Society at Chiswick, near London; with which we trust we shall be favoured monthly.

Meteorological Observations made at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Giddy at Penzance, Dr. Burney at Gosport, and Mr. Veall at Boston.

Days of Month, 1828.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston 8½ A.M.	London.		Penzance.		Gosport.		Bost. 8½ A.M.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.		Max.	Min.	Max.	Min.	Max.	Min.										
○ Feb. 1	30·055	29·980	29·96	29·94	30·06	29·92	29 43	51	32	55	47	54	48	46	sw.	w.	w.	calm	...	...	...	...	...
2	30·188	30·087	30·20	30·10	30·12	30·06	29·65	49	35	50	47	52	37	40	w.	w.	sw.	calm	...	...	0·200	0·040	...
3	30·521	30·232	30·40	30·30	30·44	30·38	29·97	48	37	54	40	47	39	36·5	w.	sw.	nw.	calm	0·10	...	0·030	·055	0·03
4	30·297	30·268	30·26	30·24	30·25	30·24	29·77	53	46	54	44	56	48	41	w.	sw.	w.	calm	...	...	0·300	...	·07
5	30·214	30·190	30·16	30·10	30·18	30·13	29·63	52	45	53	48	55	48	48·5	sw.	sw.	w.	w.	...	...	0·180	·010	·03
6	30·143	30·017	30·00	29·96	30·03	29·98	29·50	54	40	53	47	58	47	47	sw.	sw.	sw.	w.	·05	·04	...	·010	·03
7	29·979	29·898	29·88	29·88	29·90	29·84	29·47	51	40	50	48	55	42	48	w.	w.	se.	calm	...	·28	0·180	·100	·04
☾ 8	29·935	29·721	29·90	29·80	29·84	29·72	29·50	50	33	50	45	49	38	37	nw.	w.	n.	nw.	...	...	...	...	...
9	29·738	29·726	29·78	29·70	29·69	29·58	29·36	41	34	47	36	45	39	39·5	nw.	ne.	n.	calm	·05	·06	...	...	·11
10	29·857	29·794	29·80	29·68	29·68	29·61	29·55	38	30	47	36	44	35	33	n.	se.	ne.	calm	...	...	0·400	·480	·05
11	29·841	29·542	29·72	29·62	29·69	29·36	29·27	34	28	46	38	40	31	32·5	ne.	nw.	n.	e.	...	·14	0·200	...	...
12	30·108	29·933	30·00	29·92	29·96	29·90	29·73	33	27	41	36	36	28	30·5	ne.	e.	n.	n.	...	...	0·060	...	...
13	30·185	30·125	30·00	29·96	30·02	29·96	29·86	34	29	48	29	40	37	32	ne.	se.	ne.	calm	·15	...	...	...	...
14	29·800	29·538	29·66	29·44	29·60	29·45	29·53	34	29	46	40	44	34	32·5	e.	nw.	s.	w.	...	·08	0·645	·360	...
● 15	29·861	29·657	29·76	29·76	29·80	29·65	29·35	42	23	48	36	45	32	36	nw.	w.	nw.	calm	...	...	0·340	...	...
16	29·932	29·875	29·76	29·66	29·81	29·78	29·56	44	25	48	35	48	40	32	w.	se.	se.	nw.	...	...	...	...	...
17	29·782	29·618	29·50	29 26	29·64	29·50	29 45	42	27	46	42	46	39	34	se.	sf.	se.	calm	·10	...	0·080	·015	...
18	29·433	29·400	29·26	29·26	29·36	29·26	29·20	46	31	48	36	50	38	32	s.	w.	s.	calm	...	·18	0·025	·020	...
19	29·414	29·362	29·26	29·20	29·34	29·29	28·96	52	32	49	37	52	41	35·5	se.	sw.	sw.	nw.	...	...	0·025	·020	·46
20	29·372	29·228	29·14	28·80	29·26	29·09	28·82	55	40	50	38	51	44	41·5	se.	s.	se.	calm	·10	·02	0·265	·090	...
21	29·115	29·048	28·76	28·74	28·96	28·90	28·82	52	38	50	45	52	43	41	e.	s.	se.	calm	...	·03	0·455	·150	·14
☽ 22	29·175	29·070	28·90	28·86	29·03	28·90	28·75	51	40	48	41	53	43	42·5	e.	nw.	se.	e.	...	...	0·260	·015	...
23	29·561	29·371	29·40	29·21	29·50	29·24	29·05	42	38	48	42	49	38	39·5	n.	ne.	n.	calm	·05	·04	0·050	·020	·22
24	29·858	29·838	29·70	29·70	29·81	29·73	29·40	49	45	52	36	53	48	38·5	nw.	sw.	w.	calm	...	·02	0·070	·030	...
25	29·982	29·883	29·76	29·70	29·89	29·83	29·33	53	48	54	45	54	51	50	s.	sw.	sw.	nw.	...	...	0·120	·085	·04
26	30·073	30·009	29·88	29·86	29·98	29·98	29·54	58	46	54	49	61	49	51	se.	sw.	sw.	calm	·05	·05	0·125	·015	·16
27	30·260	30·161	30·00	29·90	30·14	30·02	29·65	60	40	55	48	59	49	46	s.	nw.	se.	calm	...	...	...	...	...
28	30·352	30·330	30·20	30·20	30·30	30·24	29·82	56	38	52	44	57	41	43·5	nw.	n.	n.	calm	...	...	...	...	...
29	30·343	30·309	30·30	30·30	30·27	30·20	29·80	56	41	51	43	57	44	50	nw.	ne.	n.	nw.	·10	...	...	...	...
Aver. :	29·909	29·800	30·40	28·74	30·44	28·90	29·43	47·5	35·7	55	29	61	28	39·9					0·75	0·94	4·010	1·515	1·38

Fig. 1.

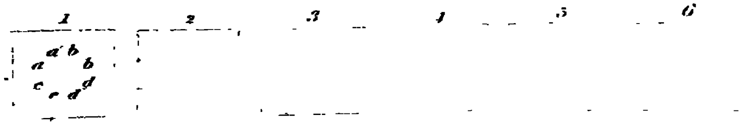
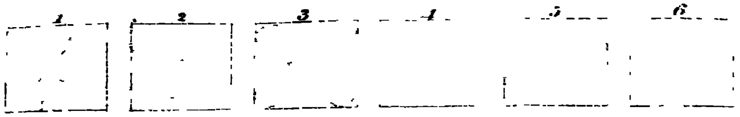


Fig. 2.



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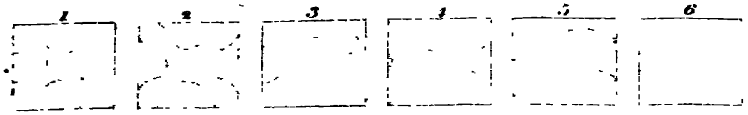

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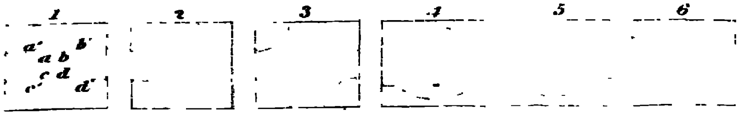

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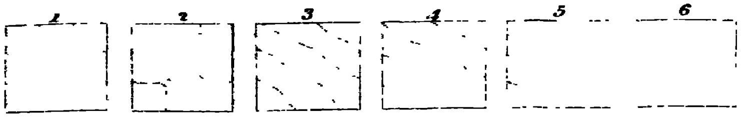

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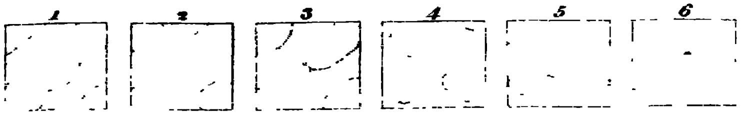

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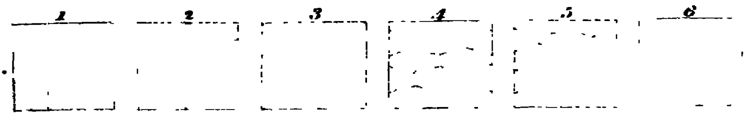

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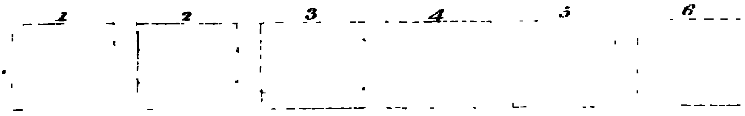


Fig. 9.

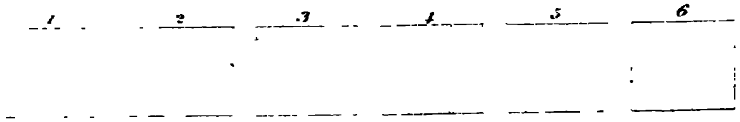


Fig. 10.

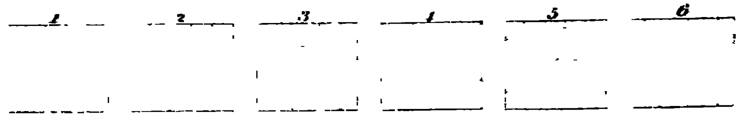
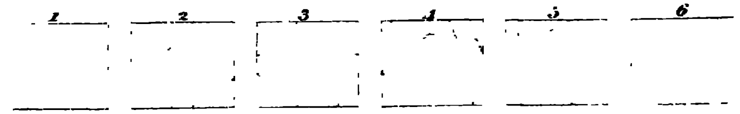


Fig. 11.



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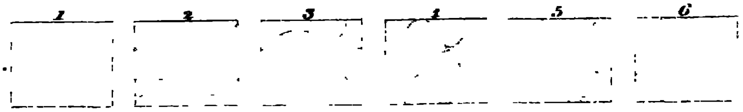

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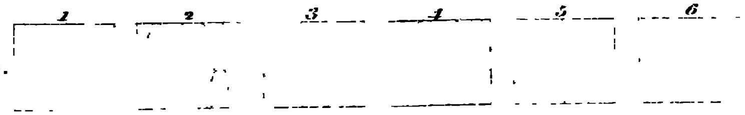


Fig. 14.

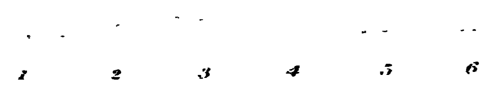


Fig. 15.

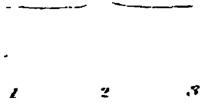


Fig 16.

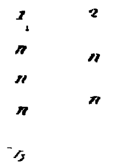
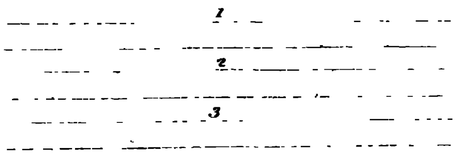


Fig. 17



THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

MAY 1828.

LI. *On the Ellipticity of the Earth, as deduced from Experiments with the Pendulum; and on the Formulæ employed for obtaining it. By* WILLIAM GALBRAITH, *Esq. A.M.**

THE number of accurate experiments made with the pendulum to determine the figure of the earth, is now considerable, and the discussion of them has been somewhat extensive. From these it appears that discrepancies in the length of the pendulum by experiment in a given latitude may be considerable, amounting to above 0·005 of an inch, arising from the unequal action of the strata immediately under the station, for which no allowance can be easily made. From these considerations, it is evident combinations of pendulum experiments may be employed to bring out any required ellipticity, at least within certain limits, unless a judicious selection be made of those placed under analogous circumstances. To obviate these, the mean of a number of experiments in the same latitude, but in very different longitudes, distributed as regularly as conveniently can be, over the same parallel, should be chosen, comprehending, if possible, every variety of geological basis. From this method of proceeding, local irregularities would be so far counteracted as to bring out results somewhat satisfactory; and until this is accomplished, perhaps other artifices may be advantageously employed to obviate as far as possible these unavoidable and troublesome incongruities. Among other considerations, perhaps a basis of a certain specific gravity might be chosen as a standard; and by comparing the lengths of the pendulum on various bases of different specific gravities, an approximate rule might perhaps be obtained by which, with considerable exactness, the lengths of the pendulums on different bases might be reduced to the same. Taking ordinary alluvial formations for the standard density, of which the specific gravity may be about 2·5,—and by experiments made on subsoils of light sand, gravel, or clay, combined with those made on sandstone, basalt, schistus and granite, in the same latitude, and thence finding what effect these have in varying the length of the pendulum,—an approximate value of that effect may be pretty well estimated; whence a rule may be obtained, to reduce them all to the standard density, so far as relates to the exterior crust of the earth. These, combined with the form of the substratum and other peculiarities, of which several instances may be seen in Captain Sabine's work on the Figure of the Earth, would give the means of obtaining pretty accurately the medium length of the pendulum in any given latitude. Thus it may be inferred, that a variation of the specific gravity from 2 to 3 at the surface, and continued for some distance towards the centre, would give a variation of the length of the pendulum amounting to about 0·01 of an inch. Consequently, if 2·5 were taken for the standard specific gravity at the surface, and the lengths reduced to this, they may be increased or diminished about 0·005 of an inch, —a quantity sufficient to reconcile the most discordant of the pendulum experiments. These reductions, no doubt, should be applied to those experiments only which are likely to be employed to determine the figure of the earth, in order to obviate the discordancies from local irregularities; while the actual lengths from observation applied to other purposes,—such as our standards of weights and measures obtained at a particular spot,—ought to be allowed to remain as they are. Indeed, in an extensive series of experiments, the several corrections for the height, shape, specific gravity, &c. of the station, should be all placed in their respective columns beside the original lengths, to be employed or omitted, as might appear advisable, according to the purposes for which they are wanted. No doubt, with regard to these corrections, a good deal is left to the accuracy and judgement of the observer; and in some of them great precision cannot be attained; but in most cases a probable approximation only, which, although not perfectly accurate, will at least tend to give the mean ellipticity more truly than when they are neglected.—Having stated these preliminaries, our attention may now be directed to the formulæ for obtaining the ellipticity.

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* Communicated by the Author.

It appears from our ordinary treatises on the Figure of the Earth, supposing it to differ little from a sphere, that the centrifugal force which acts in opposition to gravity, decreases from the equator to the pole as the square of the cosine of the latitude;

titude; and consequently the diminution of gravity from this cause, in proceeding from the pole to the equator, will also be as the square of the cosine of the latitude. But the square of the sine increases as the square of the cosine diminishes; therefore the increase of gravity in proceeding from the equator to the pole, will be as the square of the sine of the observed latitude nearly. A little consideration will readily show, however, that this is correct, on the supposition of the earth being a perfect sphere, and is only an approximation not far from the truth in the case where the compression is small. The centrifugal force, therefore, is strictly proportional to the ordinate to the polar axis which involves the compression,—the very thing we are in quest of. Now as this is supposed to be the unknown quantity, it must be assumed equal to about $\frac{1}{300}$, what it is already known to be nearly; we may infer that the centrifugal force decreases as the square of the sine of the *reduced latitude* exactly. The difference arising from these two suppositions is undoubtedly small; but in very nice disquisitions, minute quantities ought not to be entirely disregarded.

The length of the pendulum is as the force of gravity; and it has generally been inferred, from suppositions not far from the truth, that the length of the pendulum from the equator to the pole, increases as the square of the sine of the latitude. The latitude hitherto used is that derived directly from observation, supposing the plumb-line to hang perpendicularly to the surface, or to the tangent plane to that surface, at the place of observation. Now there is no doubt that, from the equilibrium of the fluid part of the earth, the *direction* of gravity at the surface is exactly, or very nearly, in this vertical line. But the *force* of gravity at any point on the surface of a spheroid is (Schubert, *Astronomie Physique*, vol. iii. § 125.) inversely proportional to the radius of the spheroid, or the line drawn from the place of observation on the surface to the *centre*. This is conformable to experience; for it is known that the length of the pendulum, independently of centrifugal force, increases as we proceed from the equator to the pole where it is nearest the centre; and consequently, on that very account, its length there is greatest. I am well aware that the length of the pendulum has, so far as I know, always been affirmed to increase from the equator to the pole, nearly as the square of the sine of the observed latitude; though from what I have already advanced, it appears that it increases as the square of the sine of the *reduced latitude*, the compression being assumed by estimation, as has already been observed.

"For in an oblate spheroid (Playfair's Outlines of Natural Philosophy, vol. ii. § 296.) differing little from a sphere, if

b be the polar semiaxis, $b+c$ the radius of the equator, ϕ *the angle which a line drawn from the centre to a given point on the surface makes with the axis of the spheroid*, and f the force with which the given point is attracted by the spheroid; then it has been shown by M. Chastellot, (in the French translation of Newton's *Principia*, tom. ii. Paris edition of 1756. § xxxvii. coroll. I. page 236, 237.) that

$$f=\frac{4\pi b}{3}\left(1+\frac{4c}{5b}-\frac{c}{5b}\sin^2\varphi\right) \;\ldots\ldots\ldots\ldots\; (1)$$

But if λ be the latitude, and θ the angle of the vertical with the radius, commonly called the reduction of the latitude, and therefore $\phi=\lambda-\theta$, or the reduced latitude, then

$$f=\frac{4\pi a}{3}\left(1+\frac{4c}{5a}+\frac{c}{5a}\sin^2(\lambda-\theta)\right), \text{or if } a=1,\ \frac{c}{a}=\varepsilon,$$

there will result

$$f=\frac{4\pi}{3}\left(1+\frac{4}{5}\varepsilon+\frac{1}{5}\varepsilon\sin^2(\lambda-\theta)\right) \;\ldots\ldots\ldots\; (2)\text{”}$$

This expression for the attraction, shows that the gravitating force in proceeding from the equator to the pole, increases as the square of the sine of the *reduced latitude.* From similar principles, and the considerations formerly advanced, it follows that the increase of the force of gravity depending upon the diminution of the centrifugal force, also increases as the square of the sine of the *reduced latitude.* On combining these, the absolute increase of the length of the pendulum from the equator to the pole, is proportional to the square of the sine of the reduced latitude, not of the observed latitude. I advance this conclusion with diffidence, and with all due deference to those who have preceded me, and am fully aware of the authority of such names as those of Clairaut and Laplace: but I think my conclusion is founded in truth; and if it be so, it cannot be shaken by the authority of any name, however great. No doubt the difference between the square of the sine of the observed and reduced latitude is not great; since at its maximum at 45°, supposing the ellipticity to be about $\frac{1}{300}$, making the reduction 11′ 29″ only, the square of the sine of the observed latitude, or 45°, is 0·5, and the square of the sine of the reduced latitude, or 44° 48′ 31″, is 0·4966596. Now if the excess of the polar above the equatorial pendulum amount to 0·208 of an inch, then 0·208 (0·5 − 0·4966596) = 0·000695 of an inch, the quantity that the computed pendulum at the observed latitude of 45° would be too great: and hence, when the lengths of the experimental and computed pendulums are compared in the usual hypothesis, a disagreement at this parallel will always occur, which may very easily be imputed to a wrong cause;

such as an error in the experimental results, a protuberance of the earth at that latitude, a diminution of density, &c. This has occurred to me from some of my own computations, as well as from those of others which I have seen, but which seem to disappear when the proper (the reduced) latitude is employed. In order to obtain correct results, the formulæ should possess all the accuracy which can be given them. For this purpose I have elsewhere reconsidered the usual method of obtaining the compression, and made some slight correction.

If ϕ denote the centrifugal force at the equator, r the radius of the equator, t the time of rotation of the earth about its axis, and l the length of the pendulum, then $\varphi = \quad \left(\frac{t}{2}\right)^2 l$. (1)

and consequently the ratio of the centrifugal force to gravity will be expressed by $1+\varphi \qquad r+\left(\frac{t}{2}\right)^2 l = q$ (2)

If the most accurate values of these quantities be introduced, q will be found to be $0{\cdot}003455 = \frac{1}{289{\cdot}436}$, somewhat less than $\frac{1}{289}$, that usually employed. Now if ε denote the ellipticity, p a certain coefficient to be found from an investigation of the conditions of equilibrium of a spheroid, y the excess of the polar above the equatorial pendulum, and z the length of the equatorial pendulum, then

$$\varepsilon = \frac{pr}{r+\left(\frac{t}{2}\right)^2 z} - \frac{y}{z} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

Since by considerations derived from the case of a homogeneous spheroid, which may be supposed very nearly applicable to the case of the earth, the value of p is 2·491516, instead of 2·5, that commonly adopted; whence it follows that

$$\varepsilon = 0{\cdot}0086082 - \frac{y}{z} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (4)$$

This formula will, I believe, give the ellipticity as nearly correct as the necessary data required in the substitutions can be depended on.

As we have not as yet a sufficient variety of experiments in different longitudes under the same parallel, affected by the various irregularities tending to destroy each other, neither have we sufficient data to reduce them to a substratum of a given density, as formerly proposed; it remains to employ the only method left, which may perhaps effect nearly the same thing. This consists in reducing as many lengths of the pendulum as possible to the same point which are very near it, by the usual formula, which, from their proximity, cannot introduce any considerable error, as none are selected for this purpose whose latitude differs more than about 5° on each side of it, from that to which they are reduced. As an equal number was generally chosen on each side of it too, any small error in the formula of reduction which was used, or $\Delta l = \pm 0{\cdot}208 (\sin^2 \lambda'' - \sin^2 \lambda')$, will be by that means counteracted.

No doubt objections may be urged against this mode of procedure; but it is perhaps the best under the present circumstances of the problem which can be employed, as the most accurate method of distributing the observations regularly over the quadrantal arc of the meridian. Now a series of observations distributed regularly over the meridian is of considerable importance to determine the exact ellipticity, even when the method of the least squares is employed; for in this case also a preponderance of observations at any latitude has its effect in obtaining the compression.—Having now assigned the reasons for adopting the methods we have chosen, it remains to apply them to a collection of all the experiments which could with confidence be employed. These are principally the series of Captain Kater, M. Biot, Captain Sabine, M. Freycinet, M. Duperrey, together with a few more from Sir Thomas Brisbane, Captain Hall, and perhaps one or two others.

Places.	Latitude.	Experimental Pendulum.	Reduced Pendulum to Latitude.	Length.
Rawak	0° 1′ 34″ S.	39·01482		
Sumatra..........	0 1 49 N.	39·01812		
St. Thomas......	0 24 41 N.	39·02074	0° 0′ 0″	39·01650
Galapagos	0 32 19 N.	39·01717		
Maranham......	2 31 43 S.	39·01214		
Ascension (D.)	7 55 9 S.	39·02364		
Ascension (S.)	7 55 48 S.	39·02410	5 0 0	39·01877
Sierra Leone ...	8 29 28 N.	39·01624		
Trinidad........	10 38 56 N.	39·01884		
Bahia............	12 59 21 S.	39·02223	10 0 0	39·02090
Madras	13 4 9 N.	39·02338		
Guam...........	13 27 51 N.	39·03023	15 0 0	39·02913
Jamaica	17 56 7 N.	39·03144		
Isle of France..	20 9 56 S.	39·04769	20 0 0	39·03856
Isle of Mowi ...	20 52 7 N.	39·04737		
San Blas	21 32 24 N.	39·03828	25 0 0	39·05167
RioJaneiro(H.)	22 55 22 S.	39·04374		

Places.	Latitude.	Experimental Pendulum.	Reduced Pendulum to Latitude.	Length.
Rio Janeiro (F.)	22° 55′ 13″ S.	39·04371	30° 0′ 0″	39·06492
P. Jackson (B.)	33 51 34 S.	39·07724		
P. Jackson (D.)	33 51 39 S.	39·07897	35 0 0	39·08166
Formentera....	38 39 56 N.	39·09424		
New York......	40 42 43 N.	39·10168	40 0 0	39·09908
Toulon	43 7 9 N.	39·11038		
Figeac...........	44 36 45 N.	39·11322		
Bordeaux.......	44 50 26 N.	39·11303	45 0 0	39·11585
Clermont.......	45 46 48 N.	39·11812		
Paris	48 50 14 N.	39·12929		
Shanklin........	50 37 24 N.	39·13614	50 0 0	39·13329
Dunkirk.........	51 2 10 N.	39·13773		
London	51 31 8 N.	39·13929		
Falkland I. (D.)	51 31 44 S.	39·13966		
Falkland I. (F.)	51 35 18 S.	39·13720		
Arbury Hill ...	52 12 55 N.	39·14250	55 0 0	39·15163
Clifton..........	53 27 43 N.	39·14600		
Leith (Biot.)	55 58 37 N.	39·15547		
Leith (Kater.)	55 58 41 N.	39·15554		
Portsoy (Kater.)	57 40 59 N.	39·16159	60 0 0	39·16772
Stockholm......	59 20 34 N.	39·16541		
Unst (Biot.)	60 45 25 N.	39·17181		
Unst (Kater.)	60 45 28 N.	39·17146	65 0 0	39·18213
Drontheim (S.)	63 25 54 N.	39·17456		
Hare Isle.......	70 26 17 N.	39·19840		
Hammerfest ...	70 40 5 N.	39·19519	70 0 0	39·19553
Greenland......	74 32 19 N.	39·20335		
Melville Isle ...	74 47 12 N.	39·20700	75 0 0	39·20600
Spitzbergen....	79 49 58 N.	39·21469		

λ	$\lambda-\theta$.	Pendulum.
0°	0° 0′ 0″	$39{\cdot}01650-z-0{\cdot}0000000\,y=\mathrm{E}$
5	4 58 1	$39{\cdot}01877-z-0{\cdot}0074963\,y=\mathrm{E}_1$
10	9 56 5	$39{\cdot}02090-z-0{\cdot}0297652\,y=\mathrm{E}_2$
15	14 54 17	$39{\cdot}02913-z-0{\cdot}0661582\,y=\mathrm{E}_3$
20	19 52 38	$39{\cdot}03856-z-0{\cdot}1156039\,y=\mathrm{E}_4$
25	24 51 14	$39{\cdot}05167-z-0{\cdot}1766568\,y=\mathrm{E}_5$
30	29 50 5	$39{\cdot}06492-z-0{\cdot}2475060\,y=\mathrm{E}_6$
35	34 49 14	$39{\cdot}08166-z-0{\cdot}3260503\,y=\mathrm{E}_7$
40	39 48 42	$39{\cdot}09908-z-0{\cdot}4099407\,y=\mathrm{E}_8$
45	44 48 31	$39{\cdot}11585-z-0{\cdot}4966596\,y=\mathrm{E}_9$

λ	$\lambda-\theta$.	Pendulum.
50°	49° 48′ 41″	$39{\cdot}13329-z-0{\cdot}5835803\,y=E_{10}$
55	54 49 12	$39{\cdot}15163-z-0{\cdot}6680546\,y=E_{11}$
60	59 50 3	$39{\cdot}16772-z-0{\cdot}7474900\,y=E_{12}$
65	64 51 11	$39{\cdot}18213-z-0{\cdot}8194250\,y=E_{13}$
70	69 52 36	$39{\cdot}19553-z-0{\cdot}8816350\,y=E_{14}$
75	74 54 15	$39{\cdot}20600-z-0{\cdot}9321740\,y=E_{15}$

Whence $z=39{\cdot}098334-0{\cdot}4067622\,y$ .... (A)

Again,

$$\begin{aligned}
0{\cdot}0000000-0{\cdot}0000000\,z-0{\cdot}0000000\,y&=E\\
0{\cdot}2924964-0{\cdot}0074963\,z-0{\cdot}0000562\,y&=E_1\\
1{\cdot}1614649-0{\cdot}0297652\,z-0{\cdot}0008860\,y&=E_2\\
2{\cdot}5820970-0{\cdot}0661582\,z-0{\cdot}0043769\,y&=E_3\\
4{\cdot}5130098-0{\cdot}1156039\,z-0{\cdot}0133643\,y&=E_4\\
6{\cdot}8987431-0{\cdot}1766568\,z-0{\cdot}0302076\,y&=E_5\\
9{\cdot}6688209-0{\cdot}2475060\,z-0{\cdot}0612592\,y&=E_6\\
12{\cdot}7425870-0{\cdot}3260503\,z-0{\cdot}1063088\,y&=E_7\\
16{\cdot}0283042-0{\cdot}4099407\,z-0{\cdot}1680514\,y&=E_8\\
19{\cdot}4272624-0{\cdot}4966596\,z-0{\cdot}2466707\,y&=E_9\\
22{\cdot}8374171-0{\cdot}5835803\,z-0{\cdot}3405660\,y&=E_{10}\\
26{\cdot}1554265-0{\cdot}6680546\,z-0{\cdot}4462970\,y&=E_{11}\\
29{\cdot}2774790-0{\cdot}7474900\,z-0{\cdot}5587402\,y&=E_{12}\\
32{\cdot}1068169-0{\cdot}8194250\,z-0{\cdot}6714573\,y&=E_{13}\\
34{\cdot}5561511-0{\cdot}8816350\,z-0{\cdot}7772800\,y&=E_{14}\\
36{\cdot}5468138-0{\cdot}9321740\,z-0{\cdot}8689486\,y&=E_{15}
\end{aligned}$$

Whence $z=39{\cdot}1498536-0{\cdot}6598551\,y$ .... (B)

Equating (A) and (B) there will result

$$y=0{\cdot}20348 \quad\text{and}\quad z=39{\cdot}015576$$

therefore $\frac{y}{z}=0{\cdot}0052153$, and

$$E=0{\cdot}0086082-0{\cdot}0052153=0{\cdot}003393=\tfrac{1}{293}\,*.$$

Adopting the formula for the length of the pendulum, or $l=z+y\sin^2(\lambda-\theta)$, the lengths of the pendulum by computation may be compared with the foregoing means derived from experiment.

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* As there is some reason to believe that several of the pendulums near the equator are too great; if the first three of those above be rejected, then the ellipticity will be $\frac{1}{297}$. Indeed, I have for some time been induced to consider $\frac{1}{300}$ to be that which must ultimately be generally adopted.

Lat.

Lat. λ	Reduced Latitude. $\lambda-\theta$.	Experimental Pendulum.	Pendulum computed by $y \times \sin^2(\lambda-\theta)$.	Deviations of Formula.
0°	0° 0′ 0″	39·01650	39·01558	$-0{\cdot}00092=E$
5	4 58 1	39·01877	39·01710	$-0{\cdot}00167=E_1$
10	9 56 5	39·02090	39·02163	$+0{\cdot}00073=E_2$
15	14 54 17	39·02913	39·02904	$-0{\cdot}00009=E_3$
20	19 52 38	39·03856	39·03910	$+0{\cdot}00054=E_4$
25	24 51 14	39·05167	39·05152	$-0{\cdot}00015=E_5$
30	29 50 5	39·06492	39·06594	$+0{\cdot}00102=E_6$
35	34 49 14	39·08166	39·08192	$+0{\cdot}00036=E_7$
40	39 48 42	39·09908	39·09899	$-0{\cdot}00009=E_8$
45	44 48 31	39·11585	39·11664	$+0{\cdot}00079=E_9$
50	49 48 41	39·13329	39·13432	$+0{\cdot}00103=E_{10}$
55	54 49 12	39·15163	39·15151	$-0{\cdot}00012=E_{11}$
60	59 50 3	39·16772	39·16767	$-0{\cdot}00005=E_{12}$
65	64 51 11	39·18213	39·18231	$+0{\cdot}00018=E_{13}$
70	69 52 36	39·19553	39·19497	$-0{\cdot}00056=E_{14}$
75	74 54 15	39·20600	39·20525	$-0{\cdot}00075=E_{15}$
80	79 56 4		39·21284	
85	84 58 0		39·21749	
90	90 0 0		39·21906	

From a review of the whole, it appears that the ellipticity is somewhat greater than it has generally been supposed, though the difference is not very great. It seems also probable that the discordancies of the results derived from the formula for computing the length of the pendulum are not very considerable, and are so irregular that no general protuberance, or uncommon diminution of the force of gravity, is any where likely to follow from the observations by the pendulum.

The only doubt that remains, is perhaps the effect of combining the lengths of the pendulum on stations of very different specific gravities, where a preponderance of dense or light materials is likely to prevail.

It is to be feared that the lengths of the pendulum near the equator are rather in excess, as the stations have been, generally speaking, on bases of considerable density; and by this means rendering the equatorial pendulum too great, the excess of the polar above the equatorial pendulum too small, and consequently the compression too great; though more numerous experiments near that circle are still wanting to decide this point in an unexceptionable manner.

It is to be expected, that some light may be thrown on the exact quantity of compression by the various arcs on the meridian, and the parallels now executing throughout Europe; though I am inclined to believe, that measurements of arcs of the meridian near the equator, and as close to the poles as convenient, will be more decisive. In the execution of these, however, care should be taken to select proper portions of the meridian under uniformity of geological character. Some of the extensive plains in the North of Russia and in South America, appear to me to be the most eligible; though I have seen Spitzbergen recommended, from the facility it affords of executing the necessary operations. Yet for these very reasons I am disposed to think that from its irregular and broken character, by extensive *fiords*, as they are called, running amongst the islands, it is to be feared that very irregular local attractions will be found to prevail; so that each series of zenith distances would be something like the Schihallien experiment; and the irregularities thence produced, would rather tend to throw a doubt over the true compression, than to point out the proper quantity.

Edinburgh, April 1827*. WILLIAM GALBRAITH.

P.S. In the foregoing remarks, it will be seen that I have endeavoured to introduce a few small corrections into the usual formulæ for obtaining the ellipticity, which may perhaps be thought by some, to be too minute to deserve much attention in the present state of the problem, as being much within the probable errors of observation. For instance, instead of taking the ratio of the centrifugal force to gravity at the equator at $\frac{1}{289}$, I have adopted $\frac{1}{289\frac{1}{4}}$ nearly, as it ought to be; and for the coefficient $\frac{5}{2} = 2{\cdot}5$, it is assumed at about 2·49. These, no doubt, appear trifling; but nevertheless their adoption reduces the constant 0·00865 to 0·00861, and this small modification diminishes the ellipticity in an equal degree. I have also preferred the reduced latitudes to the apparent, or those got directly from observation; and this removes an irregularity which at one time I was persuaded occurred in the elliptical figure of the earth; namely, a protuberance at about the parallel of 45° N., which I am now disposed to attribute to a slight error in the approximate formula usually employed. It has also been thought advisable to apply the method of the least squares with caution. I am perfectly aware that this method is the best that can possibly be applied in many cases; but that there may be particular instances where, by employing it without discrimination, it may, from the manner in which it must be employed, be the means of leading to more erroneous conclusions than by a selection of the most decisive observations. As this subject has, since the first part of these remarks was written, been treated of by Mr. Ivory, in a late Number of the Phil. Mag., much more ably than I could pretend to, it will be unnecessary for me to say any thing more at present upon the subject.

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* So in the MS.—EDIT.

Edinburgh, March 12, 1828. W. G.

LII. *Account of a Paper by Prof.* GAUSS, *intitled* "Disquisitiones generales circa Superficies Curvas:" *communicated to the Royal Society of Göttingen on the 8th of October* 1827*.

ALTHOUGH geometricians have much occupied themselves with general investigations on curve surfaces, the results of which form a considerable portion of the higher department of geometry, this subject is so far from being exhausted, that it may be safely asserted that as yet a small part only of a most fertile field of inquiry has been cultivated. The author has already endeavoured some years ago to take a new view of this subject in the solution of the problem: To find all representations of a given surface on another surface, in which the smallest parts shall remain similar. The object of the present paper is to open new views, and to unfold a portion of the new truths which are thereby rendered accessible. We shall explain as much as can be rendered intelligible without entering too deeply into the subject; but we must remark, that the new definitions as well as the theorems, in order to be generalized, will require some restrictions and qualifications which must be omitted in this place.

In investigations which involve a variety of directions of straight lines in space, it is advantageous to designate these directions by those points on the surface of an invariable sphere at which the radii drawn parallel to the same, terminate: the radius and centre of this auxiliary sphere are entirely arbitrary; for the latter, the unity of linear dimension may be chosen. This proceeding agrees, in fact, with the one constantly used in astronomy, where all directions are referred to a fictitious celestial sphere of an infinite radius. Spherical trigonometry, and some other theorems to which the author has added one of frequent application, are then employed for solving the various problems that present themselves by a comparison of the different directions which occur. If the directions of lines normal to the curve surface, drawn from every point of the same, be designated by the points of the sphere, corresponding to them according to the proceeding above explained, so that each point of the curve surface has its corresponding point on the auxiliary sphere, then, generally speaking, every line on the curve surface will have a corresponding one on the auxiliary sphere, and every portion of surface of the former will have its corresponding portion on the latter. The smaller the deviation of a part is from a plane, the smaller will be the corresponding part of the sphere; and it is therefore a very natural proceeding to employ, as measure of the total curvature, the area of the corresponding portion of the sphere. The author, accordingly, calls this area the *entire curvature* of the corresponding portion of curve surface. Besides this quantity, the *position* of the part comes into consideration, which, independently of the relation of magnitude, may be a similar or a reversed one: these two cases may be distinguished by the positive or negative sign being put before the expression of the total curvature. This distinction, however, has only a definite signification, so far as the figures are assumed to be on definite sides of the surfaces: the author assumes them in the sphere on the exterior, and in the curve surface on that side on which the normal line is supposed to be erected; and it follows that the positive sign belongs to convex-convex and to concave-concave surfaces (which are not essentially different), and the negative sign to concave-convex ones. If the portion of the curve surface in question consists of parts dissimilar in this respect, further specifications become necessary, which must here be passed over.

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* From the *Göttingische gelehrte Anzeigen.*—This abstract is probably from the pen of the distinguished Author.

The comparison of the areas of the portion of the curve surface and its corresponding portion of the auxiliary sphere, leads to a new notion (in the same manner as the comparison of volume and mass produces the notion of density). The author calls *measure of curvature* in any point of the curve surface, the value of the fraction whose denominator is the area of an infinitely small part of the curve surface at this point; and the numerator the area of the corresponding part of the auxiliary sphere, or the *entire curvature* of the element. It is clear that in the sense of the author, *entire curvature* and measure of curvature are in curve surfaces analogous to what in curve lines is respectively called amplitude and curvature; he doubted of the propriety of transferring to curve surfaces the latter expressions, which, though sanctioned by use, are not very appropriate terms. It is however of less consequence how these new notions are denominated, than to justify their introduction by remarkable and useful theorems to which they give rise.

The solution of the problem: To find the measure of curvature in each point of a curve surface,—presents itself under various forms, according to the manner in which the nature of the curve superficies is given. The simplest manner is, the points in space being expressed by three rectangular co-ordinates x, y, z, to represent the one co-ordinate as a function of the two others: in this case the expression which is obtained for the measure of curvature is the simplest. This leads at the same time to a remarkable connection between this measure of curvature and the curvatures of those curves which are produced by the intersection of the curve surface by planes perpendicular to it. It is well known that Euler has first demonstrated, that two of the intersecting planes, which are likewise at right angles to each other, have the property,—that to the one belongs the smallest, to the other the greatest radius of curvature; or rather, that in them the extreme curvatures occur. Now it results from the expression for the measure of curvature just alluded to, that this becomes equal to a fraction whose numerator is unity, and the denominator the product of the two extreme radii of curvature.

The expression for the measure of curvature becomes less simple when the nature of the curve surface is given by an equation between x, y, z; and the former still more complicated, when the surface is represented by x, y, z being given as functions of two new variable quantities p, q. In the latter case the expression contains fifteen elements; viz. the partial differential quotients of the first and second order of x, y, z for p and q: but it is less important in itself, than by its being the transition to another expression, which forms one of the most remarkable propositions of this theory. In that manner of representing the nature of the curve surface, the general expression for any linear element of the same, or for $\surd(dx^2 + dy^2 + dz^2)$ assumes this form: $\surd(\mathrm{E}dx^2 + 2\mathrm{F}dx\,.\,dy + \mathrm{G}dy^2)$; where E, F, G become functions of p and q; the above-mentioned new expression for the measure of curvature contains only these quantities, together with their partial differential quotients of the first and second order. It is evident, therefore, that for determining the measure of curvature, the general expression of a linear element only is required, and not the expressions for the co-ordinates x, y, z themselves. An immediate consequence of this is the following remarkable theorem: If a curve surface or a portion of the same can be evolved on another plane, the measure of curvature remains unchanged in every point after the evolution. From this follows, as a particular case; that in a curve surface which can be evolved in a plane, the measure of curvature is always $=0$. From this is immediately derived the characteristic equation of the surfaces capable of being evolved in a plane; namely, if z be regarded as a function of y and x

$$\frac{ddz}{dx^2}\;\frac{ddz}{dy^2}-\left(\frac{ddz}{dx\,.\,dy}\right)^2=0,$$

an equation which, indeed, has long been known, but in the author's opinion has never yet been rigorously demonstrated.

These propositions lead to a new view of the theory of curve surfaces, and open a wide uncultivated field of investigation. If surfaces be considered not as bounds of bodies, but as bodies one dimension of which is evanescent, and at the same time flexible but not expansible, it will be conceived that two essentially different classes of relations must be distinguished,—those which suppose a definite shape of surface in space, and those which are independent of the various shapes which the surface is capable of assuming. It is of the latter that the author treats; and it appears by what has been observed above, that the measure of curvature is one of them: but it will easily be seen that the consideration of the figures which can be described on the surface, their angles, their areas and *entire curvature*, the connection of the points by shortest lines, &c. belong to this class. All these investigations must proceed from this, That the nature of the curve surface is given by the expression of an indefinite linear element of this form $\sqrt{(\mathrm{E}dp^2 + 2\mathrm{F}dp\,.\,dq + \mathrm{G}dq^2)}$.

The author has inserted in the present paper a part of the investigations on this subject, which have engaged him during several years, confining himself to such as are not too remote from the point where his labours began, and which may serve as general auxiliaries for numerous further investigations. In this notice we must be still shorter, and be content to give only as a specimen the following theorems.—If on a curve surface, a system of an infinity of shortest lines, all of equal length, proceed from *one* point, the line passing through their extreme points is at right angles to every one of them. If from every point of any line on a curve surface, shortest lines of equal length and at right angles to that line are drawn,—all these lines are likewise at right angles to that line which connects their other extreme points. Both these theorems, the second of which may be considered as a generalization of the first, are demonstrated analytically, as well as by simple geometrical considerations. The excess of the sum of the angles of a triangle

angle formed by shortest lines above two right angles, is equal to the *entire curvature* of the triangle. It is here supposed, that the unity of angles is the one whose corresponding arc equals the radius ($57° 17' 45''$); and that for the entire curvature, which is a portion of the surface of the auxiliary sphere, the area of a square whose side is the radius of the auxiliary sphere is considered as unity. This important theorem may evidently be thus expressed: The excess of the angles of a triangle formed by shortest lines above two right angles: eight right angles :: the portion of the auxiliary sphere corresponding to that triangle : the whole surface of the auxiliary sphere. Generally the excess of the angles of a polygon of n sides, all shortest lines, above $2n-4$ right angles, is equal to the *entire curvature* of the polygon.

The general investigations contained in the paper are finally applied to the theory of triangles formed by shortest lines. We shall mention only a few of the principal theorems of this theory. If a, b, c be the sides of such a triangle (which are considered as quantities of the first order), A, B, C the opposite angles, α, β, γ the measures of curvature in the angular points, σ the area of the triangle; then $\frac{1}{3}(\alpha + \beta + \gamma)\,\sigma$ will be the excess of the sum $A + B + C$ above two right angles down to quantities of the fourth order. With the same accuracy the angles of a plane rectilinear triangle, the sides of which are a, b, c, will be

$$A - \tfrac{1}{12}(2\alpha + \beta + \gamma)\,\sigma$$
$$B - \tfrac{1}{12}(\alpha + 2\beta + \gamma)\,\sigma$$
$$C - \tfrac{1}{12}(\alpha + \beta + 2\gamma)\,\sigma$$

It is clear, that the latter theorem is a generalization of a well known proposition first given by Legendre, by which, omitting the quantities of the fourth order, the angles of the rectilinear triangle are obtained by diminishing the angles of the spherical one by one-third of the spherical excess. For surfaces which are *not* spherical, unequal reductions must be applied to the angles, and, generally speaking, the unequal part is a quantity of the third order: if, however, the whole surface deviates but little from the spherical form, it involves besides a factor of the same order with the deviation from the spherical form. It is, undoubtedly, important for the theory of geodetical operations, to be able to calculate the inequalities of these reductions, and to obtain thereby the full conviction that they are to be considered as insensible for all measurable triangles on the surface of the earth. Thus it is found, that in the largest triangle in the measurement conducted by the author,

thor, the greatest side of which is nearly fifteen (German) geographical miles, and in which the excess of the sum of the three angles above two right ones is nearly fifteen seconds, the three reductions of the angles to angles of a rectilinear triangle are 4″·95113, 4″·95104, 4″·95131. The author has, however, likewise calculated the terms of the fourth order wanting in the above expressions, which assume for the sphere a very simple form; but in measurable triangles on the surface of the earth they are quite insensible; and, in the above example, they would have diminished the first reduction by only two unities in the fifth decimal place, and have increased the third reduction by the same amount.

LIII. *On* Savart's *Experiments on the Motions of mediately agitated Membranes. By Dr.* WM. WEBER, *Academical Teacher at Halle*[*].

[With an Engraving.]

"FROM the manner just stated of the division of membranes into vibrating divisions," says Savart, in the *Ann. de Chim. et de Phys.* (1826. tom. xxxii. p. 385.) "it is evident that the acoustic figures called by Chladni distortions, form the transition between a variety of figures of sound which are not distorted (in which may be reckoned several tones of the flageolet). As Chladni has only observed the distortions of figures of sound which are nearest the undistorted figures (which he considers as fundamental figures), and as he fixed the number of vibrations merely by the ear, which does not admit of sufficient accuracy, he might justly assert that the tone in the distortion of the figures of sound is the same as in the fundamental figures. But in the tables accompanying his *Traité d'Acoustique*, we find distortions of figures of sound to which belong semitones, a whole tone, and even one-third higher than when the figure of sound has what is termed the fundamental form." The difference between Chladni and Savart, according to these observations of the latter, is, that Chladni asserts that there is in *sounding* plates no transition from one tone of the flageolet to another; whilst Savart says that by a repetition of experiments, he has discovered this transition.

Savart justly says, that nothing in nature appears isolated. He has actually shown in this treatise, as well as in some former ones, an extraordinary number of transitions of one kind of vibration into the other, in almost all bodies used for musical purposes. But it is also a universal rule of nature, that the transitions of the different phænomena do not show themselves when these phænomena appear most regularly and forcibly, but only when they become indistinct and indefinite. Now the *sounding vibrations* (which Chladni always considers by themselves) are constantly the *most uniform* and the *most violent vibrations.* There is, therefore, probably, a transition from a sounding vibration, through a series which do not produce sound (such as are less distinct and precise) standing vibrations, to a very different sounding vibration; but there is no transition from a sounding vibration through nothing but *sounding* vibrations to a quite different sounding vibration (producing a much higher or much deeper tone). However, Savart is perfectly right in affirming that many of the distortions of the figures of sound observed by Chladni are the beginnings of the transition of one kind of vibration to another, and that in these distortions, even the number of vibrations, whilst the tone is becoming weaker and beginning to cease, is a little altered. *But if a vibrating body gives a distinct tone, it is in a very uniform and violent vibration, which is only possible when the number of its vibrating divisions is strictly fixed, and the sum of the motions is in all as equal as possible, in which case the height of the tone (the velocity of the vibrations) is unchangeable, as it depends as much on the elasticity and form of the membrane, as the velocity of the vibrations of a pendulum of a given length on gravitation.* This rule, laid down by Chladni, has thrown an extraordinary light and intelligibility on all acoustic phænomena.

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[*] From Schweigger's *Jahrbuch der Chemie*, &c. N. R. Band xx. p. 176.

In order, therefore, to render what has been said still more intelligible; and in consequence of there being several similarities in the motions of membranes observed by Savart, with the vibrations of sounding elastic *plates;* and finally, as it is now generally necessary to find out by experiment as closely as possible the extension of small vibrations on visible bodies;—I shall here communicate from the 32nd volume of the *Ann. de Chimie et de Physique*, the phænomena discovered by Savart.

Savart examined the lines upon which the sand remains in its place on mediately agitated bodies.

If an equally-stretched square rectangular or triangular membrane is mediately and regularly agitated, grains of sand scattered over it are thrown off from equally large and regularly-bounded divisions of those membranes, but are collected on the limits of these divisions nearly in the same manner as they are on the sounding plates.

Savart wished to cause the stretched membranes to vibrate mediately and with the utmost uniformity, and thereby to determine at his pleasure, the quicker or slower result of the agitations, and to become perfectly acquainted with its velocity. How could these objects be better attained than by the sounding vibration of an organ-pipe, a bell, or disk, before which the membrane was expanded, so that every vibration transmitted by the air of those sounding bodies necessarily agitated the membrane?

In this manner he made the following double series of experiments. Once he held the extended membrane before an organ-pipe, which he was enabled by means of a stopper to lengthen or shorten at will, by which means the same membrane was successively agitated by very various rapid concussions. In the second place, he stretched a membrane of a very hygrometrical substance, viz. of paper, and made it gradually imbibe an increasing quantity of aqueous vapour. In this manner he was enabled to agitate a membrane of very different degrees of elasticity by constantly equally-quick concussions, *ex. gr.* by means of a sounding plate or bell held before it.

The membranes thus mediately agitated show, according to Savart's observations, the following similarity to sounding plates:

1. In mediately agitated membranes vibrating divisions are formed, which are separated by quiescent or slightly moved lines, as in the sounding plates.

2. These quiescent lines may undergo such distortions as Chladni has observed in the sounding plates. On the other hand, there are the following differences between the motions of mediately agitated membranes and sounding plates. First, In mediately agitated membranes the divisions near the edge are as great as those within, whilst in sounding plates the divisions near the border are not half so great. Secondly, In mediately agitated membranes *the different distortions of the quiescent lines are produced by the different width of the agitating undulations*, (*i.e.* by different high tones of the organ-pipe placed before the membrane,) whilst the observation in the sounding plates, as is generally known, is,—that when the quiescent lines are somewhat distorted, the breadth of the waves of the undulation proceeding from the plate is either not altered at all, or very imperceptibly, which may be easily known from the height of the tone.

If we hold a square membrane, the elasticity of which is not changed, before the opening of an organ-pipe provided with a stopper and made to vibrate, we may effect by means of the stopper the circumstance that the sand thrown off from the divisions represented in Plate VI. fig. 1. No. 1. remain only on the boundary line. If the tone of the organ-pipe becomes a trifle higher, the form of this boundary line upon which the sand remains, varies as in No. 2; and becomes, as the tone of the organ-pipe gets still higher, as in No. 3, 4, 5, 6, where the boundary lines merely consist of four parallel lines. In this manner the boundary lines which at first intersected each other at right angles, are changed into parallel lines. This transmutation may also be effected as represented at fig. 2, 3, 4, 5, 6, 7.

In the same manner four parallel nodes may pass into two parallels which have towards the former a perpendicular position, as shown at fig. 8; or four parallel nodes may change into four others intersected by two more perpendicularly, as shown in fig. 9. Other remarkable changes of the position and number of these boundary lines with increasing velocity of the agitating concussions, are seen in fig. 10, 11, 12, 13.

From these experiments it may be seen that there is a possibility of several transitions of one and the same sand-figure to another; as for instance, fig. 10. and fig. 11. The question is, what is it that produces any particular transition? Savart mentions a criterion by which, from the first alteration of the figure, it may be foretold what transition will ensue. He says:

First. On proceeding from a figure of nodes intersecting each other at right angles, the character of the following changes depends on the magnitude of the vertical angle on the points of intersection separate from one another. This is very apparent in the comparison of fig. 10. with fig. 11. which both form transitions of four parallels, from two other lines normally crossed, into six parallel lines.

Secondly. If, on the contrary, we have at first only parallel nodes, we may say that the character of the following changes depends on the difference of the curves which these lines may receive, which is distinctly seen from the same figures (10. and 11.), if one begins backwards (from No. 5, 4.); for in fig. 10. the nodes curve inward, whilst in fig. 11. they curve outward. The transitions are particularly remarkable when the lines form two curves outward, and one inward, or *vice versâ;* or when they form three curves outward, and two inward, &c. of which fig. 12. and fig. 13. furnish remarkable instances.

Circular and triangular membranes produce analogous phænomena. On a circular membrane, three diametrical lines may in this manner be produced, and these may be gradually again changed into one diameter and one circular line (fig. 14.). Moreover, on a circular membrane five diametrical lines may be produced and gradually changed into five parallels, as shown in fig. 15; and these five parallels may again be transformed into one diametral and two circular lines.

Very narrow long rectangular strips show similar phænomena.

mena. Thus the fundamental lines, where the sand keeps its position, may have the form fig. 16. No. 1. As the tone becomes deeper, all these lines approach the end B, so that the interval A*n*, and *n n'*, and *n' n''*, become larger; but *n''* finally advances to B, so that the membrane has only two lines left. Another change is shown in fig. 17.

Savart has formed the hypothesis, that the membrane agitated by the undulations of the air is also in a standing and sounding vibration; that from this sounding membrane waves of sound are proceeding exactly of the breadth of those which arrive at it: that he therefore knew the breadth of the waves of sound proceeding from the membrane, from knowing the breadth of those which arrive at it. From these hypotheses it is inferred, that the modes of vibration produced by various flageolet sounds are gradually transfused into one another by an uninterrupted series of intermediate tones. What now may be said of sounding membranes, says Savart, is probably also the case with sounding elastic plates. But according to Chladni's account, it is not so with sounding plates. Savart thinks that Chladni has been mistaken in his observations. (See the beginning of this paper.) But in all the experiments I have hitherto made, I have always found Chladni's observations confirmed. But should Savart find a different result, it would be desirable that he should exactly point out the material, form, and thickness of the plate with which he made his experiments, as well as the manner of his having fastened the plate and made it vibrate, in order that the experiment may be repeated *.

These discoveries of Savart *merely concern the extension of small oscillations in firm bodies, in membranes;* as there is no question here about sounding bodies, nay not even of reverberating bodies, as one may be convinced by the ear, but only as to vibrations and soundless motions, which however show much similarity to the motions of reverberating and even sounding bodies.

I had a small wooden square frame made, which measured in the hollow six Paris-inches in length and breadth. Upon this I glued a sheet of wet English letter-paper (wove paper, without any wire-marks), which was perfectly free from thin places or other defects, and glued upon this paper small laths, so that it was equally stretched, and its edge everywhere immoveable.

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* Chladni has investigated, in his *Acoustics*, Leipzig, 1802. § 114. p. 131. a particular case, in which with a similar fundamental figure, but which once has received a curve outward, and at another time inward, several tones are produced, and has proved that here too no transition from one flageolet-tone to another, through an uninterrupted series of tones, is manifested.

Such a paper will sound even on any one pushing against the frame or gently blowing upon it. If one strewed upon this paper held horizontally some grains of coarse sand, and held a bell of a watch or a small disk of glass over the paper, *ex. gr.* near a corner, making it sound by means of a violin-bow, the sand began to move, and collected in those lines described by Savart.

In these experiments I found:—

1. That here also in the mediately agitated membranes an actual intersection of the reposing lines nowhere takes place, as Oerstedt and Strehlke have observed in sounding plates.

2. That the deepening or heightening of the tone of the organ-pipe, and the change of elasticity by the wetting of the membrane, are not the only means whereby the nodes in mediately agitated membranes became curved; but that this curving of the nodes might also be produced by trifling circumstances; by bringing, for instance, a sounding bell sometimes near a corner and sometimes near the centre of the square membrane.

3. That, as I have stated before, no trace of self-sounding or reverberation of the paper is to be observed. At the meeting of the Society for the Investigation of Nature, at Halle, on the 14th of July in the present year (1827), I repeated these experiments, and convinced the assembled members of it, especially the editors of this Journal.

I consider therefore the careful separation of the sounding reverberating vibrations from those slight motions of the bodies that do not sound or reverberate (which may certainly be very similar to the former), and which I have indicated in this article, as necessary; since great confusion is introduced by the interference of the latter with the laws of motion of sounding disks and reverberating bodies.

It has been shown in the Doctrine of Undulation, published by my brother and myself, how sounding and reverberating vibrations, figures of sound and figures of vibration, should be distinguished; and how it results from this, that Savart's and Chladni's experiments do not contradict each other*: but we must also distinguish between these vibrations and *vibrations without any acoustic effect*, which latter may also *unite the scattered sand into nodes.* We have further shown, in the work just quoted, that the sounding vibration is always of the class of the standing vibrations, with which all small motions of all bodies seem to terminate. From the circumstance that in all these small motions of bodies left to themselves, *at last* a certain balance and uniformity manifest themselves; and that this uniform *final vibration* is, in fact, the standing vibration, it may be explained why standing vibrations, and especially the sounding, are most independent of the *first* vibration—of the excitement of the tone; and also why Chladni's figures of sound are likewise very independent of it. The reverberating vibration consists of the *first* intersection of the waves just excited, and may in many instances coincide with the sounding vibration; for which reason also the reverberating figures are often represented like the figures of sound, (see Savart's former treatises). One recognizes, however, the figures of reverberation by a great dependence on the original agitation, *ex. gr.* on the direction in which the sounding body is moved. Finally, it is evident, that vibrations without any acoustic effect may at times be of the same nature as the sounding and reverberating vibrations; for all standing vibrations, for instance such as are so weak as not to act upon the organs of hearing, belong to this class.

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* Vide *Jahrbuch,* 1825.

Supplement to the foregoing Paper.——*On the Employment of a resounding Membrane for the Observation of the Interference of the Undulations of Sound**.

I HAVE stated in an Essay on Savart's Experiment with mediately agitated Membranes, (See p. 340, 341.) that in these membranes we observe neither an original sound nor a resonance, when an organ-pipe, a bell, a swinging disk, or any other longitudinally or transversely swinging body of a flat surface is held before the membrane. I have however succeeded in making such a membrane resound by simply holding a tuning-fork before it, so that one prong was held close to it, parallel with the diagonal line of the square frame. The sound of a tuning-fork by itself is but slightly heard, or not at all, at a little distance. But on bringing it in the manner described towards the membrane, without touching it, the sound is heard distinctly; while it is perceived that it proceeds, not from the fork, but from the membrane.

(With the help of such a resounding membrane I have been enabled to observe the phænomena of the action of the undulations of sound, the description and investigation of which has appeared in the *Jahrbuch*, 1826. iii. p. 385 to 430.)

The sounding of the membrane is in fact very distinct, when the outside of a prong is turned towards it. It is equally distinct when the two prongs are brought near at an equal distance; but on turning the tuning-fork gradually from the first position into the second, we come to a point where the sounding of the membrane suddenly ceases almost entirely; but on the fork being turned further, it immediately reappears. If we distinguish in the prongs of the tuning-fork three different surfaces; viz. 1. front and back surfaces; 2. side surfaces; 3. end surfaces; we observe this disappearance of sound at every transition from a front surface to a side one, and *vice versâ:* again, in every transition from a front surface to an end one, and *vice versâ;* but no cessation of sound is perceived in the transition from a side surface to an end one, and *vice versâ*. These phænomena are not distinctly audible when there is any noise, for which reason the experiments will be best made at night.

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* From Schweigger's *Jahrbuch*, Band xx. p. 247.

The following differences in these phænomena from those mentioned in the treatise alluded to (in the *Jahrbuch* 1826, iii. p. 385) are remarkable: 1. That the membrane is a plane extending far beyond the tuning-fork, and that every point of it can be agitated with equal facility, whilst the reciprocating inclosed column of air had only one narrow opening, and could only resound when the undulations of sound penetrated through this aperture; 2. That the membrane did not reciprocate, and nevertheless showed these phænomena, whilst with a column of air not reciprocating, at least if it be narrow, nothing can be observed.

LIV. *On the Figure of the Earth, as deduced from Measurements of different Portions of the Meridian.* By J. Ivory, *M.A. F.R.S.**

HAVING examined the figure of the earth as deduced from experiments with the pendulum, we are naturally led to consider the same question as it depends upon the measurements that have been made, of different portions of the meridian. If this interesting inquiry be placed within the reach of the human understanding, and be accessible to our industry, the two views of it must be consistent; and, by instituting a comparison between them, we may both learn what is already sufficiently well ascertained, and be directed in our future attempts to the most important points that still remain uncertain.

Let a denote the equatorial radius of the earth; e the eccentricity of the elliptical meridian; φ an arc of the meridian reckoned from the equator; and λ the latitude of the extremity of φ: then

$$d\varphi = \frac{a(1-e^2)\,d\lambda}{(1-e^2\sin^2\lambda)^{\frac{3}{2}}}.$$

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* Communicated by the Author.

If we now put $\sqrt{1-e^2} = 1-\varepsilon$, then ε will be the compression or the ellipticity; and we shall have,

$$d\varphi = \frac{a\,(1-\varepsilon)^2 \,.\, d\lambda}{\left(1-(2\varepsilon-\varepsilon^2)\sin^2\lambda\right)^{\frac{3}{2}}}.$$

By expanding this formula and integrating as usual, rejecting the cube and higher powers of ε, we shall get,

$$\varphi = a\,.\left\{\lambda - \varepsilon\left(\frac{\lambda}{2} + \frac{3}{4}\sin 2\lambda\right) + \varepsilon^2\left(\frac{\lambda}{16} + \frac{15}{64}\sin 4\lambda\right)\right\}.$$

And, if φ denote an arc of the meridian between the latitudes λ' and λ; then

$$\varphi = a\,.\left\{\lambda - \lambda' - \varepsilon\left(\frac{\lambda-\lambda'}{2} + \frac{3}{4}\left(\sin 2\lambda - \sin 2\lambda'\right)\right)\right.$$
$$\left. + \varepsilon^2\left(\frac{\lambda-\lambda'}{16} + \frac{15}{64}\left(\sin 4\lambda - \sin 4\lambda'\right)\right)\right\}.$$

Further, let $n = \lambda - \lambda'$

$m = \lambda + \lambda'$

$p = 57^\circ{\cdot}2957795$

$\Delta = \frac{a}{p}$;

$$\text{then,}\quad \phi = \Delta\,.\left\{n - \varepsilon\left(\frac{n}{2} + \frac{3}{2}\,p\sin n\cos m\right)\right. \qquad \text{(A)}$$
$$\left. + \varepsilon^2\left(\frac{n}{16} + \frac{15}{16}\,p\sin n\cos n\cos 2m\right)\right\}.$$

In this formula Δ is the length of 1° on the earth's equator; and n and the coefficients of ε and ε^2, are reckoned in degrees.

There has been so much discussion about the merits of the different portions of the meridian that have been determined trigonometrically, and their character is so well known, that it would be superfluous to say any thing on that subject here. The following table, which is taken from a paper by Professor Airy, in the Phil. Trans. for 1826, contains the five arcs which unquestionably deserve the preference, both as they are the longest that have been measured, and likewise on account of the excellent instruments employed, and the great care taken in executing all the operations. But even of these there is one, namely the Swedish arc, measured by Svuanberg, to which some objections are made; because it makes the length of a degree in latitude 66° 20′, more than 200 toises less than it had been found to be by Maupertuis and the French Academicians who accompanied him. I shall therefore leave out this arc in determining the elements of the elliptical figure of the earth; but, these elements being found by means of the other four arcs, we may then apply them to the case omitted, both as some test of their own exactness, and likewise in order to judge what real ground there is for objecting to the Swedish measurement.

Country.	λ	λ'	φ fathoms.
Peru..........	$-0°\ 2'\ 31''$	$3°\ 4'\ 32''$	188510
India..........	8 9 38·39	18 3 23·6	598630
France.......	38 39 56·11	51 2 9·2	751567
England......	50 37 5·27	53 27 29·89	172751
Sweden	65 31 30·27	67 8 49·55	98870

Applying now the data of the four first measurements in the table to the formula (A), the four equations following will be obtained,

$$\left.\begin{aligned} 188510 &= \Delta\ (3{\cdot}11750 - \ \ 6{\cdot}2261\,.\,\varepsilon + \ \ 3{\cdot}095\,.\,\varepsilon^2) \\ 598630 &= \Delta\ (9{\cdot}89589 - 18{\cdot}1985\,.\,\varepsilon + \ \ 6{\cdot}163\,.\,\varepsilon^2) \\ 751567 &= \Delta (12{\cdot}37030 - \ \ 6{\cdot}2811\,.\,\varepsilon - 10{\cdot}266\,.\,\varepsilon^2) \\ 172751 &= \Delta\ (2{\cdot}84017 - \ \ 0{\cdot}3844\,.\,\varepsilon - \ \ 2{\cdot}166\,.\,\varepsilon^2). \end{aligned}\right\} \text{(B)}$$

It is known that ε is not much different from $\frac{1}{300}$, and certainly does not surpass $\frac{1}{150}$. We may therefore assume,

$$\frac{1}{300} + t; \ \ - = \frac{1}{90000} + \frac{2t}{300};$$

the correction being a small fraction which we are sure is less than $\frac{1}{300}$. The foregoing equations will now become, by substitution,

$$\begin{aligned} 188510 &= \Delta\ (3{\cdot}09678 - \ \ 6{\cdot}206\,t) \\ 598630 &= \Delta\ (9{\cdot}83530 - 18{\cdot}157\,t) \\ 751567 &= \Delta (12{\cdot}34925 - \ \ 6{\cdot}350\,t) \\ 172751 &= \Delta\ (2{\cdot}83887 - \ \ 0{\cdot}399\,t). \end{aligned}$$

In order to find a first approximate value of Δ, I add the four equations, neglecting the terms containing t: thus

$$1711458 = 28{\cdot}1202\,.\,\Delta\,; \quad \Delta = 60862.$$

Next, put $\mathrm{D} = 60862$, $\Delta = \mathrm{D}\,(1 + s)$; then, by substituting in the four last equations, we shall get,

$$\begin{aligned} \frac{35}{\mathrm{D}} &= \ \ 3{\cdot}097\,s - \ \ 6{\cdot}206\,t \\ \frac{35}{\mathrm{D}} &= \ \ 9{\cdot}835\,s - 18{\cdot}157\,t \\ \frac{33}{\mathrm{D}} &= 12{\cdot}349\,s - \ \ 6{\cdot}350\,t \\ -\ \frac{\text{[illegible]}}{\mathrm{D}} &= \ \ 2{\cdot}839\,s - \ \ 0{\cdot}399\,t. \end{aligned}$$

These equations are next to be reduced to two, the number of the unknown quantities, by employing the method of the least squares: thus

$$\frac{31\cdot550}{D} = 267\cdot234\,s - 277\cdot342\,t$$

$$+\frac{632\cdot332}{D} = 277\cdot342\,s - 408\cdot673\,t.$$

And, by solving these equations,

$$Ds = -6; \qquad \Delta = D + Ds = 60856$$

$$t = -\cdot00009, \quad \varepsilon = \frac{1}{300} + t = \cdot00324 = \frac{1}{309}.$$

Returning now to the original equations (B), and substituting the values of Δ and ε, we shall obtain the errors, $E^{(1)}$, $E^{(2)}$, $E^{(3)}$, $E^{(4)}$, or the quantities that must be added to the given measurements on the left sides of the equations, in order to make both sides exactly equal; as follows,

$$E^{(1)} = -20, \quad E^{(2)} = +10, \quad E^{(3)} = -7, \quad E^{(4)} = +13.$$

The greatest error is in the Peruvian arc, which appears to be too great, as has generally been surmised. But the smallness of the errors proves the consistency of the different measurements; and shows that the four portions of the meridian, although very remote from one another, belong to one and the same elliptical spheroid.

If we now substitute the data of the Swedish measurement in the formula (A), we shall get,

$$98870 + E^{(5)} = \Delta\,(1\cdot622022 + 0\cdot8378\,.\,\varepsilon - 0\cdot022\,.\,\varepsilon^2):$$

and, by substituting the values of Δ and ε, it will appear that the error $E^{(5)} = +5$. This measurement is therefore very consistent with the other four; and, as it seems liable to no just objection, it adds to the evidence in favour of the elliptical elements that have been found.

The degree of the meridian bisected by the parallel of 45°, is 111115 metres at zero of the thermometer. This result is deduced from the actual measurement of the meridional arc between the parallels of Greenwich and Formentera, and is independent of any assumption about the figure of the earth. To reduce this length to fathoms at the temperature of 62° of Fahr., it must be multiplied by $\frac{39\cdot37079}{72}$, which gives 60758 fathoms. Now, if we put $\lambda = 45° + \frac{1}{2}$, $\lambda' = 45 - \frac{1}{2}$, we shall have, in he formula (A), $n = 1°$, $m = 90°$, and the length of the degree having its middle point in latitude 45°, will be,

$$\left(1 - \frac{\varepsilon}{2} - \frac{7}{8}\varepsilon^2\right);$$

or, in numbers 60856 (1 —·001629) = 60757 fathoms, which is only one fathom less than the observed quantity.

There is also another measurement with which we may compare the elliptical elements that have been found. A portion of the parallel to the equator in latitude 45° 43′ 12″, little short of 13° in amplitude, has been measured, and the most probable length of 1° of longitude on that parallel has been fixed at 77865^{m}·75*. The large arc was measured in six portions, the difference of longitude between the extremities of every portion being ascertained by means of signals made by exploding gunpowder. If we compare the six degrees, deduced from the several partial arcs, it must be allowed that their differences are great and irregular†. There must therefore be considerable uncertainty in the arithmetical mean of such irregular quantities; which mean is only three metres less than the most probable value of a degree. The length of a degree deduced from the four partial arcs contained within the territory of France, which together exceed 7° of longitude, is 77885^{m}·75, or 20 metres more than the length deduced from the whole arc of about 13°‡. It is to be observed that the errors of longitude at the intermediate stations accumulate as the arc is extended; so that there is not the same advantage in measuring a large arc of the parallel, as in the case of the meridian; for, in one instance, the total amplitude is affected by the sum of the errors at all the intermediate stations; whereas, in the other, it is affected only by the errors of the two extreme latitudes. If we further add that a great length of the parallel answers to a minute portion of time; in so much that an error of a single second in the amplitude of the large arc, would produce a variation of no small magnitude in the mean degree of longitude; it must be evident that in point of accuracy, we cannot repose the same confidence in a degree of the parallel measured in the manner described, as we can in a degree of the meridian deduced from an arc of nearly the same amplitude.

The radius of the parallel to the equator at the latitude λ, or the perpendicular drawn to the polar axis, will be found, by the properties of the ellipse, equal to

$$\frac{}{\sqrt{1-e^2\sin^2\lambda}} = \frac{}{\sqrt{1-(2\varepsilon-\varepsilon^2)\sin^2\lambda}}.$$

And if this expression be expanded and multiplied by $\frac{1}{p}$, we shall have the length of a degree of the parallel equal to

$$\Delta\cos\lambda\left\{1+\varepsilon\sin^2\lambda+\varepsilon^2\left(\frac{3\sin^4\lambda}{2}-\frac{\sin^2\lambda}{2}\right)\right\}.$$

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* *Conn. des Tems* 1829, p. 291. † Ibid. p. 290. ‡ Ibid. p. 293.

Now, put $\lambda = 45^\circ\ 43'\ 12''$, and a degree of longitude at that latitude, will be equal

$$42487{\cdot}8 \times (1 + {\cdot}001661) = 42557{\cdot}4 \text{ fathoms.}$$

But $77865^{m}{\cdot}75$ reduced to fathoms at 62° of Fahr. is equal to 42578·2 fathoms, which is 20 fathoms more than the calculated quantity. If this discrepancy appear very great, we shall only remark, that it is very small in comparison of the differences of the degrees deduced from the partial arcs, and that it supposes not quite so much as $1''\frac{1}{2}$ of time, for the sum of all the accumulated errors in the amplitude of the large arc. Unless we possessed some means of estimating with tolerable exactness the error really existing in the degree of the parallel, the discrepancy we have found can hardly be deemed sufficient to throw any doubt upon the elliptical elements which agree so well with all the most exact measurements that have been made on the earth's surface.

If the earth be really an elliptical spheroid, there can be little doubt that the ellipticity must be very nearly what we have found it to be. But the solution of the problem would be greatly improved, and the results obtained would be more exact and more certain, if the measurement of the meridian in England were extended as far north as possible. An arc of 8° or 10° north of Dunnose, would be the most valuable addition that in the present state of this research can be made to the data for determining the figure of the earth. Another very profitable addition would be the extension of the Indian arc, already the largest we have except the French measurement; and more especially if it could be extended southward nearer the equator. Supposing the difference between λ and λ' to be small, it will readily appear that the coefficient of ε in the formula (A) will be equal to zero, or so small as to render the term insensible, when

$$\text{Cos}\,(\lambda + \lambda') = -\tfrac{1}{3}, \text{ and } \lambda + \lambda' = 109^\circ\ 28'.$$

Thus if, by means of the formula mentioned, we compute the expression of the length of an arc extending northward from Dunnose about a degree beyond Portsoy, to latitude 58° 55′ 30″, it will be found that the length of this large arc is almost independent of the ellipticity ε, and equal to an arc of the equator. The reason is that, in latitude 54° 44′, the radius of curvature of the meridian is equal to the radius of the equator, and the degree of the meridian having its middle point in that latitude is equal to Δ, that is, to a degree of the equator. Thus the length of the portion of the meridian contained between the extreme north and south points of Britain depends chiefly on Δ, and very little on the other element ε; at the equator, the compression ε has greater influence on the length of a meridional arc, than at any other position on the surface of the globe; and it follows that two arcs of considerable extent, in the situations mentioned, combined with other good measurements in our possession, must lead to a very exact determination of the elliptical elements of the figure of the earth. One remark it seems important to add; namely, that the ellipticity cannot be determined with any precision by combining different degrees of the meridian measured in England; because the element sought has little influence on the lengths of such degrees. And this observation may be extended to all latitudes within 15° or 20° of 54° 44′ on either side; within which limits the ellipticity contributes only an inconsiderable share to the length of a meridional arc. And what has now been said will serve to explain the discrepancy of the ellipticities deduced by combining different degrees of the meridian within the boundaries of England and France; and likewise the great variation between the quantities so obtained, and what is found by the comparison of remote degrees. These observations may be illustrated by examining the equations (B). In the English and Swedish measurements, the coefficients of ε are small, and have opposite signs. The coefficients of the same quantity are nearly equal in the French and Peruvian arcs, although the former has more than four times the length of the latter. In the Indian and French measurements, the coefficients are as 3 to 1, while the arcs themselves are as 3 to 4; and this shows how important it is, for the exact determination of the ellipticity, to have a large arc near the equator.

April 12, 1828. J. Ivory.

LV. *Mineralogical and Chemical Examination of the Diopside of Fassa in the Tyrol. By Dr.* H. Wackenroder, *Professor of Chemistry at Göttingen*[*].

Among the various new minerals discovered by Bonvoisin[†] during a journey through Piedmont, there were two which this mineralogist considered as distinct species of minerals; and he called them, after the places in which they were found, *Mussite* and *Alalite.* According to an account given by Tonnellier[‡], Haüy found in the crystalline form of these two minerals such a degree of conformity as induced him to consider them to be one and the same mineral, and which, from its peculiar crystallographic characters, he presented as a particular species, of the name of *Diopside*[*]. Soon afterwards, however, Haüy himself published a memoir, in which, after a further investigation of this mineral, he proved, from crystallographical reasons, that diopside coincides with pyroxene[†]. He also found diopside to agree with pyroxene in its physical characters[‡].

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[*] Communicated by the Author.

[†] *Journ. de Phys.* par Delametherie, tom. lxii. p. 418, 423.

[‡] *Journ. des Mines,* tom. xx. p. 65.

Having found the specific gravity of the mussite to be 3·2374, that of the alalite 3·31, and that of pyroxene from Vesuvius 3·3578, he says that the specific gravity of diopside lies within the limits of that of pyroxene, as indeed the remaining external characters of the mineral substances hitherto considered to belong to pyroxene, sometimes differed more from each other than from diopside.

Other mineralogists have grouped diopside, according to their different views of the systematic arrangement of minerals, with different species, comprehended under augite or pyroxene. Thus Hausmann[§] enumerates diopside among the formation of the malacolites in the pentaklasite substance; Leonhard[‖] quotes it as a variety of augite; Mohs[¶] refers it, together with the coccolite, fassaite, augite, sahlite, baikalite, malacolite, &c. to the paratomous augite-spar, &c. Independent of these well-known classifications of diopside in the different mineralogical systems, it must form, in a system founded on the relation of the external characters to the chemical composition of the minerals, one of the divisions which, in consequence of the experiments made on the substituted constituents, must now be made in the species of pyroxene and augite. It is in consequence of this that, according to the latest system of Counsellor Hausmann, the diopside is considered as an augitic substance, and as represented by the formula

$$CS^2 + MS^2.$$

To this formation also belongs the diopside which I lately bought of a dealer under the name of *Diopside of Fassa*, and which I subjected to a strict examination, as we do not possess, to my knowledge, either a chemical analysis or a detailed description of the diopside of the valley of Fassa.

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[*] *Journ. des Mines*, tom. xx.

[†] "Sur l'Analogie du Diopside avec le Pyroxène;" in the *Ann. du Mus. d'Hist. Nat.* tom. xi. p. 77. *Journ. des Mines*, tom. xxiii. p. 145.

[‡] *Journ. des Mines*, tom. xxiii. p. 154, 156.

[§] *Hanbuch der Mineralogie.* Band ii. p. 694.

[‖] In his *Hanbuch der Oryktognosie.* New edit. p. 503.

[¶] In his *Grundriss der Mineralogie.* 2d vol. p. 306.

I. *Mineralogical and Chemical Examination of the Diopside of the Valley of Fassa.*

This diopside, in the state I received it, consisted of single loose crystals, which appeared to have been attached. They usually are a couple of inches long, and are mostly compressed, so as to appear flat. They are generally free from extraneous substances, and I saw only a few crystals covered on their surface with laminæ of chlorite. But they are found more frequently covered with a gray tarnish or bloom resembling iron, which is especially perceptible near the ends, and seems to have at times come out from the mineral itself, in which case it also seems more intimately connected with it.

I am indebted to the obliging kindness of my respected teacher Counsellor Hausmann, for the following crystallographical communication on the diopside of the valley of Fassa.

" The crystals of diopside, usually broken at the ends, present themselves most frequently as rectangular prisms, the greater sides of which are more or less striated longitudinally, whilst the other sides are either smooth or slightly striated.

" The appearance of planes replacing the lateral edges explains the nature of the surface of these rectangular prisms. For the planes agreeing with the planes E of the prism of augite, which, according to Haüy, are respectively inclined at 92° 18′ and 87° 42′, the narrower planes of the sides which form with the replacing planes an angle of 136° 9′, present themselves as the planes B′, and the broad ones as the planes B.

" Sometimes the planes BB′ 3 (*f*) appear perfectly formed, making with each other angles of 141° 44′ and 38° 16′. A disposition to the formation of these planes is distinctly discovered in the deep striation of the planes B.

" The observed combinations are:

2 B′. 2 B
2 B′. 2 B. 4 E.
2 B′. 4 BB′3.
2 B′. 2 B. 4 BB′3
2 B′. 4 E. 4 BB′3.

" The respective inclinations of these planes are, the angles given by Haüy being taken as the basis,

$B' - B = 90^\circ\ 0'$
$B' - E = 136\ \ 9$
$B\ - E = 133\ \ 51$
$B' - f = 109\ \ 8$
$B\ - f = 160\ \ 52$
$E\ - f = 152\ \ 59$
$f\ - f = 141\ \ 44$

"Sometimes a combination of several individual crystals is seen. One end of the crystals is always fractured, while the other often shows a tendency to a summit.

"Besides the marked cleavages of E′, those of B′ and B are also perceptible. Besides the cleavages, we perceive fissures; also transverse fissures, which are sometimes irregular and sometimes corresponding to the planes D′."

The fracture of diopside is imperfectly conchoidal and splintery. The angular pieces which are detached from it are frequently tabular and prismatic.

Its colour is a pistachio-green or olive; but sometimes runs in the same prism from greenish white to colourless.

The crystals are usually translucent only, sometimes transparent. Small detached laminæ can always be seen through, and are slightly coloured. The fissures in the interior of the crystals sometimes produce a play of colours.

I have not been able to perceive a double refraction in these crystals. The external planes as well as the cleavage-planes show a glassy and pearly lustre. It scratches fluor spar, and is scratched by felspar.

According to Brewster, diopside will show signs of electricity; but neither by heating nor by rubbing it, have I been able to discover any electricity in this variety. As little does this diopside act on the magnet. Its specific gravity, at 59° Fahr. is = 3·299. A piece strongly striated not belonging to the purest portion, gave at the same temperature a specific gravity of 3·296; and one almost white, covered at one end with the above-mentioned bloom, a specific gravity of 3·277. Heated in a platinum spoon, the diopside of Fassa loses its peculiar green colour, but suffers no further change.

In the flame of the blowpipe, sharp splinters of it are rounded at the edges, but by means only of a very strong heat; while thick pieces only become of a dirty green colour. With borax it is dissolved very slowly and without colouring the bead.

Carbonate of soda at a red heat dissolves it with ebullition, and gives a bead of a reddish colour. Phosphate of soda mixed with the mineral in powder, and melted before the blowpipe, yields a dark yellowish bead. On charcoal with nitre it yields a greenish mass, which by further heating assumes a dirty reddish colour. Concentrated sulphuric acid, muriatic acid, or nitric acid, does not dissolve the diopside.

II. *Preliminary analytical Experiments.*

A certain quantity of pulverized diopside ignited with thrice the quantity of carbonate of soda freed from water, gave a half-melted mass, which was dissolved in water and muriatic acid. The solution having been evaporated to dryness, and the residuum digested with water and muriatic acid, a large quantity of silica remained undissolved.

Carbonate of soda precipitated from the solution when cold some oxide of iron with a few traces of alumina; but oxalate of potash precipitated much oxalate of lime, which was partly ignited and partly mixed with caustic ammonia. A very small quantity of deutoxide of manganese was found mixed with the lime. After this, carbonate of magnesia together with a little manganese was precipitated, by means of carbonate of soda, from the hot solution; and subsequently, by means of triple phosphate of soda and ammonia, a considerable quantity of magnesia.

Again, about 2·0 grammes of pulverized diopside were fused with carbonate of soda and 1-6th of its weight of nitre. Water gave with the melted mass a greenish solution, which soon completely lost its colour in the air; and on being tested for the chromic, fluoric, phosphoric and boracic acids, no trace of either was perceptible.

III. *Analysis for the Proportions of the Constituents.*

a. 2·502 grammes of select and very pure pieces of diopside dried on a hot iron plate, were exposed for three quarters of an hour to a red heat. But the mineral was unaltered in appearance and weight, except that its colour turned nearer bottle green.

b. 2·503 grammes of the diopside finely powdered, were intimately mixed with thrice their weight of carbonate of soda freed from water and exposed for half an hour to a moderate red heat. The fused mass was dissolved in a moderate quantity of water, placed in an evaporating-dish, and diluted with as much muriatic acid as was necessary to effect a perfect solution. By the evaporation to dryness of the yellowish solution, and by digesting the residuum (which was of the same colour) in water and some muriatic acid, pure white silica was obtained, weighing, after having been dried and heated, 1·3581 gramme. It was completely dissolved in a solution of caustic potash, as well as in a concentrated solution of carbonate of soda assisted by heat; and after having stood for some time, only a few unimportant flakes were deposited from this solution. A part of it only was dissolved in caustic ammonia, but the remainder was entirely taken up by carbonate of soda. It will be worthy of a distinct investigation to ascertain the cause of this phænomenon*.

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* Dr. C.F. B. Karsten has also mentioned the facility with which silica is dissolved in caustic ammonia. See Poggendorff's *Annalen*, Band vi. p. 357.

c. The solution having been copiously diluted, was mixed first, in the cold, with carbonate, and then with bicarbonate of soda, in order to separate the oxide of iron and alumina. The precipitate was then slightly heated, and again dissolved in concentrated muriatic acid. There remained a small quantity of silica, which after having been heated, weighed 0·002 gramme, and remained unaffected even after a continued digestion in sulphuric acid.

d. The solution of iron in (*c*) was abundantly mixed with caustic potash, and the precipitated hydrate of iron separated and slightly heated. The 0·0701 gramme of pure oxide of iron calculated for protoxide, in which state the iron evidently exists in the mineral, give 0·6029 gramme of protoxide, if the equivalents of the two oxides bear the proportion to each other of 39 to 35.

e. Carbonate of ammonia, with heat, precipitated from the solution (*d*), which had been saturated with muriatic acid, the alumina contained in the mineral; which, after the burning of the filtre that had been used to separate it, and after deducting the ashes of the latter, consisted of no more than 0·005 gramme.

f. In order to separate the manganese from the liquid (*c*) which had been treated with carbonate of soda, I made use of the method lately pointed out by Counsellor Stromeyer*. Accordingly the solution, in consequence of the small quantity of manganese it contained, was first reduced by boiling to a smaller bulk, saturated with chlorine gas, and then mixed with as much neutral carbonate of soda as was necessary to precipitate the deutoxide of manganese, and render the solution colourless. The precipitate having been dried off for forty-eight hours, and then collected on a filtre and heated, amounted to 0·0027 gramme of deutoxide of manganese, which, according to the equivalents, 39·17 for the deutoxide, and 36·5 for the oxide, indicate 0·0025 gramme of the latter, it being requisite to assume that the manganese is in this state of oxidation in the mineral.

g. The solution having been again concentrated by boiling, it was mixed with as much oxalate of potash as was necessary to precipitate the lime; and the precipitate separated as soon as possible by filtering, its separation having been promoted by a small addition of oxalic acid. The oxalate of lime obtained was made red hot, in order to destroy its acid, again dissolved in nitric acid; and the solution mixed with caustic ammonia freed from carbonic acid, gave a slight precipitate. This when made red-hot amounted to 0·0024 gramme, and consisted of deutoxide of manganese, which reduced to oxide, according to the proportion of these two oxides assumed above, is equal to 0·0022 gramme of oxide of manganese.

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* *Götting. Gelehrte Anzeigen.* St. 158. October 1827.

The reason of this small quantity of manganese yet being found, is the facility with which, on its being separated from solutions by means of chlorine, a small portion of it is redissolved, when the precipitate is left in contact with the fluid beyond the time necessary for its formation, during which a little muriatic acid may be produced.

h. The nitric solution of lime was now, while boiling, mixed with caustic ammonia and carbonate of ammonia. The precipitated carbonate of lime gave, after heating, 0·6064 gramme of pure caustic lime, which was completely soluble in greatly diluted nitric acid, and with little effervescence.

As in precipitating the lime with carbonate of ammonia, especially without a sufficient addition of caustic ammonia, small quantities may easily be left suspended, and the carbonate of lime not being quite insoluble in the water used for washing it;—the solution having been much boiled down, was again tested with oxalate of potash, and by means of it a little more oxalate of lime was precipitated, but which, when made red-hot, yielded only 0·0150 gramme of caustic lime. Consequently the entire quantity of pure lime separated amounts to 0·6064 + 0·0150 = 0·6214 gramme.

i. In order to separate the magnesia from the solution (*g*), the former was acidulated with muriatic acid, concentrated by boiling, and mixed with phosphate of soda and caustic ammonia. At the expiration of about twenty-four hours the crystalline deposit was collected and heated: it weighed 1·1790 gramme.

After a second similar treatment, rendered necessary by the easy solubility of the substance in a large quantity of water, even if the latter should be mixed with some caustic ammonia, which may perhaps itself promote the solubility by the access of carbonic acid from the atmosphere,—another precipitate was obtained weighing, after being heated, 0·0582 gramme. Consequently the whole of the phosphate of magnesia obtained amounts to 1·2372 gramme; in which, if 100 parts of this salt, heated, indicate 37 parts of magnesia*, 0·4577 gramme of the earth are contained.

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* See the above-mentioned Essay of M. Stromeyer.

Consequently, 2·503 grammes of the diopside of Fassa are composed of

Silica	(*b*)	1·3581	1·3601 gramme.
	(*c*)	0·0020	
Lime	(*h*)	.....	0·6214
Magnesia	(*i*)	.....	0·4577
Protoxide of iron	(*d*)	.....	0·0629
Oxide of manganese	(*f*)	0·0025	0·0047
	(*g*)	0·0022	
Alumina	(*e*)	.....	0·0050
			2·5118 grammes.

Accordingly, 100 parts of this diopside contain,

		Oxygen.	
Silica	54·154	27·07	14·22
Lime	24·740	6·94	
Magnesia	18·222	7·28	
Protoxide of iron	2·504	0·54	0·68
Oxide of manganese	0·183	0·05	
Alumina	0·195	0·09	
	100·001		

If as the essential constituents of the diopside we take only silica, lime and magnesia, the composition of this mineral is to be expressed by two single bisilicates of lime and magnesia. But this expression presupposes 2·74 per cent more silica than the analysis has furnished; which difference, however, will be reduced by one per cent, if we assume in the magnesia 38·71 per cent of oxygen; consequently assume in the quantity of magnesia found 7·05 parts of oxygen, instead of putting 20 as its equivalent, as we have done in conformity with Gmelin's latest *Handbuch der Chemie*, whose statement of equivalents has been followed throughout in this calculation.

If we wish yet to take into consideration, that, if the proportion of magnesia should be diminished by so much as was precipitated in the analysis (*i*), but the omission of which may have been assumed by chemists for the calculation of the calcined magnesia, the magnesia first obtained would then contain very nearly as much oxygen as the lime: we have exactly such quantities of the principal constituents of the diopside, that the latter may be exactly expressed, in a chemical point of view, by the symbols,

$$(CaO + 2SiO) + (MgO + 2SiO);$$

and in a mineralogical one by

$$CS^2 + MS^2.$$

The composition of the mussite indeed, according to Laugier*, differs considerably from that of our diopside: we might, however, presume that this difference partly arises from the less perfect state of chemistry with respect to those substances in Laugier's time. It is possible, too, that the same circumstance has produced the smaller difference from the composition of our mineral which appears in the analysis of the malacolite or sahlite examined by Vauquelin†.

Among the other existing analyses of minerals referred to pyroxene or augite, according to which other formations of augite-substance are proved, we must consider, with respect to the diopside of Fassa, several examinations made by Von Trolle Wachtmeister Hisinger, Von Bonsdorf, and H. Rose. From the comparison of the results of these analyses enumerated in the well-known treatise of Professor Rose, " On the Pyroxene ‡," with those of the analyses just-mentioned of Laugier and Vauquelin, and with those obtained by me on the composition of the diopside of Fassa, for which purpose I have prepared the following table; it seems that all the minerals thus examined, may be referred to the diopside, and their composition expressed by the formula CS^2+MS^2.

Analyses.	Silica.	Lime.	Magnesia.	Oxide of Iron.	Ox. of Manganese.	Alumina.	Loss by Heat.	Total.
Laugier.—Mussite of Piedmont	57·5	16·5	18·25	6 with ox. ma.	......	...	...	98·25
Von Trolle Wachtmeister. — White malacolite from the TafelTjötten, in Norway...	57·40	23·10	16·74	0·20 protox.	......	0·43	...	97·87
H. Rose.—Yellowish malacolite from Längbanshyttan in Wärmland	55·32	23·01	16·99	2·16	1·59	...	...	99·07
Hisinger. — Malacolite from Längbanshyttan in Wärmland	54·18	22·72	17·81	2·18	1·45	...	1·20	99·54
H. Rose. — Greenish sahlite from Sahla	54·86	23·57	16·49	4·44 protox.	0·42	0·21	...	99·99
Von Bonsdorf. — Perfectly white malacolite from Tammare in Finland	54·83	24·76	18·55	0·99	......	0·28	0·32	99·73
H. Rose.—White malacolite from Orrajerwi in Finland	54·64	24·94	18·00	1·08	2·00 mang·	...	...	100·66
H. Wackenroder. — Diopside from Fassa in the Tyrol ...	54·154	24·740	18·222	2·505 protox.	0·183	0·198	...	100·001
Vauquelin. — Malacolite or sahlite	53·00	20·00	19·00	4·00 with ox. mangan.	......	3·00	...	99·00

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* *Ann. du Mus. d'Hist. Nat.* tom. xi. p. 153.

† Haüy, *Traité de Minéralogie*, 1st edit. vol. iv. p. 382.

‡ Schweigger's *Journ. f. Chem. u. Phys.* Band xxxv. p. 86. &c.

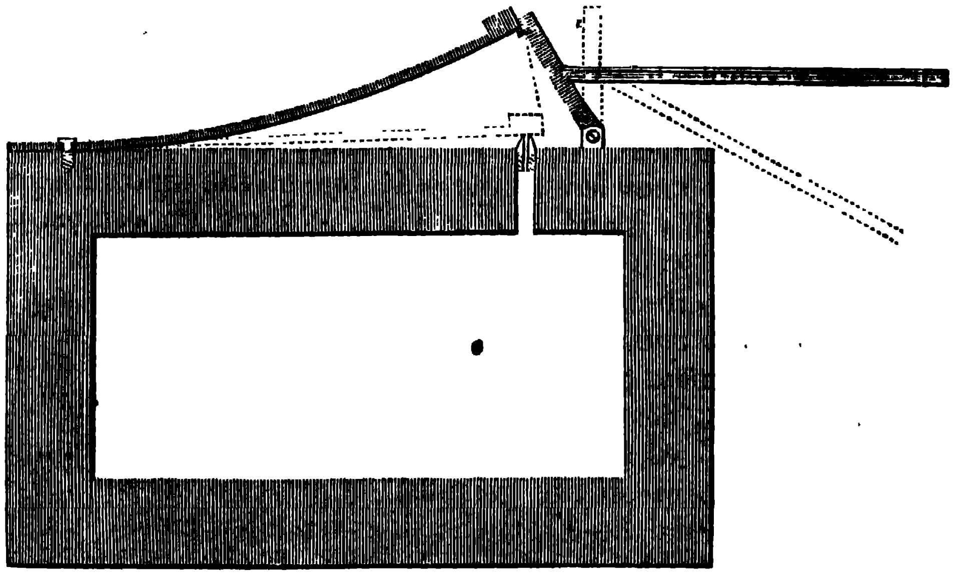
LVI. *De-*

LVI. *Description of a Shell, exploding by Percussion when trod upon. By Lieut.-Col.* Miller, *F.R.S.**

THIS is a very simple contrivance, and will easily be understood from the figure. A spring is attached to the upper part of the shell, which produces ignition by falling on a copper cap; and a support is placed under the spring, moving on a hinge, with a handle attached to the support; so that by treading on the handle, the support is withdrawn, the spring falls, and explosion follows. The sides of the shell must be of equal thickness, for the better splintering; and as an oval form will be the most convenient for fixing the spring. The following construction is given:

Length of shell	8 inches.
Breadth of ditto	5·5
Depth of ditto	5
Length of chamber	6
Width of ditto	3·5
Height of ditto	3
Length of spring	6
Fall of ditto	1
Length of handle from 10 to 30.	

Vertical section of shell when cocked, taken lengthwise.



Shells of this description, it is conceived, might be made to perform the duty of sentinels on many occasions, by giving notice of the approach of an enemy, and presenting a considerable obstacle to his advance by their explosion. They might accordingly be used with advantage in the ditches of fortresses, before breaches, and to defend bridges and passes, wherever an enemy is likely to attempt a surprise. They might also be placed around field works likely to be attempted by assault. They would require to be sunk a little in the ground, so that the splinters of one might not derange those near to it, and covered lightly over to protect them from wet, and also to conceal their position from an enemy.

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* Communicated by the Author.

This principle might also be applied to the firing of artillery by percussion; more particularly at sea, where the roll of the vessel presents so great an obstacle to accuracy of fire. In that case, the vent of the gun would require to be placed a little on one side, to be clear of the line of sight, and a hole drilled through the spring opposite the vent, to allow the flash from it to escape. The support of the spring would, of course, be pulled away by a string, so that the man who laid the gun might also fire it.

LVII. *Account of the Iron Mine at Haytor, in Devonshire. By* J. T. KINGSTON, *Esq.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

A MINERAL production discovered at the Haytor Iron Mine having formed the subject of two or three papers in your valuable Journal[*], you may perhaps consider a description of the mine itself not unworthy of occupying a page or two of your next Number; especially as it is, at least to the best of my knowledge, the only one of the kind hitherto discovered in this island, and as such, of some importance in a geological as well as in an œconomical point of view[†].

The lode, to the depth at present explored, is a very regularly stratified one, of oxidulated iron ore and argillaceous schist, in alternate beds; and is situated on the edge of the granite district, near the base of the Haytor rocks. The hill, on the brow of which, near the centre, it crops out, is immediately incumbent on the granite; its principal slope is gradual and towards the East, the sides having a more precipitous descent to the North and South. It consists chiefly of micaceous passing into clay schist, and of trap[‡] (provincially termed Ironstone) of a compact basaltic texture, and great specific gravity; and mostly containing a proportion of iron-ore, which occasionally runs in distinct threads and patches through small portions of it. The lode occurs in the clay schist, and the direction of its strata is nearly North-west and South-east, underlying to the North-east at an angle of 22° or 23° only for the first few feet from the surface; but below this the dip is very regular at an angle of 45°. It happens to be situated in two distinct estates, which a road, intersecting the lode near the centre, divides; and having hitherto been worked separately, a complete wall from the surface to the depth explored, has been left, of which the following is an accurate section and measurement; the measurement is taken at the depth indicated by the dotted line, which is also the direction of the level driven from the back of the lode.

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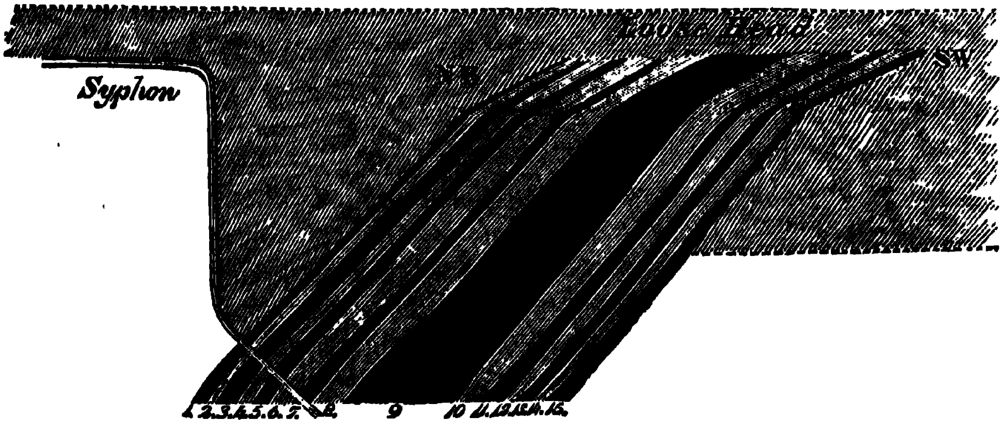
[*] See Phil. Mag. and Annals, N.S. vol. i. p. 38, 40, 43.

[†] Some particulars of the vein of ore worked in this mine, will be found in Mr. W. Phillips's paper on Haytorite, which is the first of the communications just referred-to.—EDIT.

[‡] Mr. W. Phillips, we observe, has stated that this substance appears to be siliceous schist.—EDIT.



Thickness of Beds.		
No.	Ore.	Schist.
1	$1^{\text{ft.}}\ 0^{\text{in.}}$	
2		$0^{\text{ft.}}\ 6^{\text{in.}}$
3	1 3	
4		1 3
5	1 3	
6		2 0
7	0 6	
8		2 6
9	8 0	
10		3 6
11	2 0	
12		1 3
13	0 6	
14		1 0
15	1 6	

16 feet ore.
12 feet schist.

28 feet whole width of lode.

By this it will be seen, that the central bed is of iron-ore, and considerably the largest; whilst the other alternating beds of schist and ore are disposed above and below it in a tolerably regular relative proportion. The schist having a tendency to contract in width, as the depth increases, and the ore to approximate each way towards the central bed, into which, the obvious probability is, that the other beds, at a greater depth, run; but whether this is the case, or whether, if they do so, the central bed is proportionally increased, must of course remain uncertain, until a level is driven, which I understand it is proposed to drive from the northern ravine, and which would cut the lode at about 150 feet perpendicularly below the surface. Permanent springs rise at the depth of a few feet, the water from which is carried off by a syphon bent over the northern slope of the hill. A level driven from the back of the lode, at the depth of about 20 feet from the surface, through schist, in a south-west direction, intersected, about 30 feet from its commencement, another small bed of ore, about 3½ feet in width, in which a large proportion of iron-pyrites is disseminated, the dip being the same as that of the principal lode, with which, however, it does not seem to be connected; the latter being included in a well-defined manner, within the limits pointed out by the above section, from which the width will be seen to be about 28 feet. The length, of course, is not capable of being so accurately defined: from the part where the section was taken on the western side, the traces are observable on to the granite, a distance of 230 yards, which they do not enter, but are conformable along its edge for some distance in a northerly direction; and on the eastern or lower side, it has been traced for the distance of 250 yards, down to a compact stratum of trap*, blended with garnet and actinolite, against which the traces appear to have been hove in a zigzag curved manner, and to dip under it. The length however of the beds containing ore of a quality available for smelting, does not (at least to the depth at present excavated) exceed 150 yards.

The surface along the whole line of the lode, as well as on each side of it, is apparently regular and undisturbed, and consists of a loose head of flat schistose stones and fragments, and the earthy mould arising from their decomposition, to the depth of five or six feet. In this head, at either end, along the line of the lode, bunches of manganese contaminated by iron occur. Near the central part of the lode an old sinking was discovered to the depth of 42 feet on one single bed of the ore. There remain no records in the neighbourhood, as to when or for what purpose this excavation was made; but the probability is, that it was mistaken for tin, old stream-works for obtaining which abound in the neighbourhood. Only a small portion (if any) appears to have been carried off; as the chief part was left in heaps, and strewed on the surface to the extent of two or three acres immediately round, and which in fact led to the discovery of the mine, and remained unaccounted for some time after.

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* See former note on this substance.—Edit.

The chief part of the ore is of a compact texture, but portions of it, especially on approaching the surface, are coarsely granular; more or less perfectly formed crystals, loosely aggregated, are also frequent. The per-centage of iron it contains varies from 40 to 70 (the average probably of what has hitherto been worked yielding in the large way about 50); some of the richer specimens are actively magnetic; when pulverized, the ore is brownish black, and passively magnetic. It occurs also mixed with sulphur and with arsenic, in coarsely granular masses. Spathose carbonate of iron and also iron-pyrites are met with; the latter, either in decomposing granular concretions, radiated, in more or less perfect cubic crystals, and in small spangles disseminated through the coarser granular ore. Copper-pyrites and arsenical pyrites also occur, the former very sparingly.

The other mineral specimens discovered in the faults and cavities of the lode, and in the loose head immediately above it, having excited considerable local attention, I shall (with your permission) give as complete an enumeration of them as I am able, though a minute and lengthy description would be equally tedious and unnecessary; as they are chiefly aqueous depositions of siliceous and aluminous matter, coloured by the oxides of iron and manganese, and do not essentially differ from other formations of the same kind of frequent occurrence in other mining districts; for as to the statement, that precious gems, &c. had been discovered there, that being now admitted to have originated in a mistake, no further notice need be taken of it.

Quartz—Occurs in a granular form, in corroded amorphous masses, in thin plates crossing each other at right angles (cellular quartz), stalactitic and crystallized. The crystals are all secondary, and mostly hexagonal prisms, terminated by an hexagonal pyramid; the faces generally unequal. In some the pyramid is nearly triangular, the alternate angles being deeply replaced by planes. In some the edges, either of the prism, or pyramid, or both, are bevelled; occasionally very regular prisms, terminated by a pyramid at each end, are met with, and in some specimens the prism is so shortened that the pyramids appear to be nearly joined base to base. Columnar hexagonal prisms of opaque quartz are of frequent occurrence, forming the nucleus, round which the other crystals are often clustered, or stalactitic chalcedony deposited. Of the transparent crystals, some are colourless; of the rest, the colours are ferruginous (this most common), clear topaz yellow, dark red passing into black; smoky brown (cairngorm); various shades of purple, violet, or rose-pink (amethystine quartz), and bright ruby red: these last are minute and very perfectly formed crystals, lining cellular quartz. The finest specimens of the amethystine quartz occur (very sparingly) in geodes, in the loose head; the rest are found clustered in rounded, reniform, or radiated masses, lining cavities, and investing and invested by chalcedony.

Flint—In amorphous masses, and passing into chalcedony, &c.; the colours mostly ferruginous, or brownish black.

Chalcedony—Passing into and also investing crystallized quartz; stalactitic (this occasionally incloses distinct globules of water), mamillated, botryoidal, and lenticular. The prevailing colours, various shades of ferruginous brown passing almost into black; but occasionally, specimens are met with of clear yellow; pure milk-white, passing into pearly and translucent white; cæruleau, passing into peach-colour, and light gray; and fine plum-colour (the last from a bunch of manganese). Frequently two or three successive coatings, sometimes of the same, sometimes of distinct colours, invest each other, between each of which a thin lining of clay is often interposed; some of the white and cærulean varieties are hydrophanous; others have a very delicate coating of aluminous matter, which causes their colour to deepen considerably on the application of moisture. Pseudo-morphous crystals of this substance also occur, formed within cavities from which other crystallized substances have been by some natural cause, (probably solution,) removed. Irregular masses of siliceous matter that have obviously invested, either wholly or partially, crystals of iron-ore. Iron-pyrites, quartz or garnet, are of frequent occurrence. I have specimens of the substance in moulds of this kind, and conceive that much of (if not all) the Haytorite (as those crystals have been named) is referable to some of the different forms of the above substances, and is chalcedonic matter filling the said cavities by stalagmitic deposition; some of the faces of the crystals are very splendent, others rough; in some of the crystals all are rough: the fracture is different from that of a true crystal, nor have I ever succeeded in obtaining a regular cleavage; the crystals are of various sizes;

the sides of the planes in some exceeding an inch, in others scarcely 1-8th of an inch. They are mostly found in a matrix of ferruginous clay, in juxta-position with massive chalcedony, quartz, and specular iron.

Semi-opal—Mostly in irregularly rounded nodules, invested by indurated clay (lithomarga). The colours, various shades of blue, green, or yellow, occasionally all passing into each other in the same specimen, or forming distinct stripes. It also frequently alternates in stripes with the indurated clay.

Garnet—Massive, blended with actinolite, &c. in the lode, and especially at the eastern end; and crystallized, mostly in rhomboidal dodecahedrons, and their modifications. The colours, various shades of yellowish or reddish brown, passing into black minute crystals, occur (rarely) on the surface of the ore under glassy actinolite. Most of the crystals are coarse, and of inferior size.

Actinolite—Intimately blended with the ore, in amorphous masses, and in distinctly radiated clusters, the radii from 1-8th of an inch to an inch in length: the colours varying from a light to a very dark green. A thin variety, with a vitreous lustre, occurs sparingly on the surface of the ore. Actinolite is very abundant, and gives a greenish hue to a large portion of the ore. It is most prevalent in the upper and under beds, and seems to increase with the depth.

Some fine cabinet-specimens of all the above species have been found; an interesting series of which (the most complete I believe that has been formed) is in the possession of Shirley Woolmer, Esq. of Exeter.

The ore, when melted, produces iron of a very tough and superior description, as I have myself proved by some experiments. It is also stated, on good authority, to be capable of conversion into excellent steel; but the purpose to which it has hitherto been applied is that of mixing with the argillaceous ironstone of the Welch coal-measures, to improve its quality; for which, I understand, it is preferable to the hematitic ores of Lancashire; and this, I should suppose, will be the purpose that it will ultimately be found most practically available for; as its distance from proper fuel (the Bovey coal, situated within three or four miles of it, from some late experiments that have been made, giving very little hope of being efficient for the purpose of smelting, and the Dartmoor peat being at too great a distance) would probably be an effectual obstacle to the erection of any works in the neighbourbood, even supposing a less limited quantity of ore to exist, than at present appears to be the case. But this is a part of the subject on which I am incompetent to treat, and it is besides foreign to the object of this communication, which is to illustrate a small portion of a district, that in the circle of a few miles (taking the large clay-deposit of Bovey-heath as a centre) is perhaps, in a geological point of view, one of the most interesting districts in the kingdom. And I propose (in conjunction with J. G. Croker, Esq. of Bovey Tracey, who has for many years paid great local attention to the subject), if consistent with the plan of your Journal, to make it the medium of two or three communications (accompanied by sections, &c.), pointing out carefully the different strata, their extent and junction, and the lodes, as far as explored, that occur in them. To this gentleman, as well as to Mr. Petherick the superintendent of the iron-mine, I am indebted for much valuable assistance in drawing up the present paper. I remain, Gentlemen,

Your most obedient servant,

Ilsington, Devon, April 9, 1828. J. T. KINGSTON.

LVIII. *Account of a new Method of mounting Thermometers.*

By Mr. W. MAGEOUGH.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE following is an account of a method of mounting the thermometer-tube, in order to make it applicable to purposes for which, as fitted-up at present, it could not be used.

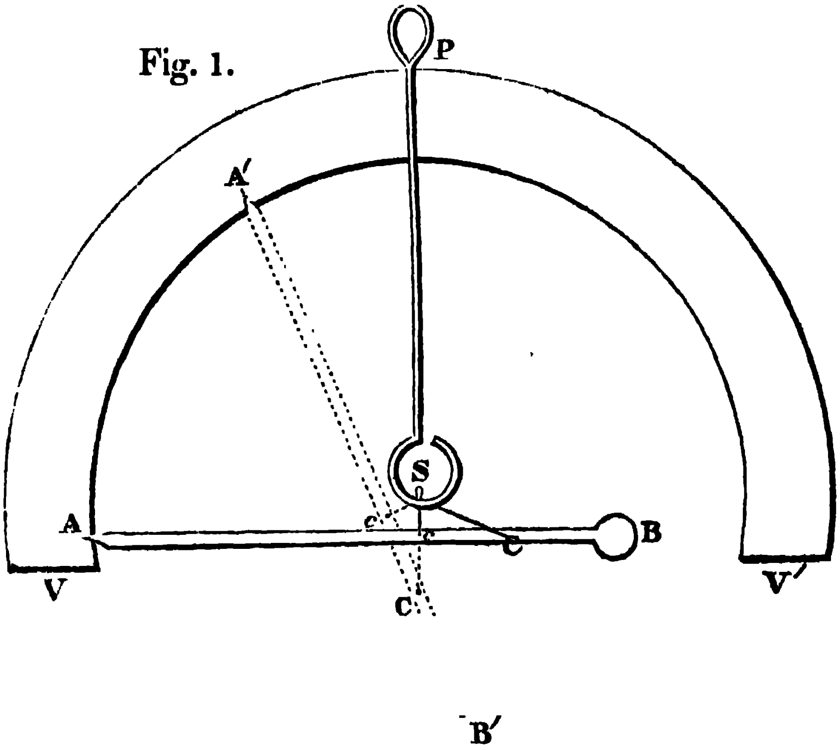
This method has not, that I know of, been hitherto practised in making thermometers; but about two years since, the same principle was successfully applied to barometers.

Mounted in this manner, the tube may be made of earthenware or metal, as well as of glass; it may contain metals or other substances which will expand by heat; and by a little management be introduced into, and made to indicate degrees of temperature, that would speedily destroy thermometers of the ordinary construction. The present account relates only to tubes of glass, such as are generally used for making thermometers, without any other condition, than that the size be adapted to the purpose proposed to be effected by the instrument. If large enough, it will move with sufficient force to give notice when the apartment where it is placed attains a given temperature, by touching a light lever connected with the sort of alarum-machinery often put into the cheapest clocks. Or, a prepared paper may be made to pass over a hair-pencil, charged with a liquid not liable to dry or freeze, which pencil, by means of a simple addition to the hour-movement of a common clock, will trace on the paper all the changes in temperature occurring through twelve or twenty-four hours.

But, where nothing more is required, the highest and lowest degrees of temperature happening in a given time may be registered by the assistance of a couple of fine slips of light wood, or two bristles, poised on axles, the ends being brought over the circular scale so as to come in contact with a catch fixed on the tube.

Let AB, fig. 1. represent a thermometer tube, containing the mercury, and being marked at the boiling and freezing points. Such tubes are easily obtained at the opticians. Let C be the centre of gravity when the mercury is at the freezing, and *c* the centre of gravity when it is raised to the boiling-point; S is the place of an axle fixed across the tube, and resting in two hooks or rings, one of which is represented in the figure at the termination of the wire PS. Suppose the point S to be in a perpendicular to the axis of the tube, erected at the point *c*; then it is clear that when the mercury rises to the boiling-mark, the centre of gravity moves into *c*, this point will therefore get into the perpendicular to the horizon under S, and put the tube AB into the horizontal position, as shown in the figure. Again, when the mercury sinks to the freezing-mark, the centre of gravity coming into C will cause that point to get into the perpendicular under S, and put the tube into the position A′B′: the arch AA′ through which the end of the tube

Fig. 1.



tube has moved, measuring an angle equal to 90° less by the angle *c*CS.

If the arch AS and the tube be graduated, the corresponding divisions are easily noted as the mercury rises or sinks, and the divisions can be marked on the former at leisure.

Because in the tubes commonly to be obtained, the distance *c*S, of the point of suspension above the axis of the tube, must be very small, the adjustment is rendered comparatively easy, by making the axle of a piece of thin steel wire bent in the form of fig. 2, the inner part of the curve being as nearly as possible half the circumference of the tube; the curve being underneath, the axle is fastened to the tube by a thread wound four or five times round both, and they may be secured from slipping by a drop or two of varnish. Now it is evident that by increasing or diminishing the distance between the points of support or bearing hooks, the centre of gravity *c* will be more or less sunk below the points of suspension S, and the distance *c*S will be obtained without the trouble of filing. A knife-edge is however undoubtedly preferable to the wire.

Fig. 2.

To find the centres of gravity on the tube, it is only necessary to sling it in a horizontal position, by means of a thread fastened to it at each end, and put over a pin: then a plumb-line, hung from the same pin, will cross the tube at the centre of gravity, the place of which should be marked on the glass by a file or diamond. The arch VPV′ may be of pasteboard, wood, or metal, and should be attached to the wires SP. At P is a loop or ring by which the instrument is suspended.

If the figure of the tube be altered, and the bulb contain spirits of wine in the upper part and mercury in the lower, it is evident the distance between the centres of gravity at any two temperatures, and consequently the power of the instrument, will be much increased. The shading in the subjoined figure shows the portion of the bulb and tube to be occupied by the quicksilver when the temperature is low.

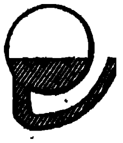


Fig. 3.

It may be added that thermometers of this construction might perhaps advantageously accompany wheel-barometers in large apartments or halls. I am, Gentlemen, &c.

Feb. 23, 1828.
W. Mageough.

LIX. *No-*

LIX. *Notices respecting New Books.*

Part I. of a Descriptive Catalogue of the Lepidopterous Insects contained in the Museum of the Honourable East-India Company, Illustrated by Coloured Figures of New Species and of the Metamorphosis of Indian Lepidoptera, with Introductory Observations on a General Arrangement of this Order of Insects.—By THOMAS HORSFIELD, M.D. F.R.S. L.S. & G.S. *Member of the Royal-Asiatic, and Zoological Societies of London, and of the Imperial Academy Naturæ Curiosorum; Corresponding Member of the Academy of Natural Sciences of Philadelphia, and of the Historical Society of Pennsylvania, &c. To be completed in Six Parts.*

THE subjects which this work will bring before the public are arranged in the Museum of the Honourable East-India Company. They consist principally of a general series of Lepidopterous insects from the island of Java, accompanied by an extensive set of drawings representing their metamorphosis, the history of which is detailed in the Introduction. To this will be added various subjects contained chiefly in a collection of insects from Ceylon, presented by M. Jionville, and in a smaller miscellaneous collection from continental India, presented by Claude Russell, Esq., the brother of Dr. Patrick Russell. It is likewise the author's intention to include such additions as may be made from time to time to the Museum during the progress of his work, from the territories of the Honourable Company in the Eastern World.

The work will be published in royal quarto, and will consist of six parts, each containing about eighty pages of letter-press; the distribution of the subjects being the following:

Part I. INTRODUCTION: detailing the outline of a general arrangement of Lepidopterous insects according to their metamorphosis. Description of the First Tribe, or of the LEPIDOPTERA DIURNA. Stirps the *first*, with VERMIFORM LARVÆ. Genera *Polyommatus* and *Lycæna*.

Part II. Conclusion of the Vermiform Stirps. *Second* Stirps of Lepidoptera Diurna with CHILOGNATHIFORM or IULIFORM LARVÆ. *Third* Stirps with CHILOPODIFORM or SCOLOPENDRIFORM LARVÆ. *Fourth* Stirps with THYSANURIFORM LARVÆ.

Part III. *Fifth* Stirps of Lepidoptera Diurna, with ANOPLURIFORM LARVÆ.
Second Tribe of LEPIDOPTERA or SPHINGIDÆ.

Part IV. Third Tribe or BOMBYCIDÆ.

Part V. Fourth Tribe or NOCTUIDÆ.

Part VI. Fifth Tribe or PHALÆNIDÆ.

Each part will be illustrated by four plates consisting of highly finished engravings by artists of eminence. Three of these will be coloured with accuracy and elegance; the fourth, more elaborate as an engraving, will be given plain.

The first plate is devoted to the illustration of new *species*, and such subjects will be preferred as are typical of the groups defined in the progress of the work: they will be arranged, as far as possible, according to their affinities. For the second plate, those subjects will be chiefly selected which form types of *genera;* and they will, in most cases, be accompanied by dissections. On the third and fourth plates the history of the *metamorphosis* will be elucidated, and they will likewise contain additional generic illustrations and dissections.

As to the plan of the descriptive part, a very concise outline can only be given in this place. The arrangement proposed to be followed, and the constitution of the higher divisions, namely of *tribes* and *stirpes*, are explained in the Introduction. These are defined from a review of the whole order: but the sections indicated either in the stirpes or in the genera are provisional only, as they are regulated by the extent of the collection. A detailed generic character is given in the Latin language; this is followed immediately by a somewhat amplified description in English. Every species is distinguished by the generic and specific name at length. This is followed by a Latin description, in technical language, intended to exhibit a concise but accurate delineation, sufficiently minute to afford the means of precise discrimination from all other species. In the English description of new species the object is to give a full history of the external character in all its details. It is not consistent with Dr. Horsfield's present plan to give specific characters according to the Linnæan models: these belong, in his opinion, to works in which a general comparison of species contained in extensive collections, enables the writer to define the characters with a precision and confidence which cannot be obtained in the examination of a merely local collection.

The detailed specific descriptions will be followed, in most cases, by a series of miscellaneous observations. In these it is the author's chief object to illustrate the history of those individuals which he has traced through their various stages of existence, and of which the collection contains representations in their larva and pupa states. The arrangement projected for this work being founded primarily on the metamorphosis of the insects of this order, this part of the subject will be found to have an important bearing on the whole. These observations will also afford the necessary explanations of the figures contained in the third and fourth plates, and they will lead to the detail of the remarks made on the food of the larvæ, the season of the year when found, their abundance or scarcity, and to such other peculiarities as may have been noticed in Java. Under this head, he will also give an account of the state of the collection regarding the materials from which the descriptions have been made, with the view to illustrate many doubtful or imperfectly known species. The public or private collections in the British metropolis, in which the species described may have been observed, will also be indicated; and finally their range through other parts of India: and in the whole of these miscellaneous observations, as well as in the generic and specific descriptions,

scriptions, a principal object will be to render the work generally useful and interesting to the British naturalist.

The parts will follow each other with every degree of expedition consistent with the preservation of the style of publication in which the work has been commenced. Those preparatory arrangements which are inseparable from every undertaking of this nature have in some measure retarded the first part, but the publishers are enabled to engage, with every prospect of success, a regular continuation of the work: accordingly they announce the appearance of the *second* part early in July next, of the *third*, at the commencement of the ensuing year, and of the remaining parts at intervals of six months. According to this plan, the whole will be completed within three years from the commencement.

Lepidoptera Britannica; sistens Digestionem novam Insectorum Lepidopterorum quæ in Magnâ Britanniâ reperiuntur, larvarum pabulo, temporeque pascendi; expansione alarum; mensibusque volandi; synonymis atque locis observationibusque variis. Autore A. H. Haworth, *Linn. Soc. Lond. Soc., &c. &c.*

We congratulate the admirers of British Entomology on the long expected appearance of Part IV. of Mr. Haworth's *Lepidoptera Britannica*, which is at last completed.

This comprehensive and valuable work consists of 609 closely printed octavo pages, and 36 of Preface, and has been divided into four parts; to the first of which was appended above 200 similar pages, called *Miscellanea Naturalia.*

Complete new-wrought descriptions and synonymies of all the known British *Lepidoptera* are given throughout in Latin; together with occasional observations in English, respecting their peculiarities, size, food, times and places of appearance, &c.

The First Part, which was published by the author in 1803, contains the *Papilionidæ*, *Sphingidæ*, and *Bombycidæ*. After this, the work languished for want of encouragement, till the beginning of 1809, when the Second Part was published. It contains the *Hepiali*, *Lithosiæ*, *Falcariæ*, *Noctuadæ*, *Phalænadæ*, &c.

The Third Part appeared in 1811. It contains the *Pyralidæ*, *Tortricidæ*, &c. And the Fourth Part, which has just been published, completes the arduous and useful undertaking of its indefatigable author. It contains the *Tineadæ*, &c.; and concludes the work with a complete Index of all the genera and species of *Lepidoptera* described; about 1450 in number.

LX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 21 & 28.—READ An account of the accident to the packet-ship the New York, from lightning. By T. Stewart Traill, M.D. of Liverpool. Communicated by Henry Brougham, Esq. M.P. F.R.S.

The ship which met with the accident of which the effects are the subject of this communication, was the American packet the New York, of 526 tons, commanded by Captain Bennet. She sailed from New York for Liverpool, on the 16th of last April; and on the morning of the 19th was struck by lightning, which shattered the main royal mast, and gliding down the iron chain main-top-sail tie, burst the iron bands on the main-mast head. It was thence conducted by the iron main-top-sail sheets, to the iron work of the pumps. It then entered between decks, demolishing the bulkheads that formed the store-room, in its way to a small leaden cistern: whence it was conducted, by a leaden pipe, through the starboard side of the ship, where it started three five-inch planks, ten feet in length, at the lower part of the bends. Many other parts of the ship, not in the direct line of its passage, were also shattered, apparently from the effects of a lateral explosion; several doors and partitions were thrown down, a large mirror in the cabin was shivered into small fragments, and a piano-forte was thrown down, its top blown off and broken in pieces. The loudness of the explosion was appalling and spread universal consternation. A sulphureous smoke, which had issued with a bluish flame from the hatches, filled the cabins; and at first inspired alarm, lest the cargo in the hold, consisting chiefly of cotton and turpentine, had taken fire; but on clearing the main hatch, it was soon ascertained that no danger from fire existed. The ship however had sprung a leak which made four inches of water every hour, but which on washing the pumps was found to be under command, and would not prevent her proceeding on her voyage to England.

When the first terror created by the accident had somewhat subsided, it was found that none of the passengers or crew had sustained any injury. The chief mate was sleeping in the birth opposite to the main hatch, near the spot where the lightning entered the store-room, the lock of which was forcibly driven into his cabin: but he was not himself affected by the shock, and a quantity of gunpowder which was kept under his bed was fortunately not ignited by the lightning. An ewer and a basin placed on a stand over a child's bed were thrown down by the explosion, but the child had escaped unhurt. A remarkable effect was however produced on an elderly gentleman, who for the last five years had not been able to walk half-a-mile at a time: terrified by the crash, he forgot his debility, and springing from his bed, rushed on deck with singular quickness and agility. He has retained, ever since the event, the power over the muscles of his limbs, derived from this sudden emotion.

The threatening aspect of the heavens, the appearance of numerous water-spouts on the surface of the sea, and other electrical indications, gave rise to apprehensions of further danger, and induced the captain to put up the conductor with which he was provided, but which had not been previously applied. It was made of iron links eighteen inches long, connected by iron rings one inch in diameter; and was furnished at the top with an iron rod four feet long and half an inch in diameter, tapering to a fine point. This rod was fixed so as to rise three feet above the main royal mast head; and the chain was made to descend along the back-stay, and below was kept at a distance of ten feet from the starboard bulwarks by a light wooden outrigger, or spar. Its whole length was 145 feet, of which about nine feet of its lower part descended into the sea. The wisdom of adopting this precaution was soon apparent, for in the course of the same morning the ship was struck by a second explosion, which is stated by the unanimous testimony of all on board to have far exceeded in violence the first. It melted a great part of the conductor, producing a vivid combustion of many of the links, which burned like so many tapers; and descending into the sea, darted off to a considerable distance along the surface of the waves. The resistance to its passage was so great as to cause the ship to recoil with a sudden and violent shock, so as to throw down several of the crew. The melted iron of the conductor fell in large drops on the deck, which although already strewed with hailstones that had previously fallen, intermixed with rain, was set fire to in many places by the ignited metal. No damage, however, was done to the masts or rigging, not the least injury to any of the crew, with the exception of a carpenter, who being at work with an iron auger in his hand, received a smart shock through the wrists, which occasioned a livid tumour which was still visible six weeks after the accident.

Soon after the arrival of the vessel in Liverpool, she was docked in order to ascertain what damage she had sustained. Some of her planks were found to have started, but her timbers were uninjured. Every instrument made of steel,—such as the carpenter's tools, and the knives and forks; and also those made of soft iron, even to the very nails in every part of the ship,—had been rendered permanently magnetic. All the watches and chronometers were either stopped or rendered useless, by the magnetism imparted to the balance-wheels and other parts of their works that were made of steel. Contrary to what usually happens from shocks of artificial electricity, the lightning had given a strong northern polarity to the upper part of the conductor. Many parts of the iron-work indeed had acquired the magnetism corresponding to their position with respect to the magnetic direction; but in others no relation of this kind could be traced. Great changes were produced on the magnetism of the compass needles, in many of which were formed several sets of poles, and their indications could therefore no longer be relied on.

The circumstances attending the accident which is the subject of this paper, are considered by the author as strongly confirming the value of conductors to ships in obviating the destructive effects of lightning. From the inquiries he has made, he is led to the belief that injuries from lightning at sea are much more frequent than is generally imagined. One source of increased danger of late years is to be found in the greater proportion of metal, and particularly of iron, which is employed in the rigging; more especially as the metallic masses are there nearly insulated, or connected only by very imperfect conductors. In the instance before us, it is in the highest degree probable, that if the New York had been without the protection of the conductor, she must inevitably have been destroyed by the second tremendous explosion, which, thus guarded, she sustained without the slightest injury. The author remarks that copper is a better material for such a conductor than iron, from its being less liable either to fusion or corrosion: and also that a rod is, from its continuity, a better form of conductor than a chain. In the case of ships, however, the greater convenience of a chain, arising from its flexibility, will generally insure it the preference. The author recommends, that, instead of carrying the conductor through the decks to the keel, as suggested by Mr. Harris, the lower end of the chain should be kept at a distance from the sides of the ship, by means of a light outrigger, or spar, as was done in the New York.

March 20.—Read a paper On the Phænomena of Volcanoes; by Sir H. Davy, Bart. F.R.S.

In a paper on the decomposition of the earths, published in the Phil. Trans. for 1812, the author offered it as a conjecture, that the metals of the alkalies and earths might exist in the interior of the globe; and on being exposed to the action of air and water, give rise to volcanic fires, and to the production of lavas, by the slow cooling of which basaltic and other crystalline rocks might subsequently be formed. Vesuvius, from local circumstances, presents peculiar advantages for investigating the truth of this hypothesis; and of these the author availed himself during his residence at Naples, in the months of December 1819, and of January and February 1820. A small eruption had taken place a few days before he visited the mountain, and a stream of lava was then flowing with considerable activity from an aperture in the mountain a little below the crater, which was throwing up showers of red-hot stones every two or three minutes. On its issuing from the mountain, it was perfectly fluid, and nearly white hot: its surface appeared to be in violent agitation from the bursting of numerous bubbles, which emitted clouds of white smoke. There was no appearance of vivid ignition in the lava when it was raised and poured out by an iron ladle. A portion was thrown into a glass bottle, which was then closed with a ground stopper, and, on examining the air in the bottle some time afterwards, it was found not to have lost any of its oxygen. Nitre thrown upon the surface of the lava did not produce such an increase of ignition as would have attended the presence of combustible matter. The gas disengaged from the lava proved on examination, to be common air.

When the white vapours were condensed on a cold tin plate, the deposit was found to consist of very pure common salt; and the vapours themselves contained nine per cent of oxygen, the rest being azote, without any notable proportion of carbonic acid or sulphureous acid gases; although the fumes of the latter of these gases were exceedingly pungent in the smoke from the crater of the volcano. On another occasion, the author examined the saline incrustations in the rocks near the ancient bocca of Vesuvius; and found them to consist principally of common salt, with some chloride of iron, a little sulphate of soda, and a still smaller quantity of sulphate or muriate of potassa, with a minute portion of oxide of copper. In one instance, in which the crystals had a purplish tint, a trace of muriate of cobalt was detected. From the observations made by the author at different periods, he concludes that the dense white smoke which rose in immense columns from the stream of lava, and which reflected the morning and evening light of the purest tints of red and orange, was produced by the salts which were sublimed with the steam. It presented a striking contrast to the black smoke arising from the crater, which was loaded with earthy particles, and which in the night was highly luminous at the moment of the explosion The phænomena observed by the author afford a sufficient refutation of all the ancient hypotheses, in which volcanic fires were ascribed to such chemical causes as the combustion of mineral coal, or the action of sulphur upon iron; and are perfectly consistent with the supposition of their depending upon the oxidation of the metals of the earths upon an extensive scale, in immense subterranean cavities, to which water or atmospheric air may occasionally have access. The subterranean thunder heard at great distances under Vesuvius, prior to an eruption, indicates the vast extent of these cavities; and the existence of a subterranean communication between the Solfatarra and Vesuvius, is established by the fact that whenever the latter is in an active state, the former is comparatively tranquil. In confirmation of these views, the author remarks, that almost all the volcanoes of considerable magnitude in the old world, are in the vicinity of the sea: and in those where the sea is more distant, as in the volcanoes of South America, the water may be supplied from great subterranean lakes; for Humboldt states that some of them throw up quantities of fish. The author acknowledges, however, that the hypothesis of the nucleus of the globe being composed of matter liquefied by heat, offers a still more simple solution of the phænomena of volcanic fires.

LINNÆAN SOCIETY.

April 1.—Lord Stanley in the chair.

His Lordship opened the meeting of the Society by adverting, with much feeling, to the great loss which had been sustained by the country and by the world, and more especially by the Society, in the death of its illustrious and beloved President Sir James Edward Smith, who from its first establishment, in which he had taken an active part, had been called upon to preside over it by the annual and unanimous votes of its members, and had greatly contributed to place the Society in the distinguished rank which it had attained, by his great talents, indefatigable industry, sound judgement, and enlarged views as a naturalist;—by the high estimation in which he had long been held by men of science all over the world; by the excellence of those valuable and accurate works in which he had done so much to promote and improve the study of natural history; and especially by the qualities of his heart, mind, and temper, for which his memory would long be revered by those who had enjoyed the happiness of his friendship.—He could not forbear expressing what he felt on the present occasion, especially with reference to the particular moment of his loss,—at a time when those considerations of religious distinction were about to be removed, which had seemed to have a tendency to deprive those who, like this excellent and distinguished man, differed from the established religion, of the rank in society due to their talents or their worth*.

His Lordship expressed his anxiety that whatever choice might be made by the Society to fill the vacancy in its Chair, should be such as would contribute to its prosperity, however impossible it might be adequately to supply the loss which it had now so much to regret.

Lord Stanley then adverting to the last volume of the English Flora, which had been received from Sir James Smith but a few days before his death, and was among the presents on the table, related that, showing it to a friend, he had exclaimed, "This is the close of my labours."—As its distinguished author was now removed from the possibility of receiving the customary vote of thanks, His Lordship concluded, by proposing that the grateful feelings of the Society might be expressed to Lady Smith for this last gift of their revered President.

A portion of Dr. F. Hamilton's Commentary on the *Hortus Malabaricus*, Part V., was then read.

April 15.—Read a letter addressed to the Secretary, from Charles Lucien Buonaparte, Prince of Musignano, F.M.L S., dated on board the Delaware, near Gibraltar, March 20th, 1828, containing the following notice relative to the migration of certain birds.—"A few days ago, being 500 miles from the coasts of Portugal, 400 from those of Africa &c., we were agreeably surprised by the appearance of a few swallows, *Hirundo urbica* and *rustica*. This, however extraordinary, might have been explained by an easterly gale, which had cut off the swallows migrating from the Main to Madeira, only 200 miles distant from us;—but what was my surprise on observing several small warblers hopping about the deck and rigging! These poor little strangers, exhausted as they were, were soon caught and brought to me. The following short list is that of the species.

"1. *Sylvia trochilus.*—2. *Sylvia erithacus* (*tethys*, Temminck).—3. *Sylvia suecica,* or rather a similar species, which I have already received from Egypt and Barbary.—4. A species new for Europe and perhaps even a nondescript, having the plumage of an *Anthus*, and which I think belongs (as *Sylvia cisticola* and others) to the *hitherto African* genus *Malurus*. This, however, must rest undecided, my specimen missing its tail, which was pulled off by the sailor who caught the bird."

A communication was likewise read to the Society from J. Morgan, Esq. F.L.S., relative to the structure of the mammary organs of the Kangaroo, in which a detailed account was given of a recent dissection of these parts, both in the virgin and in the impregnated animal; together with the author's opinions respecting the physiology of certain structures which have been hitherto unnoticed, and of others which have been incorrectly or imperfectly described by former investigators of this interesting branch of natural science.

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* Alluding to the proceedings for the abolition of the sacramental test,—Sir James Smith having been a member of the congregation of Unitarian dissenters at Norwich.

The author first pointed out the anatomical peculiarities which he had discovered in a dissection of the pouch and mammæ of a young and unimpregnated kangaroo; by which it appears, that, in the virgin state, the two upper nipples only are found to be developed, and that beneath each of these a minute circular aperture, resembling in appearance the mouth of a follicle, occupies the exact situation in which the lower teat is known to exist in the adult impregnated animal.—The mammæ are described as consisting of double glandular structures on each side; they are situated directly behind the follicular openings already mentioned, and are closely confined to the posterior surface of the integuments. Each double mamma is formed by an upper and smaller gland, which is attached by its excretory ducts to the already developed nipple, and by a second and larger glandular substance from which no excretory duct could be traced. The follicular apertures which occupy in the pouch the situation of the lower teats, form the external openings of cylindrical membranous canals which lie imbedded in the substance of the larger and lower mammary glands. Each of these membranous canals or tubes is about three-fourths of an inch in length, and extends through nearly the whole diameter of the larger gland which encloses it; the interior of the tube is lined by cuticle, and the internal extremity is terminated by a rounded papilla which projects into its cavity.

In these papillæ the author found a perfect miniature resemblance to the extremities of the lower teats in the adult animal, which teats he considers to be formed, *during the first gestation*, by the complete eversion of the membranous canals, and the consequent projection of their papillary terminations. He further states, that by artificially everting the parts, two perfect teats are produced in the precise situation of those which are found in after life. It has been however ascertained that this extraordinary change occurs only during the first gestation, since after being once developed the teat remains permanently formed and projected.

Having thus described the condition of the mammary glands and the development of the lower teats in the virgin animal, the author gave a particular account of his dissection of an adult female, which at the time of death was suckling a young one nearly half-grown. In this we are informed that the panniculus carnosus which covers the anterior surface of the belly is of extraordinary thickness, and composed principally of perpendicular muscular fibres, which in their course from the thorax downwards surround the mouth of the pouch to which they form a sphincter muscle, and that a fasciculus of its fibres descending over the symphysis pubis is inserted into the sphincter muscle of the cloaca; so that the contraction of this part of the panniculus carnosus would powerfully operate in approximating the external aperture of the vagina with the mouth of the pouch.

On removing the panniculus carnosus a pair of muscles (of which the attachments and uses have been hitherto incorrectly described) are brought into view. Each of these muscles is of a triangular shape, being attached by a narrow origin to the posterior part of the pelvis; and expanding in its course, is continued transversely round the lower part of the belly, before the abdominal muscles, and immediately above the brim of the pelvis. Each of these triangular muscles encloses, between an anterior and posterior layer of its fibres, the mammary gland, and the two muscles afterwards cross the fore part of the abdomen to unite in front of the linea alba. By this union a perfect muscular girdle is formed, by the contraction of which the mammæ are compressed against that part of the abdomen in which the marsupial bones lie imbedded.

The conclusion of this paper, containing further particulars of the dissection of the mammary organs as well as of the muscles attached to the marsupial bones of the adult and impregnated animal, together with the author's opinion respecting their physiology, remains to be read at a future meeting.

ASTRONOMICAL SOCIETY.

Feb. 8.—This day being the Anniversary, Dr. O. G. Gregory, one of the Secretaries, read the following Report of the Council to the Eighth Annual General Meeting.

Your Council are happy in being able to commence this, their Eighth Report, with a general expression of congratulation on the progress of the Society, and on the prospects of increased and increasing utility, which a view of its actual state and resources, and a consideration of the events of the year now closed, in their minds appear to justify. They trust, too, that a review of these events will at the same time justify the confidence reposed in them, by showing that they have not been neglectful of the opportunities which have offered of advancing in various ways the objects and interests of the, Society.

One of the first acts of the Council of the year elapsed, was to enter into an arrangement with Mr. Taylor, the printer to the Society, and who is also one of the editors of the Philosophical Magazine, for the publication of a series of monthly notices of its Proceedings, and for the supply of a sufficient number of copies of them, in succession, for distribution among the members. The convenience and advantages of this plan have been sufficiently proved by the trial which has been given it, and it will of course be continued. The public is hereby brought more immediately into contact with the Society—the labours of its contributors are canvassed and discussed, while the interest of the author in his subject is yet warm, and when the interchange of ideas respecting it is most beneficial, not only to the public, but to the author himself, whose views may, and probably in many instances will, be enlarged or corrected by such intercourse. An authentic and at the same time public record is, as it were, opened, of the papers read, and the outlines of their contents rendered matter of history;—thus affording ready means of establishing the claims of

authors

authors to priority of discovery, so far at least as priority of public communication can be regarded as evidence in questions of that nature. The same arrangement offers the further advantage of relieving the Council from all difficulty in the disposal of matter of merely temporary interest; such as notices of phænomena, ephemerides of the smaller planets, comets, &c. which require speedy circulation, and do not need to be formally enrolled in the *Memoirs*; and it may occasionally happen, that the matter of more regular communications may so far be condensed into the monthly abstract, as to dispense with a second publication in the Memoirs—to the material relief of the funds of the Society.

The same consideration of the advantages derived from the speedy publication of communications read to the Society, has induced the Council to adopt the principle of sending immediately to press all papers which, on passing through the prescribed formalities, shall be deemed of a nature for publication in their Memoirs; and although objections have been considered to exist against the separate publication of each particular memoir, the division of their volumes into smaller parts can be attended with no inconvenience. Acting on this principle, the Council have directed the publication of Part I. Vol. 3. of the Memoirs, including all papers (regarded by them as suitable for printing in the body of the Memoirs) read during the interval elapsed from the last publication. This Part is now, accordingly, before the public, and furnishes satisfactory proofs of the zeal, diligence, and talents, both of our Home Members and our Associates.

It is not merely, however, in communicating to the world the observations of their members, but also in causing observations to be made, and in lending every assistance in their power to those meritorious and public-spirited individuals, who, actuated only by an earnest desire to render available their leisure and talents in the cause of science, are willing to undertake the task of regular observation,—that a Society like this can render service to science. The munificent act of a private individual, has happily placed this particular line of utility more directly than heretofore in the power of the Society; and its members have not been slow in availing themselves of its advantages, and applying them to effective use.—Among the great and lamented losses which the Society has sustained in the course of the last year, is that of the late Colonel Mark Beaufoy; the latter days of whose existence we recollect with a melancholy pleasure to have been cheered and gratified by the highest mark of this Society's approbation, in the award of their medal for his Astronomical Observations. His son, Lieut. George Beaufoy, has, with the utmost liberality, placed his deceased father's astronomical instruments in the possession of this Society. They consist of

One 4-feet Transit, by the late Mr. Cary, Strand.
One Altitude and Azimuth Circle, by the same.
One Clock, adjusted to mean Solar time.
One Clock, adjusted to Sidereal time.

Your Council conceive, that the opportunity fortunately placed before

fore them, by Lieut. Beaufoy's recent election as a member of this Society, of marking their sense of this liberal and public-spirited conduct on his part, by a high and complimentary distinction, ought not to be neglected. They have therefore thought it adviseable to recommend to the Society to elect him a member for life.

The Council wish to observe, with respect to the measure so recommended, that though it may seem equivalent to the acknowledgement of a class of members similar to that, which in the constitutions of some scientific bodies are deemed honorary members,—yet they are rather desirous to avoid the establishment of purely complimentary distinctions, and to mark by it, in this and any future case which may occur, the Society's sense of some distinct benefit accruing to itself or to astronomical science, through the individual so distinguished, which could not be properly acknowledged otherwise:—thus leaving every future case to rest on its own individual merits, as if it were the first of its kind.

The surest criterion of the utility of a donation is its immediate and effective practical application. That of Lieut. Beaufoy was scarcely announced to the Council, when an application was made to them by one of our members, Captain Smyth, R.N. (justly distinguished for his knowledge of the resources of practical astronomy), for their loan, which was immediately accorded; and the Council have the high satisfaction of being able to announce to you, that the instruments in question are at this moment (with the exception of one of the clocks) mounted in the best manner, in a regular observatory established by Captain Smyth, at his residence at Bedford, for their express reception, and already in actual use in celestial observation. The Council, though not unaware of the general nature of Captain Smyth's astronomical views, purposely forbear from publicly stating at present the course of observations in which he purposes to engage; being desirous to leave his meritorious exertions as far unfettered as possible by any public pledge—and trusting rather to his high character and well-known zeal, talent, and activity, than to any express stipulation, that the means thus placed in his hands will be exerted for the advancement of astronomical science.

Actuated by a similar desire to promote, as far as our means will admit, the great objects of astronomical inquiry in all its branches, the Council have ordered two invariable pendulums of iron and of copper, on a construction somewhat different from those hitherto employed, to be consigned to Captain Foster, for the purpose of investigating the possible effects of the earth's magnetism in various geographical positions, in these delicate researches, in the course of his approaching scientific voyage; an enterprise which honourably characterizes the enlightened views of Government, and from which a rich harvest of important results may be anticipated.

From the report of the Finance Committee it appears that the funds of the Society continue in a flourishing state, notwithstanding the heavy demands for computation and printing during the last year. This extraordinary expenditure has principally arisen from the great expense incurred in publishing the Catalogue of the principal fixed stars, alluded to in the last Report, and which appears to have met the approbation of every astronomer. But the Council are happy to state, that this disbursement has been followed by a corresponding spirit of zeal and liberality on the part of several of the members, who, by entering into a subscription, have enabled the Council to defray nearly the whole expense of that useful and valuable work, without intrenching on the ordinary funds of the Society. And it may be mentioned, as a proof of the liberal spirit which animated those members, that the sum of 320*l.* was subscribed, within a few hours, towards the expense of publishing the Catalogue above alluded to.

The Council cannot too earnestly impress on the attention of the members, the necessity of encouraging the sale of the Memoirs as much as possible: since it is only by a quick return, arising from such sale, that they can expect to continue the publication, from year to year, without a serious diminution of the funds of the Society.

Among the accessions to our home list of members, in the year elapsed, the Council have to record, with all those sentiments of respect which his illustrious rank so peculiarly inspires, the name of His Royal Highness the Lord High Admiral: and they trust that this accession, so honourable in itself, and so gratifying to the Society, will be attended with the most beneficial results to the science which this Society was formed to promote, and which is so intimately connected in its practical application with the department over which His Royal Highness presides. Our home list has been further increased by the accession of fourteen new members, and our foreign, by that of one associate.

Among the losses sustained by death, the Society has to lament, in addition to Colonel Beaufoy, that of its most illustrious associate Laplace: and on our home list, of the Rev. Lewis Evans; His Grace the Duke of Gordon; our amiable and excellent trustee, Mr. Daniel Moore (whose loss will be felt far beyond the limits of this body by many, as the privation of a benefactor, in whose ears the calls of distress never sounded in vain); Mr. W. M. Mosely; and Mr. J. Sanders.

Mr. Mosely, in addition to a competent knowledge of various sciences, had turned much of his attention, in the latter part of his life, to astronomy. He possessed several valuable instruments, and is said to have left behind him a series of observations of transits and north polar distances, and some measures of double stars confirmatory of their changes.

Mr. Evans also possessed instruments of considerable merit, and for several years employed himself as a skilful and successful observer.

It will not be expected that any sketch should here be attempted of the labours of M. de Laplace. No history of his scientific life, of the origin and development of his views, adequate to the occasion, could be comprised in such limits as those of this Report: and the general nature of the more important results of his researches is already so well known, and so incorporated with the intellectual history of the last half century, that it constitutes a portion of the knowledge of every well informed man. During the long period of fifty-five years, from the date of his first considerable mathematical production on the integration of equations of differences, in the *Turin Memoirs* in 1772, to almost the moment of his death, his march was in the van of intellect; and the highest point of scientific attainment to which the age had reached, was uniformly marked by his progress. His whole career was a succession of brilliant and profound discoveries, where every great step in the theoretical departments of analytical science was sure to be attended with a corresponding advance in its practical application; and every difficulty which occurred in applying his principles to the sublime problems of physical astronomy, served only to give rise to new methods, and create more powerful engines of mathematical inquiry; a rare combination,—of which Archimedes and Newton had afforded the only previous examples,—of a philosophical spirit of the first rank wielding all those unbounded resources of abstract science, which a few extraordinary men of a different turn may have perhaps possessed in an equal or superior degree, but which only attain their highest value when exerted from the vantage-ground of a mind at home in every department of experimental philosophy, impressed with the fullest sense of the importance of practical application, and familiar with all the means of disentangling principles from natural phænomena. Laplace also afforded a conspicuous instance of that union of gentle and amiable social qualities, which is one of the best characteristics of the highest order of genius—as indicating a mind secure of its rank. No pretension—no assumption—nothing dictatorial in science or offensive in taste, marked his mild and modest deportment. They, who have heard his sublimest views propounded with the diffidence of a youth seeking information; they, whose early scientific attempts have been encouraged, and whose maturer efforts were assisted and directed by his talents,—will long retain a penetrating sense of this estimable feature of his character.

The only one of the prize questions proposed at the instance of the Council in February 1824, the period of which remained unexpired during the year elapsed, is that relating to the development of the differential equations of the lunar theory, and the improvement of the lunar tables. No answer has been received to this question; and the proposed period has now expired. The Council have not thought it adviseable either to prolong this period, or to propose any new questions for the present or future years: but, should subsequent researches either afford complete solutions of any of those questions, or material elucidations of their peculiar difficulties, future Councils will not fail to bear in mind the importance that attached to their subjects in the minds of former ones, and recommend to the Society the accordance of such marks of their approbation as the degree of progress made in them may appear to demand.

Two medals have been awarded this year by the Council. One to Sir Thomas Macdougal Brisbane for the inestimable benefit conferred by him on astronomical science, in the establishment of his observatory at Paramatta in New South Wales, and for the valuable and important series of observations made there by himself, and under his directions, during his residence as governor of that colony. The other to Mr. James Dunlop, for his disinterested and indefatigable pursuit of astronomical researches, subsequent to the departure of Sir T. M. Brisbane from the colony of New South Wales, whereby he has added, in a most material degree, to our knowledge of the nebulæ of the southern hemisphere.

These medals will be delivered to these gentlemen respectively, or to their proxies, by the President, before the Society proceed to the business of election of the officers for the ensuing year.

The Council cannot conclude this Report, without an earnest exhortation to the members of the Society, collectively and individually, to cooperate by their active exertions in the great cause for which the Institution exists. They entreat them to remember that every one who possesses an instrument, whose claims rise even not above a humble mediocrity, has it in his power to chalk out for himself a useful and honourable line of occupation for leisure hours, in which his labour shall be really valuable, if duly registered: that those who possess *good* instruments, have a field absolutely boundless for their exertions. To such they would hold out the brilliant examples of many other of the members and associates of this Society,—of a Bessel, a Struve,—as showing what may be accomplished by persevering industry, and how little reason there is to apprehend the failure of matter for their researches. They would strongly impress on the minds of all observers, however, whether private or public, the daily increasing necessity which exists for the *reduction* of their observations when made, and for the employment for that purpose of a uniform system of corrections adopted by common consent: and that, not when the work of many years shall have accumulated on hand, till it shall have become overwhelming; but regularly, year by year—nay, month by month, while the immediate interest of the observations retains its freshness in the memory, and while their actual value can be checked, and the means of further refinement suggested and attained. The extreme facility afforded for these reductions by the Catalogue published by this Society, leaves no excuse for their neglect, when stars contained in it are observed; and it is most earnestly to be hoped, that no departure in other cases, from the coefficients there used, will take place.

The Council would also strongly draw the attention of the members of the Society to the high importance, both for nautical and astronomical science, of observations of occultations, eclipses of the sun, and satellites of Jupiter, moon-culminating stars, and even ordinary lunar distances well and carefully observed on land, at fixed stations, such as observatories, and at all remarkable points in distant countries, where opportunities may offer. The Society will feel its utility most materially increased by the communication of such observations, if carefully made. No class of individuals have so frequent and available opportunities for such observations, as naval officers, nor to any is the object of such direct utility.—It is gratifying to know, that the requisite scientific attainments for such researches are already widely diffused among them, and daily becoming more so; and it is cheering to indulge the pleasing reflection, that the calls thus made upon them will not be unavailing.

(*The President then addressed the meeting on the subject of the award of the Medals, as follows :—*)

Gentlemen,—In pursuance of the award of your Council, which you have just heard, I have now to call your attention to the subject of the honorary marks of this Society's approbation, which it is part of our business at this meeting to bestow. The selection of objects on which such distinction may most deservingly and most usefully be conferred, has been, in this instance, of much interest and some difficulty,—not from a paucity of claims, but from their variety and magnitude. On all sides, both abroad and at home, the spirit of Astronomical research and discovery has been diligently alive. The great work which has been commenced on the Continent, for the determination of the places of all the stars of our hemisphere in zones, has been continued with a patient ardour to which no words can do justice.—The heavens have been ransacked for double stars; and the results of the search, developing a most rich and unlooked-for harvest of striking discoveries, being the first fruits of the great telescope of Fraunhofer, have been consigned to immortality, in a work which does honour to its age and nation, and which has already been brilliantly rewarded in another quarter. The ingenuity of one of our own countrymen, has placed new, simple and powerful means in the hands of observers. for verifying the stability of their instruments, and determining their fluctuations. And in every quarter, to go no further in this detail, an activity worthy of the high ends and dignity of our science, has been remarkably displayed. Among so many important labours, however, some of which are yet awaiting their final completion, or receiving the last touches of their authors, the attention of your Council has been fixed, by the imposing mass of valuable observations which has emanated during a series of years, from the Observatory at Paramatta, established by the late governor of the colony of New South Wales, Sir Thomas Macdougal Brisbane, one of our Vice-Presidents, long distinguished among us by his ardent love of Astronomy, and an intimate familiarity both with its theory and practice.

Nothing can be more interesting in the eyes of an European astronomer, especially to those whose field of research, like our own, is limited by a considerable northern latitude,—than the southern hemisphere, where a new heaven, as well as a new earth, is offered to his speculations; and where the distance, the novelty, and the grandeur of the scenes thus laid open to human inquiry, lend a character almost romantic to their pursuit.

A celestial surface equal to a fourth part of the whole area of the heavens, which is here for ever concealed from our sight, or whose extreme borders at least, if visible, are only feebly seen through the smoky vapours of our horizon,—affords to our antipodes the splendid prospect of constellations different from ours, and excelling them in brilliancy and richness. The vivid beauty of the Southern Cross has been sung by poets, and celebrated by the pen of the most accomplished of civilized travellers; and the shadowy lustre of the Magellanic clouds, has supplied imagery for the dim and doubtful mythology of the most barbarous nations upon earth. But it is the task of the Astronomer to open up these treasures of the southern sky, and display to mankind their secret and intimate relations. Apart, however, from speculative considerations, a perfect knowledge of the astronomy of the southern hemisphere is becoming daily an object of greater practical interest, now that civilization and intercourse are rapidly spreading through those distant regions,—that our own colonies are rising into importance,—and that the vast countries of South America are gradually assuming a station in the list of nations, corresponding with their extent and natural advantages. It is no longer possible to remain content with the limited and inaccurate knowledge we have hitherto possessed of southern stars, now that we have a new geography to create, and latitudes and longitudes without end, to determine by their aid. The advantages too, to be obtained, even for the perfect and refined astronomy of the north, by placing nearly a diameter of the globe, between the stations of observatories, and taking up the objects common to both hemispheres, in a point of view, and under circumstances so every way opposite to those which exist here, have been strongly pointed out by a venerable and illustrious member of this Society, in an elaborate paper published in its Memoirs, and would alone suffice to justify a high degree of interest, as due to every well conducted series of observations from that quarter. The observations of Halley at St. Helena, had made known the places of a moderate number of the brighter southern stars; but the only catalogue of any extent and accuracy, which existed previous to the establishment of the observatories of the Cape and Paramatta, was that of Lacaille, who spent three years at the Cape of Good Hope, and the Isles of France and Bourbon; and, though with very inadequate instrumental means, yet, by dint of the most indefatigable industry, succeeded in observing and registering upwards of 10,000 stars. But by far the greater part of these observations have never been reduced; a selection only from them of 1942 of the principal ones, not amounting to a fifth of their whole number, having been formed into a catalogue, and published by this meritorious astronomer. It must be admitted, however, that the degree of accuracy stated by Lacaille himself to have been probably attained by him, is hardly such as to make us now very deeply regret their want of reduction, especially as the observations themselves are printed with every requisite for that purpose, when required. Still, however, from his method of observing, which was with a fixed telescope and rhomboidal network, his observations have what may be termed a dormant value, as they most probably give correct differences for each night's work; and when a catalogue of standard southern stars shall be completed, Lacaille's observations will become available, by regarding these as zero points, and referring all the rest to them.

Such was nearly, with little improvement, the state of the astronomy

nomy of the southern hemisphere, when Sir Thomas Brisbane was appointed governor of the Colony of New South Wales. The intention of our Government to found an observatory on the largest scale, at the Cape of Good Hope, was, indeed, already fixed; and the observer, a member of this Society, supplied with instruments sufficient for the purpose of constructing a preliminary catalogue, occupied himself with the necessary observations, while awaiting the arrival of those ultimately destined to adorn that establishment, and the building of his observatory. The approximate catalogue so constructed and reduced, containing all the southern stars observed by Lacaille, down to the 5th magnitude, is already printed by the Royal Society in their Transactions.

Sir Thomas Brisbane's attachment to Astronomy had ever been a prevailing principle of his mind, and one which even amidst the distractions of a military life of no ordinary degree of activity and adventure, he found means to indulge; and which never deserted him, however the calls of his country might demand his services in a different and more splendid career.

His appointment to the important office of governor of New South Wales, however, put it in his power to execute to their fullest extent and under the most favourable circumstances, plans of astronomical investigation, which to a private individual would have been utterly impracticable. The opportunity was embraced with eagerness. The best instruments,—consisting of an excellent transit of 5½ feet focal length, by Troughton; a mural circle of two feet in diameter, the workmanship also of Troughton, and said to have been the model on which that of Greenwich was constructed, and which had long been in his possession; and a fine 16-inch repeating circle of Reichenbach,—were destined for this service: and two gentlemen engaged as assistants at considerable salaries; the one a foreigner of high estimation as a mathematician and calculator, the other Mr. Dunlop, of whom I shall presently have occasion to say much more. It ought to be mentioned, that this noble equipage was furnished entirely from Sir Thomas's private fortune, and maintained wholly at his own expense. Immediately on his arrival in the colony in 1821, and so soon as an observatory could be erected, and the instruments established, the work of observation commenced, and continued with little interruption under the immediate superintendence and direction of Sir Thomas Brisbane himself, who, though the pressing and important duties of his high office would of necessity seldom admit of his devoting any material proportion of his time to actual observation, yet frequently took a personal share in the labours of the observatory, as a relaxation from higher duties, and in particular, a great portion of the transits were observed by himself.

The first fruits of this enterprise, were the observations of the December solstice of 1821, which were published in the Astronomical Notices of Schumacher; in which work also appear those of both the solstices of 1822, and a number of detached and occasional observations, which reached Europe at different times by a variety of channels, and found their way into that valuable collection. The solstices of 1823, were communicated by Sir Thomas Brisbane to this Society, in a letter to our late worthy president, together with a considerably extensive series of observations of principal stars, chiefly those visible in both hemispheres, and which have undergone a careful reduction and close scrutiny in the hands of Dr. Brinkley, the details of which, as well as the original observations, are printed in the first part of the second volume of the Transactions of this Society, and which justify, in the eyes of that experienced observer, as they must in those of every practical astronomer, a decided opinion of the great care and skill with which they have been made.

A great number of occasional observations,—such as eclipses, occultations, and observations of the planets Venus and Uranus, near their conjunctions and oppositions, and of comets from the same source,—are also printed in the same volume. One of the most remarkable single results we owe to the establishment of Sir Thomas Brisbane's observatory, consists in the re-discovery of the comet of Encke in its predicted place, on the 2d June 1822. The history of this extraordinary body is well known to all who hear me; and as its re-discovery at Paramatta by Mr. Rümker, has already been, on a former occasion, distinctly noticed and rewarded by this Society, there is no occasion that I should here enlarge on it; and yet I cannot help pausing a moment to figure the delight its celebrated discoverer must have experienced, to find the calculations, on whose exactness he had pledged himself, thus verified beyond the gaze of European eyes; and this strange visitant gliding, as if anxious to elude pursuit, into its primitive obscurity, thus arrested on the very eve of its escape, and held up to mankind,—a trophy at once of the certainty of our theories, and the progress of our civilization.

Observations of the length of the pendulum were not neglected by Sir Thomas Brisbane; and the determination of this important element at Paramatta, forms the subject of a highly interesting and valuable communication made by him to the Royal Society, and printed by them in their Transactions for 1823, and discussed by Captain Kater with his usual care and exactness.

The remainder, and indeed the great mass of the observations made with the mural circle and the transit instrument, have at different periods been communicated to the Royal Society, and are for the present deposited in its archives. Forming our judgement only upon those of which an account has been publicly read at the meetings of that illustrious body, but which are understood to constitute only a comparatively small part of the whole,—they form one of the most interesting and important series which has ever been made, and must ever be regarded as marking a decided æra in the history of Southern Astronomy.

It is for this long catalogue of observations—whether scattered through the journals of Europe, printed in our own Transactions, or deposited as a precious charge in the care of a body so capable of appreciating their merits; but still more for the noble and disinterested example set by him in the establishment of an observatory on such a scale, in so distant a station, and which would have equally merited the present notice, had every observation perished in its conveyance home—that your Council have thought Sir Thomas Macdougal Brisbane deserving the distinction of a medal of this Society, which, as he is unable personally to attend this meeting, I will now deliver to his proxy, Mr. South.

Mr. SOUTH,—We request you to transmit to Sir Thomas Brisbane this medal, accompanied with the strongest expressions of our admiration of the patriotic and princely support he has given to Astronomy, in regions so remote. It will be a source of honest pride to him while he lives, to reflect that the first brilliant trait of Australian history marks the æra of his government, and that his name will be identified with the future glories of that colony in ages yet to come, as the founder of her science. It is a distinction truly worthy of a British governor. The colonial acquisitions of other countries have been but too frequently wrested from unoffending inhabitants, and the first pages of their history blackened by ferocious conquests and tyrannical violence. The treasures of gold and silver they have yielded—the fruits of rapine—have proved the bane of those who gathered them; and in return, ignorance and bigotry have been the boons bestowed on them by their parent nations. Here, however, is a brighter prospect. Our first triumphs in those fair climes have been the peaceful ones of science; and the treasures they have transmitted to us, are imperishable records of useful knowledge, speedily to be returned with interest, to the improvement of their condition and their elevation in the scale of nations.

(*The President then resumed his address to the Members, as follows:*—)

I have now to call your attention, Gentlemen, to the award of another Medal, to Mr. Dunlop, who accompanied Sir Thomas Brisbane in capacity of his assistant, and who, since the middle of the year 1823, when his companion Mr. Rümker left the observatory, remained in the sole charge of the instruments; and up to the period of the departure of his principal from the colony, continued an uninterrupted series of observations with a care and diligence seldom equalled, and never surpassed. In such cases it is not only the head which plans, but the hand which faithfully and promptly executes, that claims our applause. The most liberal provision of instrumental means would have been comparatively unavailing, had the spirit of him who supplied them, been seconded by any ordinary zeal on the part of his assistants. The records of this Society already alluded to, bear sufficient testimony to the merits of Mr. Rümker, and to our sense of them. In Mr. Dunlop were combined qualities rendering him of all others, the very individual fitted for the duties imposed on him—zealous, active, ready—but above all (and the combination is not an ordinary one), industrious and methodical. In the vast mass of observations made and registered by him, all is equable and smooth, as if the observations had all been made at a sitting:

> " . . . . . Servatur ad imum
> Qualis ab incepto processerit"—

no lacunæ—no long intervals of inactivity—nothing hurried or sketchy; but the same pains-taking, laborious filling in, pervading the whole,—marking that the observer's whole heart and soul were in his work, and that each individual observation possessed its own peculiar, though momentary, interest. Nor is this wonderful. The heavens visible to Europeans, have been so thoroughly examined, and their contents so carefully registered, that there is not the slightest rational probability of any thing new or uncommon offering itself to instruments of moderate power in the ordinary course of observation. Here, however, all was new;—for the optical power of Lacaille's telescope was far too feeble to afford much insight into the physical constitution of the objects determined with it: and thus all the excitement of discovery was maintained during every step in the progress of the work.

But to be susceptible of this excitement, so maintained, the observer must be animated by the true spirit of the Astronomer; and few have possessed this spirit in a greater degree than Mr. Dunlop. In a scientific point of view, therefore, he must be regarded as the associate, rather than the assistant of his employer; and their difference of situation becomes merged in their unity of sentiment and object.—These considerations alone would have rendered it impossible to your Council to disunite in any expression or mark of their approbation, individuals who have thus, each in his sphere, gone hand in hand together, towards the perfection of Southern Astronomy, even had the labours of Mr. Dunlop been confined to the ordinary business of an observatory, or to observations with fixed instruments. But this is very far from having been the case. The nebulous, as well as the sidereal heavens, have occupied his attention; and in the prosecution of this most delicate and difficult branch of Astronomy, he has availed himself entirely of his own resources, in the most literal sense,—the instrument which he used being not simply his *own*, but the work of his own hands; and the observations being performed by him after the departure of Sir Thomas Brisbane from the colony, at a personal sacrifice of his private interests, and in the face of difficulties which would have deterred any one not animated with a real and disinterested love of science from their prosecution. The results of these observations have been the description and determination of the places of upwards of 600 nebulæ and clusters of stars. And when it is recollected that Lacaille was able to observe not more than about 40 or 50 of these curious objects; we may form some idea of the extent of this labour. In addition to these interesting results, Mr. Dunlop has amassed a copious and valuable collection of Southern double stars, which he is at present occupied in reducing and arranging; and a variety of interesting and curious particulars relative to the magnitudes, colours, and other peculiarities of all the more conspicuous single ones.

Shut out as we are by our geographical situation from the actual contemplation of these wonders, the astronomers of Europe may view, with something approaching to envy, the lot of these their more fortunate brethren. The feeling, if an unworthy, is, however, but a passing one, and merges in that of admiration of their zeal and enterprise, and of gratitude for the information they have afforded us. In testimony of that admiration and that gratitude, on the part of this Society, towards Mr. Dunlop, I beg of you, Mr. South, to transmit to *him* also, this our medal, and to accompany it with the assurance that wheresoever his future fortunes may lead him—whether in the land which has already witnessed his meritorious labours—to complete and extend them, or in his native country, which is both able and willing to appreciate his value, to put the finishing stroke to the noble fabric he has been mainly instrumental in raising, by taking a leading part in the less exciting, but not less useful or indispensable work, of reducing the observations already made:—in either case he will be attended by our best wishes for his prosperity and happiness, and our confidence that science will continue to benefit by his exertions.

(*The President having quitted the chair, was succeeded by* MR. SOUTH, *one of the Vice Presidents, who addressed the Meeting as follows:*—)

GENTLEMEN,—Our excellent President in his Address has informed you of the appropriation of two of your Gold Medals, since our last Anniversary:—a third, however, has been decreed by your Council; and when it is known that Miss Caroline Herschel is the individual to whom it stands adjudged, it is not difficult to determine why the President has avoided the slightest allusion to it.

But that your Council has not selected one from the many of its Members, infinitely more competent to do justice to the transcendent merits of that illustrious lady, is most assuredly matter of regret. I must, therefore, throw myself upon your indulgence, hoping that the goodness of the cause may in some measure compensate for the inability of its advocate.

The labours of Miss Herschel are so intimately connected with, and are generally so dependent upon, those of her illustrious brother; that an investigation of the latter is absolutely necessary ere we can form the most remote idea of the extent of the former: but when it is considered that Sir W. Herschel's contributions to Astronomical Science occupy sixty-seven Memoirs, communicated from time to time to the Royal Society, and embrace a period of forty years, it will not be expected that I should enter into their discussion. To the Philosophical Transactions, I must refer you, and shall content myself with the hasty mention of some of her more immediate claims to the distinction now conferred. To deliver an eulogy (however deserved) upon *his* memory is not the purpose for which I am placed here.

His first catalogue of new nebulæ and clusters of stars, amounting in number to one thousand, was made from observations with the 20-feet reflector, in the years 1783, -4, and -5. A second thousand were furnished by means of the same instrument in 1785,-6,-7, and -8; whilst the places of five hundred others were discovered between 1788 and 1802. But when we have thus enumerated the results obtained in the course of *sweeps* with this instrument, and taken into consideration the extent and variety of the other observations, which were at the same time in progress, a most important part yet remains untold. Who participated in his toils? Who braved with him the inclemency of the weather? Who shared his privations? A female.—Who was she? His sister.—Miss Herschel it was, who by *night* acted as his amanuensis. She it was whose pen conveyed to paper his observations as they issued from his lips; she it was who noted the right ascensions and polar distances of the objects observed; she it was who having passed the night near the instruments, took the rough manuscripts to her cottage at the dawn of day, and produced a fair copy of the night's work on the subsequent morning; she it was who planned the labour of each succeeding night; she it was who reduced every observation, and made every calculation; she it was who arranged every thing in systematic order; and she it was who helped him to obtain an imperishable name.

But her claims to our gratitude end not here; as an original observer she demands, and I am sure she has, our most unfeigned thanks. Occasionally her immediate attendance during the observations could be dispensed with. Did she pass the night in repose? No such thing; wherever her illustrious brother was, there you were sure to find her also. A sweeper planted on the lawn became her object of amusement, but her amusements were of the higher order, and to them we stand indebted for the discovery of the comet of 1786; of the comet of 1788; of the comet of 1791; of the comet of 1793; and of the comet of 1795, since rendered familiar to us by the remarkable discovery of Encke. Many also of the nebulæ contained in Sir Wm. Herschel's catalogues were detected by her during these hours of enjoyment. Indeed, in looking at the joint labours of these extraordinary personages, we scarcely know, whether most to admire the intellectual power of the brother, or the unconquerable industry of the sister.

In the year 1797, she presented to the Royal Society a catalogue of 560 stars taken from Flamsteed's observations, and not inserted in the British catalogue; together with a collection of errata that should be noticed in the same volume.

Shortly after the death of her brother, Miss Herschel returned to Hanover. Unwilling, however, to relinquish her astronomical labours whilst any thing useful presented itself, she undertook, and completed the laborious reduction of the places of 2500 nebulæ, to the 1st Jan. 1800, presenting in one view the results of all Sir Wm. Herschel's observations on those bodies; thus bringing to a close half a century spent in astronomical labour.

For this more immediately, and to mark their estimation of services rendered during a whole life to Astronomy, your Council resolved to confer on her the distinction of a medal of this Society. The peculiarity of our President's situation, however, and the earnest manner in which the feelings, naturally arising from it, were urged when the subject was first brought forward, caused your Council to pause,—and waive on that occasion the actual passing their proposed vote. The discussion was however renewed on Monday last; and although there was every disposition to meet the President's wishes, still under a conviction that the doing so would have been a dereliction of public duty, it was'

Resolved unanimously, "That a Gold Medal of this Society be given to Miss Caroline Herschel, for her recent reduction, to January 1800, of the Nebulæ discovered by her illustrious brother, which may be considered as the completion of a series of exertions probably unparalleled either in magnitude or importance, in the annals of astronomical labour." A vote which I am sure every one whom I have the honour to address, will most heartily confirm.

Mr. HERSCHEL.—In the name of the Astronomical Society of London, I present this Medal to your illustrious Aunt. In transmitting it to her, assure her that since the foundation of this Society no one has been adjudged, which has been earned by services such as hers. Convey to her our unfeigned regret that she is not resident amongst us; and join to it our wishes, nay, our prayers, that as her former days have been glorious, so her future may be happy.

PROCEEDINGS AT THE FRIDAY-EVENING MEETINGS OF THE ROYAL INSTITUTION OF GREAT BRITAIN.

March 21.—Mr. Millington entered into an experimental account of the manufacture of paper, principally with a view of introducing and explaining by working models the making of paper by machinery, and especially some recent and important improvements. Numerous specimens of paper, varying both in quality and size, were exhibited.

Some experiments on the deceptive appearances of coloured shadows were shown in the Library by Mr. Marshall; and a large meteoric stone having a very peculiar polished metallic fracture was laid with other things upon the table.

March 28.—Dr. Harwood gave an account of some parts of the structure and œconomy of the Greenland whale, during which he entered into the illustration of certain views relative to the blubber and the skin which he had been induced to entertain, from a close examination of these and other parts of the animal. The specimens obtained for illustration from the museums of the Zoological Society, by Mr. Brookes and Dr. Harwood, were very fine and numerous.

The library-tables were covered with objects of interest from the East, from the museum of Lady Raffles and the collection of Mr. Bennett.

April 18.—Mr. Ainger delivered an illustrated explanation of some recent improvements in clock and watch escapements, especially of the one invented by Mr. Hardy. The principles of the four great divisions of escapements were exhibited and distinguished.

OBITUARY:—SIR J. E. SMITH.

On Monday the 17th of March, died at his residence in Norwich, Sir James Edward Smith, President of the Linnæan Society. This distinguished botanist was born at Norwich, December 2nd, 1759; and to the locality of his birth we are probably to attribute his early predilection for Natural History, for here he fell in with some of the earliest and most devoted disciples of the great Linnæus. This city has for more than two hundred years been famous for its florists and botanists. Here lived and flourished Sir Thomas Browne, the author of "Vulgar Errors," and "The Garden of Cyrus, or the quincuncial, lozenge, or network Plantations of the Ancients, artificially, naturally, and mystically considered." A weaver of this commercial place claims the honour of having been the first person who raised from seed a *Lycopodium;* as a Manchester weaver was the first to flower one of our rarest *Jungermanniæ.* During the middle of the last century Mr. Rose, the author of the Elements of Botany, Mr. Pitchford, and Mr. Crowe, names familiar to every botanist, took the lead in botanical science in their native city; and instilled into the youthful mind of the future President an ardent attachment to their favourite pursuit, and a skill in discriminating species for which these gentlemen were so eminent. Having remained the usual time at a school in the city, he went in the year 1780 to the University of Edinburgh, where he distinguished himself by obtaining the gold medal given to the best proficient in botany.

Upon leaving Edinburgh he came up to London to finish his studies, and soon became acquainted with the late Sir Joseph Banks. This acquaintance, and the access it obtained for him to men of science, only riveted more firmly his ardent attachment to botany; and accordingly, we find Sir Joseph recommending him as early as 1783 to become the purchaser of the Linnæan collection. As this circumstance laid the foundation of the President's future fame, and is one of peculiar interest at the present moment, we shall detail the history of the transaction.

The younger Linnæus had died suddenly, November 1, 1783; and his mother and sisters, desirous of making as large a profit as they could by his Museum, within a few weeks after his death offered through a mutual friend the whole collection of books, manuscripts, and Natural History, including what belonged to the father as well as the son, to Sir Joseph Banks, for the sum of one thousand guineas. Sir Joseph declined the purchase, but strongly advised Sir James Smith to make it, as a thing suitable to his taste, and which would do him honour.

Sir James in consequence communicated his desire to become the purchaser to Professor Acrel, the friend of the family of Linnæus, and who seems to have conducted the negotiation with scrupulous honour. The owners now began to suspect they had been too precipitate; having received an unlimited offer from Russia, while also Dr. Sibthorpe was prepared to purchase it, to add to the treasures, already famous, of Oxford. They wished to break off their treaty with Sir James Smith; but the worthy Swedish Professor would not consent to it, and insisted on their waiting for his refusal.

In consequence of the subtraction of a small herbarium made by the younger Linnæus, and given to a Swedish baron to satisfy a debt he claimed, a deduction of one hundred guineas was made in the purchase-money; and in October 1784 the collection was received, in twenty-six great boxes, perfectly safe. The whole cost, including the freight, was 1029*l.* The duty was remitted on application to the Treasury. The ship which was conveying this precious treasure had just sailed, when the King of Sweden (Gustavus III.), who had been absent in France, returned, and hearing the story, sent a vessel in pursuit, but happily it was too late.

The collection consists of every thing possessed by the great Linnæus and his son relating to natural history and medicine. The Library contains about 2500 volumes. The old herbarium of the father comprehends all the plants described in the *Species Plantarum*, except perhaps about 500 species (*Fungi* and *Palmæ* excepted), and it had then perhaps more than 500 undescribed.

The herbarium of young Linnæus appears to have had more attention bestowed upon it, and is on better paper. It consists of most of the plants of his *Supplementum*, except what are in his father's herbarium, and has besides about 1500 very fine specimens from Commerson's collection, from Dombey, La Marc, Pourrett, Gouan, Smeathman, Musson, &c., and a prodigious quantity from Sir Joseph Banks, who gave him duplicates of almost every one of Aublett's specimens, as well as of his own West Indian plants, with a few of those collected in his own voyages round the world.

The insects are not so numerous; but they consist of most of those that are described by Linnæus, and many new ones. The shells are about thrice as many as are mentioned in the *Systema Naturæ*, and many of them very valuable. The fossils are also numerous, but mostly bad specimens and in bad condition.

The number of the MSS. is very great. All his own works are interleaved with abundance of notes, especially the *Systema Naturæ*, *Species Plantarum*, *Materia Medica*, *Philosophia Botanica*, *Clavis Medicinæ*, &c. There are also the *Iter Lapponicum* (which was afterwards published), *Iter Dalecarlicum*, and a Diary of the Life of Linnæus for about thirty years of his life. The letters to Linnæus, (from which a selection was also published by the President,) are about three thousand.

This splendid acquisition at once determined the bent of the proprietor's studies. He considered himself, as he has declared, a trustee only for the public, and for the purpose of making the collection useful to the world and to natural history in general. How well he has fulfilled this trust will appear from the sequel. He had no sooner obtained quiet possession than he began to fulfill his engagement, for we find him in the year 1785 making his first appearance as an author, by translating the Preface to the *Museum Regis Adolphi Friderici* of Linnæus, being succinct and admirable reflections on the study of nature.

In the year 1786 he prepared himself for an extensive tour on the Continent; in which his chief object was to examine into the state of natural history in the different cities and towns he might pass through, not neglecting the incidents, especially the fine arts, which usually engage the attention of travellers. At Leyden he graduated in medicine; but it does not appear that he tarried there a longer time than was necessary for this purpose. On this occasion he published his Thesis *De Generatione.* The "Sketch of a Tour on the Continent," though long superseded as a companion to the tourist, is still curious to the naturalist, as showing the state of science at that time. It contains, too, a fund of good sense expressed with facility; and to those who enjoyed the acquaintance and friendship of the author, will always remain valuable, as furnishing the truest image of his mind, reviving his liberal opinions in their recollection, and his easy and elegant manner of communicating them.

In the year 1788, when he had returned and was settled in London, he with some other naturalists projected the establishment of the Linnæan Society, which had for its object the cultivation of Natural History in all its branches, and especially that of Great Britain. This Society, which has grown now into considerable importance, was a scion of the Royal Society, and had its origin in the jealousy which some of the members of the parent Society entertained of the preference which they alleged was given to Natural History in their Transactions; while its then President was thought to favour the subject, to the exclusion of others of equal, if not of greater importance. There are still some who recollect the argumentative and vehement eloquence by which this side of the question was supported by a reverend Prelate.

It was during this stormy period that Sir James Smith, in conjunction with the late Bishop of Carlisle, Sir Joseph Banks, and others, laid the foundation-stone of the Linnæan Society. Its first meeting was held April 8, 1788. The Society then consisted of fifty Fellows, and about twice as many more Foreign Members, Dr. Smith being the first President, Dr. Goodenough the first Treasurer, and Mr. Marsham the first Secretary. Of these original Fellows, how few are left! and of those who are, their hoary locks, still seen occasionally at the meetings of the Society, remind us of the respect and gratitude we owe to them as fathers. May their declining years derive consolation from the success of this their early project!

At the first Meeting, the President delivered a Discourse, judicious and appropriate, On the Rise and Progress of Natural History. We find him also about this time producing a paper which was read before the Royal Society, entitled Observations on the Irritability of Vegetables. It chiefly regards the mode of impregnation in the Barberry; and attracted considerable attention at the time, being translated into other languages, and appearing in different publications.

The next considerable work which we find him undertaking, is the re-publication of the wooden blocks of Rudbeck, which had fallen into his hands with the Linnæan collections. Linnæus was possessed of about 120 of these blocks, which had escaped the fire at Upsal, where almost the whole impression of the second volume, and all but three copies of the first, were burnt. As Rudbeck was the founder of a school at Upsal destined afterwards to give laws to the rest of the world, the re-publication of this fragment of his great work was a tribute of gratitude to his profound and varied learning.

From 1789 to 1793, our author was engaged in various publications relating to his favourite science. Most of them terminated in being only fragments, for want of patronage by the public. Such were his *Plantarum Icones hactenus inedita; Icones pictæ Plantarum rariorum; Specilegium Botanicum;* and "Specimens of the Botany of New Holland." One of these literary projects, "English Botany," however, did not suffer the shipwreck experienced by the others, but has received the encouragement it deserved. This is not attributable to its execution being superior to the other works which have failed, but because it treats of the plants of our own country, in which all are interested. It has the singular merit of being the only national Flora which has given a figure and description of every species native to the country whose productions it professes to investigate; and while other works of a similar kind have enjoyed the patronage of foreign Crowns, and have even been supplied with funds to carry them forward in their tardy progress, this work has been rendered complete by the patronage of the public alone, and was brought to a successful termination in 1814, by the united efforts of the President of the Society, and of Mr. Sowerby, the draughtsman and engraver.

In the year 1793 appeared in the Memoirs of the Academy of Turin, of which he was a Member, his essay *De Filicum Generibus dorsiferarum,* and which was republished in English in his "Tracts on Natural History."

Soon after our author's marriage in 1796 he removed to Norwich, his native place, where he continued to reside, paying occasional visits to London, for the remainder of his life.

The next considerable work upon which the reputation of our author is built, is the *Flora Britannica,* which appeared in the years 1800—1804. It is remarkable, like all his other labours, for accuracy in observing, accuracy in recording, and unusual accuracy in printing. It comprises descriptions of all the phænogamous plants, of the *Filices,* and the *Musci;* and every species has been carefully collated with those which Linnæus described. Being written in the Latin language, the information is condensed into a small compass; while it has the rare advantage of having had every synonym compared with the original author.

The *Compendium Floræ Britannicæ* has gone through four editions, and is become the general text-book of English botanists. It is perhaps the most complete example of a manual furnished on any subject.

While he was engaged in the *Flora Britannica,* the executors of the late Professor Sibthorpe selected him as the fittest person to engage in editing the splendid posthumous work of that liberal patron of science; a task for which the unrivalled attainments of the President, and his personal friendship with the Professor, peculiarly qualified him. The drawings which were made by Ferdinand Bauer, and the letter-press which was written by Sir James Smith from scanty materials furnished by Dr. Sibthorpe, are both worthy of so munificent an undertaking. To complete the work, which is to consist of ten folio volumes, and 100 coloured plates in each, Dr. Sibthorpe bequeathed a freehold estate at South Leigh, in Oxfordshire; which, after the completion, is to be charged with the support of a Professor of Rural Economy in the University of Oxford.

The

The Introduction to Physiological and Systematic Botany has also been a most successful publication, having passed through five editions. It is indebted for its popularity to a happy method which the author has of communicating knowledge, to the good taste he every where displays, and to that just mixture of the *utile* with the *dulce*, which he knew so well how to apportion.

In 1810 appeared his Tour to Hafod, the seat of his old and accomplished friend Thomas Johnes, Esq.

In 1814 he received the honour of knighthood from the hands of his present Majesty, on the occasion of his Majesty consenting to become the patron of the Linnæan Society, and granting them a charter.

About 1818 the Professor of Botany at Cambridge encouraged the President to offer himself for the Professorship of that University. He obtained the countenance of many of the heads of houses, but unfortunately it terminated in a controversy on his religious opinions, which he could not, and never would, compromise. It produced two small tracts from his pen, which at least show that he was not disqualified by the absence of the most charitable spirit.

In 1812 his Grammar of Botany appeared; and in the same year, a "Selection of the Correspondence of Linnæus and other Naturalists."

During a large portion of his literary life, he was in the habit of writing articles for Dr. Rees's *Cyclopædia* on different subjects in botany and biography connected with it. Many of these biographical memoirs are choice morsels of original information; and we need only refer to the words Curtis, Linnæus, Hudson, and Sibthorpe, in justification of our assertion. Most of his articles will be found marked with the letter S, it being his undeviating rule never to publish any thing on anonymous authority in science. Even some Reviews which he had written early in life, he afterwards avowed by republishing them in his "Tracts."

The second volume of the Supplement to the *Encyclopædia Britannica* is indebted to our author's pen for a Review of the Modern State of Botany, an article which supplies some deficiencies in his Introduction, though chiefly an abridgement of the *Prælectiones* of Linnæus, as published by Giseke.

During the whole of his literary career he occasionally contributed papers to the Linnæan Transactions.—But the last and best work of the distinguished President is the "English Flora," consisting of four volumes octavo, and describing the phænogamous plants and Ferns of Great Britain, though its title might imply a more limited range. *Finis coronat opus.* There is no Flora of any nation so complete in flowering species, and none of any country in which more accuracy and judgement are displayed. If any person should in future contemplate a work of this kind,—whatever the originality of his information, whatever the novelty of his subject,—let him imitate this illustrious author in careful remark, in taking nothing upon trust, in tracing every synonym to its source, and lastly in arranging his matter in such a manner, by the aid of different types, as shall render it easy of reference,

ence, and point out at a glance the nature of it. However mechanical some of this may appear, it is absolutely essential to be attended to in Natural History, where the subjects are infinite in number, and where aid must be derived from every mode of generalizing particulars.

In summing up the scientific character of the deceased, it may be comprised in a few words. As a naturalist, he contributed greatly to the advancement of science; and stood pre-eminent for judgement, accuracy, candour, and industry. He was disposed to pay due respect to the great authorities that had preceded him, but without suffering his deference for them to impede the exercise of his own judgement. He was equally open to real improvement, and opposed to the affectation of needless innovation. He found the science of Botany when he approached it, locked up in a dead language,—he set it free by transfusing into it his own: he found it a severe study fitted only for the recluse,—he left it of easy acquisition to all. In the hands of his predecessors, with the exception of his immortal master, it was dry, technical, and scholastic; in his, it was adorned with grace and elegance, and might attract the poet as well as the philosopher.

LXI. *Intelligence and Miscellaneous Articles.*

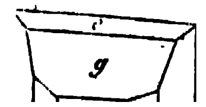
NEW MINERALS.

PROF. BREITHAUPT (Schweigger's *Journal der Chemie und Physik*, N. S. vol. xx. p. 314, &c.) gives a description of the following new mineral species:

I. *Karphosiderite.* Name derived from the straw-yellow colour. Reniform masses, rarely from granular composition, uneven; shining and glimmering in the streak, with resinous lustre. Colour and streak pale and high straw-yellow. Hardness = 4·0...4·5. Sp. gr. = 2·5. Feels greasy. Before the blowpipe upon the coal it becomes black, and melts in a strong fire into a globule which is attractible by the magnet. In glass of borax it is easily soluble, and with salt of phosphorus it melts into a black scoria. It contains oxide of iron, phosphoric acid, water, with small quantities of oxide of manganese and zinc. It has a great similarity to oxalite, yellow iron-ore, or iron-sinter. It occurs in Greenland.

II. *Mesitine-spar.* Name derived from *μεσιτης*, that is, what stands in the middle (of brachytypous lime-haloide, and brachytypous parachrose baryte). Rhombohedral, $R - \infty$. $R = 107^\circ\ 14'$ $R + \infty$. Cleavage distinct, parallel to R. Lustre vitreous. Colour dark-grayish, and yellowish-white... yellowish-gray. Streak white. Transparent...translucent. Hardness = 4. Sp. gr. = 3·34...3·37. Before the blowpipe the mesitine-spar decrepitates. In muriatic and nitric acid a feeble effervescence takes place, but it is entirely soluble. It contains probably magnesia, lime, protoxide of iron, and oxide of manganese. It is found in little crystals, in rhombohedral quartz at Traversella in Piemont.

III. *Tautolite.* Prismatic. Fundamental form, scalene four-sided pyramid.



$$a : b : c = 1 : 1{\cdot}9451 : 1{\cdot}3648.$$

Observed combinations:

1, $M = \infty\, a : b : c = 109^\circ\, 46'$.
$g = 4\, a : b : \infty\, c = 51^\circ\, 52'$.
$h = \infty\, a : \infty\, b : c$ fig. 9 of Mohs's treatise,
2, $M ; g ; h$;
$i = \infty\, a : \frac{3}{4}\, b : c = 86^\circ\, 22'$.
$e = 2\, a : b\, \infty\, c = 88^\circ\, 25'$.

Cleavage, only in traces and interrupted, parallel to M and *h*. Fracture conchoidal. . .uneven. Lustre vitreous. Colour velvet-black. Streak gray. Opaque. Hardness = 6·5. . .7·0. Sp. gr. = 3·865. Before the blowpipe, upon charcoal, the tautolite melts to a blackish scoria, which is attracted by the magnet; with borax it melts to a clear green glass. These and other reactions show that the mineral consists of silica, black protoxide of iron, magnesia, and alumina. It is found in the volcanic feldspath-rocks, in the neighbourhood of the Lake Laach Sea in Rhein-Prussia. The tautolite seems to be related to the chrysolite, as the Ceylanite to the Spinelle.—Brewster's *Journal of Science*.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1828.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·30 Mar. 16. Wind W.—Min. 29·02 Mar. 21. Wind S.W.
Range of the index 1·28.
Mean barometrical pressure for the month .... 29·879
Spaces described by the rising and falling of the mercury............ 5·640
Greatest variation in 24 hours 0·580.—Number of changes 17.
Therm. Max. 63° on several days.—Min. 30° March 6. Wind NW.
Range 33°.—Mean temp. of exter. air 47°·92. For 30 days with ☉ in ♓ 49·72
Max. var. in 24 hours 23°·00—Mean temp. of spring water at 8 A.M. 50°·86

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the morning and evening of the 16th 100°
Greatest dryness of the air in the afternoons of the 19th & 24th... 44
Range of the index.. 56
Mean at 2 P.M. 58°·4 —Mean at 8 A.M. 73°·0—Mean at 8 P.M. 71·4
—— of three observations each day at 8, 2, and 8 o'clock......... 67·6
Evaporation for the month 1·90 inch.
Rain near ground 1·755 inch.—Rain 23 feet high 1·620 inch.

Summary of the Weather.

A clear sky, 4½; fine, with various modifications of clouds, 15; an overcast sky without rain, 7½; foggy 1; rain, 3.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
12	6	24	1	19	25	15

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
3½	2½	1	3	½	6	5½	9	31

General Observations.—This month commenced with cold weather, which kept up till the 7th; and it was also cold and humid from the 23rd to the end: the other part was mild for the season, the maximum temperature of the external air in the day often being at 63°.

Rain, frequently accompanied with hail, fell on twelve different days, but the quantity does not amount to two inches.

On the 14th, 15th, and 16th, thick fogs prevailed here.

In the afternoon of the 22nd there were two winds, the lower one from N.W., and the upper one from the South, which, uniting the clouds, gradually brought on several vivid flashes of lightning, and loud claps of thunder, which were immediately followed by a heavy shower of hailstones coated with a snowy substance. Soon after 5 o'clock in the morning of the 29th, a smart shower of snow fell, which covered the ground and the surrounding hills for a short time only, as it was soon afterwards succeeded by rain. In the evening of the 30th it again snowed a little, but it disappeared before the morning.

After the vernal equinox winter revisited us in keen northerly and easterly winds in the days, and thick hoar frosts in the night; so that winterly weather was felt here some weeks after the beginning of spring.

The mean temperature of the air this month is about three degrees higher than the mean of March for the last twelve years. The temperature of spring water is nearly at a stand; and the ground is now warmer than it has been at the close of March since 1822. But this additional heat does not appear to have modified the prevailing chill in the atmosphere, it being either too small, or dissipated by evaporation, before it could arrive at a sufficient altitude to have any effect.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are one solar and one lunar halo, one meteor; lightning and thunder in the afternoon of the 22nd, and nine gales of wind, or days on which they have prevailed; namely, two from the North, two from North-east, one from South-east, one from West, and three from North-west.

REMARKS.

London.—March 1, 2. Cold and cloudy. 3—5. Fine. 6. Clear and cold. 7. Cloudy: showers at night. 8. Hazy: fine. 9, 10. Very fine. 11. Slight fog: very fine. 12. Cloudy. 13—15. Very fine. 16, 17. Cloudy. 18. Fine: stormy at night. 19. Very fine. 20, 21. Cloudy: with showers of hail. 22. Cold with slight showers. 23—25. Clear and cold. 26. Cloudy: rain at night. 27. Rain in morning: fine. 28. Drizzly: cloudy. 29. Cloudy and cold. 30. Cloudy. 31. Foggy: very fine.

Boston.—March 1. Cloudy: rain P.M. 2, 3. Cloudy. 4. Cloudy: stormy night. 5. Fine. 6. Fine: snow A.M. 7. Cloudy: rain P.M. 8. Cloudy. 9—11. Fine. 12, 13. Cloudy. 14, 15. Fine. 16—18. Cloudy. 19. Stormy. 20. Cloudy: rain at night. 21. Cloudy: rain P.M. 22. Fine: rain at night. 23. Fine. 24. Fine: rain P.M. 25, 26. Cloudy. 27. Stormy and rain: rain A.M. 28. Cloudy. 29. Fine: rain A.M. 30. 31. Fine.

Penzance.—March 1. Clear: fair. 2. Fair: clear. 3—6. Fair. 7. Fair: showers. 8. Clear. 9. Fair. 10. Fair: misty. 11. Misty: rain. 12—14. Fair. 15. Foggy. 16, 17. Fair. 18. Clear: rain. 19. Rain: fair. 20. Rain: showers. 21, 22. Hail-showers. 23. Hail and snow. 24, 25. Clear: hail-showers. 26. Cloudy: rain. 27. Cloudy: clear. 28. Cloudy: showers. 29, 30. Clear: showers. 31. Clear.—Rain-gauge ground level.

Meteorological Observations made at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Giddy *at Penzance, Dr.* Burney *at Gosport, and Mr.* Veall *at Boston.*

Days of Month, 1828.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston 8½ A.M.	London.		Penzance.		Gosport.		Bost. 8½ A.M.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.		Max.	Min.	Max.	Min.	Max.	Min.										
○ Mar. 1	30·315	30·091	30·28	30·26	30·24	30·08	29·75	49	42	50	46	54	44	44	NW.	N.	NW.	calm	...	...	...	0·015	...
2	30·121	30·083	30·08	30·06	30·04	30·02	29·62	49	33	50	44	50	36	46	NW.	NE.	N.	N.	...	...	...	...	0·03
3	30·028	29·981	30·06	30·00	30·01	29·83	29·50	50	40	50	44	54	40	42	NW.	N.	NW.	calm	0·15	...	...	·030	...
4	29·967	29·862	30·02	30·00	29·94	29·80	29·40	50	36	52	46	53	42	41·5	NW.	N.	NW.	calm	...	...	...	...	...
5	29·891	29·731	29·90	29·88	29·75	29·69	29·30	46	28	48	44	54	31	40	N.	NW.	N.	W.	...	...	...	...	...
6	30·181	29·964	30·03	29·96	30·07	29·91	29·55	39	27	44	39	42	30	31	N.	NE.	N.	NW.	...	...	...	...	...
7	30·281	30·164	30·12	30·10	30·18	30·13	29·84	42	40	49	38	48	45	33	NW.	NW.	NW.	calm	·20	·10	...	·100	...
8	30·271	30·110	30·14	30·12	30·22	30·06	29·70	59	47	53	44	59	45	50	W.	NW.	NW.	calm	...	...	...	...	·22
☾ 9	30·301	30·260	30·20	30·19	30·22	30·22	29·72	61	42	54	45	58	45	50	SW.	W.	SW.	calm	...	...	...	...	...
10	30·278	30·144	30·19	30·18	30·23	30·20	29·63	61	38	55	45	57	44	52	W.	W.	SW.	W.	·20	...	...	...	...
11	30·284	30·252	30·18	30·08	30·22	30·17	29·75	62	48	53	45	60	44	50·5	SW.	S.	NW.	W.	...	...	...	...	...
12	30·105	30·073	29·99	29·98	30·06	30·02	29·65	58	41	54	46	59	46	44·5	SW.	W.	SW.	NW.	...	...	0·090	...	...
13	30·220	30·163	30·04	30·00	30·17	30 14	29·58	61	47	59	47	60	48	49·5	W.	W.	SW.	calm	·15	...	...	...	...
14	30·339	30·298	30·18	30·14	30·21	30·20	29·71	63	40	54	48	59	44	53·5	W.	S.	W.	W.	...	...	...	...	...
● 15	30·395	30·385	30·28	30·18	30·29	30·24	29·83	67	42	54	47	63	41	51	S.	W.	SE.	W.	...	...	...	...	...
16	30·379	30·224	30·28	30·26	30·30	30·19	29·73	62	48	54	47	59	43	53	SW.	W.	W.	calm	·18	...	...	·010	·03
17	30·195	30·167	30·18	30·16	30·12	30·12	29·62	61	45	54	48	60	49	53	W.	NW.	W.	calm	...	...	...	...	...
18	30·213	29·760	30·20	30·00	30·12	29·89	29 68	58	48	54	44	63	48	47	W.	W.	NW.	W.	...	...	...	·150	...
19	29·607	29·498	29·80	29·74	29·54	29·53	28·87	55	38	52	46	63	40	49·5	SW.	NW.	NW.	W.	·25	...	0·280	...	...
20	29·410	29·180	29·50	29·30	29·46	29·15	28·91	53	40	53	45	54	48	47	SW.	NW.	W.	calm	...	·06	0·080	·150	...
21	29·151	28·997	29·20	29·20	29·16	29·02	28·65	55	35	48	44	56	44	41	NW.	NW.	SW.	calm	...	·09	0·390	·140	·04
22	29·326	29·287	29·34	29·30	29·30	29·23	28·84	48	30	48	37	54	39	42	W.	NW.	NW.	W.	·16	·03	0·230	·020	·06
☽ 23	29·569	29·387	29·50	29·42	29·48	29·37	28·95	48	32	48	34	53	36	41	N.	NE.	NW.	NW.	...	...	1·010	...	·05
24	29·810	29·679	29·87	29·70	29·66	29·58	29·21	47	27	47	40	54	33	39·5	NE.	NE.	N.	NW.	...	...	0·105	...	...
25	29·906	29·861	29·88	29·88	29·83	29·75	29·45	42	24	47	38	52	33	41	E.	NE.	NE.	NE.	·18	...	...	...	·05
26	29·900	29·763	29·73	29·42	29·79	29·64	29·52	47	38	46	38	49	31	42·5	SE.	SE.	SE.	E.	...	·12	0·405	·380	...
27	29·509	29·470	29·35	29·30	29·41	29·36	29·20	55	36	50	41	52	43	41·5	SE.	E.	SE.	E.	...	·14	0·360	·500	...
28	29·778	29·553	29·75	29·50	29·66	29·38	29·25	50	28	49	43	59	42	43	SE.	NE.	NE.	E.	·20	·02	...	·150	·09
29	29·944	29·865	29·80	29·90	29·78	29·74	29·46	45	31	48	40	50	36	38	SE.	NW.	NE.	NW.	...	·03	0·200	·110	...
30	30·223	29·908	30·06	30·00	30·05	29·90	29·65	49	26	50	36	48	35	42	N.	E.	E.	calm	...	...	0·170	...	·10
○ 31	30·388	30·303	30·20	30·20	30·27	30·17	29·90	54	27	52	38	52	38	43·5	NE.	S.	SE.	calm	·23	...	0·125	...	...
Aver. :	30·019	29·886	30·28	29·20	30·30	29·02	29·46	53	37	59	34	63	30	44·6					1·90	0·59	3·445	1·755	0·67

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

JUNE 1828.

LXII. *On the Artificial Production of Cold. By* RICHARD WALKER, *Esq. of Oxford.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IT is now forty-one years since my discoveries on the "artificial production of cold" were first made public by their appearance in the Philosophical Transactions for the year 1787, and several succeeding volumes. Passing over what has already been published respecting them, I shall proceed to a detail of a few other circumstances as a kind of appendix, which I have for several seasons intended to offer for publication, had not other matters, as professional avocations and professional communications, too much engaged my attention to allow of it.

Immediately on the announcement of the discoveries as above stated, I received various proposals from respectable persons respecting their practical utility in this country. I answered these by a declaration that wherever natural ice could be obtained and preserved, this must ever supersede the use of the artificial means alluded-to. It is true that I had an eye to their application in hot climates, as between the tropics; and so soon as my experiments became public, a treatise on the diseases of tropical climates appeared from the pen of Dr. Moseley, who fixed upon one, which he considered the most appropriate, and strongly recommended its adoption as a very valuable acquisition as well in a medicinal point of view, as a luxury.

Relinquishing, from various causes, the design of applying them myself to any such purpose, I took care however to point out, in my original communications, the complete efficiency of them for such intention to their utmost extent, and the best mode, as it appeared to me, of applying them in hot climates.

Understanding, a few summers ago, that a manufactory had been established for preparing ice-creams, as well without the use of ice, as with it; and likewise for making for sale an apparatus for the purpose,—I was induced to visit it. I examined the apparatus,—a very appropriate one for the purpose, and likewise the freezing powder, which I instantly recognized to be the weakest in power of my various compositions for the purpose, but possessing the advantage of being readily recovered repeatedly for the same purpose with undiminished effect. This powder, by its taste and appearance, I found to be a mixture of sal ammoniac and nitre, which I was informed was repeatedly recoverable in a fit state for refrigeration. I originally exerted every effort, in vain, to increase its power by the addition of a third ingredient, possessing likewise the advantage merely by evaporation to dryness, of being repeatedly recovered for the same use. This powder, as related in my original communications, consists of equal parts by weight of sal ammoniac and nitre. By way of test, I recovered it by evaporation twelve times, without any abatement of its efficacy, as originally stated*.

It is unnecessary to enter into a description of the apparatus just mentioned, or the principle and mode of its application, especially as the whole is embraced in the following statement.

A circumstance occurred here (at Oxford) which occasioned the method to be put to the test of useful application. A confectioner, happening in a scarce season to be unprovided with natural ice, applied to me for assistance. I assured him that in the large way (as I have stated in my original communications) the best method was to freeze water first, and then to use the ice in the usual way for freezing creams. Accordingly an apparatus of large dimensions, of rather an oblong form, was made of tin (fitter for the purpose if cased with wood) consisting of channels so constructed that the water to be frozen should be subjected to the freezing mixture on both sides. This, properly prepared, was placed in a cool cellar during the night, and early in the morning (the temperature in the open air in the shade in the day-time being above 80°) the ice was collected, which amounted to several pounds in weight. This ice, which was as limpid as the finest flint-glass, was applied in the usual way, and with the apparatus ordinarily used by confectioners for the purpose of freezing creams, and the mixed powder, of which he had procured an adequate quantity, repeatedly recovered by evaporation over his hot iron plates, for fresh use.

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* The manufactory alluded to is at "No. 41, New Bridge-street, Blackfriars, London (late Patterson's), now Armstrong's."

I shall now present the immediate object of my present communication; viz. what I consider to be the best mode and fittest apparatus for cooling wine in summer, for freezing creams in the small way for private use, and likewise for freezing a small portion of water, merely as an experiment for public or private exhibition.

The drawing annexed (Plate VII.) is designed to represent on a small scale the construction and exact proportions of each freezing apparatus, and likewise the construction and form of the apparatus for cooling wine.

Fig. 1. is an apparatus for freezing water on the smallest scale, as above mentioned, in the hottest weather. The vessel for containing the freezing mixture is three inches and a half in width, and its height equal in measure to its width; and the tube for containing the water to be frozen five-eighths of an inch in width, and reaching, as represented, very near to the bottom of the vessel: there is likewise a rim or continuation of the vessel, without a bottom, to insulate it from the table or stand it rests upon. The apparatus itself consists of two parts; viz. the vessel for containing the freezing mixture, and its cover, in one piece with the tube, fitting close over it (represented together in the drawing). When the water is frozen, upon taking off the cover and wiping the tube, the solid ice will have become detached by the heat, and on inverting it drop out.

The process may be known to be completed by the going off or melting of the hoar-frost which exhibits a curious appearance outside the apparatus.

Fig. 2. consists of an apparatus in one piece; viz. the vessel for containing the cooling mixture, and the cup or can (if I may so call it) for receiving the decanter, its top rising somewhat above the height of the vessel for an obvious reason, with a cover that will admit of easy removal (in the drawing represented together). This apparatus likewise has an appendage or rim like the former, to insulate it from the table:—it may be convenient to be possessed of a couple of these.

Fig. 3. The apparatus for freezing creams, in which the freezing mixture is to act on both surfaces of the part containing it, as being more œconomical and expeditious, is not so simple. This however consists only of two parts; viz. the vessel for containing the freezing mixture; and a cover, to which is attached, in the same piece (instead of a tube or cup as in fig. 1.), a concentric annular cavity or chamber, in which the

prepared cream is to be frozen: this cavity, forming a circle within the vessel itself, is open at the top, as represented, and of course closed at the bottom, and reaching very nearly (as the tube in fig. 1.) to the bottom of the vessel: this secondary part, as likewise represented, fits close as in fig. 1. over the vessel containing the freezing mixture. The proportions of the apparatus when together are thus: The outer space in width, two parts all round; the middle space, or that which contains the cream, one part all round; and the inner space three parts in width,—this serving as a general scale of proportions for an apparatus of any size. The proportions for an efficient apparatus, as my own, may be: for the first space ten-eighths of an inch (one inch and one-fourth); for the second, five-eighths of an inch; and for the third space, fifteen-eighths, or rather two inches, making the width of the apparatus itself somewhat above five inches and a half; its height being equal to its width, a projecting rim at the bottom likewise to insulate it from the table. It will be perceived that in the figure there are seven very small holes or apertures in the central part of this cover (one in the centre and six round at due distances), just sufficient for the escape of the air, to admit of the ascent of the freezing mixture in the middle part of the vessel. This apparatus is somewhat elevated at the top, or slightly convex, and the part in which the apertures are placed guarded by a shallow rim to prevent an accidental running-over of the mixture into the part containing the cream. This apparatus should be furnished (as expressed in the figure) with an outer cover similar, but less elevated, to the one at fig. 2. Previously to use it will be proper to ascertain the quantity of liquid the apparatus will contain when together, and mark its height; likewise the proportion of the ingredients for furnishing a given quantity in measure should be known. Thus, if the three salts are used (which I would recommend to a private individual, always doing so myself, although these cannot be recovered for future use, but being more efficacious than the two only) for each pint, small or old measure, will be required of sal ammoniac and nitre (each equal parts by weight reduced together into fine powder) six ounces, and of Glauber's salt, in clear crystals and dry, four ounces and a half, freely reduced to fine powder, or kept from the access of air, and in a separate parcel from the former; and water ten ounces, or enough to make up one pint in measure when added to the former ingredients:—of course, the whole must be well stirred together, and expeditiously, before introducing that part of the apparatus which contains the article to be frozen, and occasionally afterwards, till the object is completed, avoiding as much as possible any accidental accession of heat. A freezing mixture composed of sal ammoniac and nitre with water, all at the temperature of 50°, to which temperature, or nearly so, they may all be reduced by water from a pump by drawing off a sufficient quantity first, will from 50° produce a cold of 22° below the freezing point, and with the addition of Glauber's salt to 28°. The confectioners find a degree of cold at 12° or 15° below the freezing point sufficient for their purpose; but it must be recollected that the cold produced by salts dissolved in water is not so durable as with ice and salt; the duration of the refrigerating power in the above mixtures will of course be in proportion to the quantity and thickness of the apparatus. In the way the confectioner managed, the mixture in the apparatus retained its freezing property till the morning: my usual way is, in extreme hot weather, to place the vessel containing the powdered salts in the coldest water drawn from the pump previously; but in the ordinary way it will suffice to add the cold water without the above precaution: it may be advisable to be provided with a second quantity of the ingredients to preserve the cold by a renewal of the mixture. The drawings are taken from an apparatus of each kind of my own,—they are made of tin, for want here of a fitter material, and are painted outside of a grass-green colour. The confectioner abovementioned laid in a stock of a hundred weight of each of the articles; viz. sal ammoniac and nitre; the former at the rate of one shilling per pound, and the nitre at fourpence—which of course when mixed, was at the moderate price of only eightpence per pound. Glauber's salts may be procured in the large way at the rate of about twopence per pound, and by the single pound at fourpence. The apparatus abovementioned may be only half or three parts filled for use; care must be taken in every instance that the surface of the subject to be acted upon be rather below the surface of the freezing mixture.

For cooling wine, the coldest water drawn from a pump will be quite sufficient; however, if required, a small portion of the cooling powder may be added to the water.

The addition of Glauber's salt, it may be observed, increases the density of the mixture, which then becomes a better conductor of the cold, if I may so express myself, and moreover retains the same temperature longer: of course it will be better of the two to overcharge than undercharge the proportion of the salts to the water. It will be apparent, for obvious reasons, that the part containing the subject to be cooled should be as thin as may be, and the whole of the external part in every apparatus thick.

This detail may probably appear prolix to any person induced by curiosity only to look it over; but to any one who means to put it in practice, the whole will be found essential, and with a little attention and experience become familiar and easy, and in which I have endeavoured to combine every advantage the subject will admit of; and as coming from the "fountain head," it may not prove uninteresting to some at least of your numerous readers. I am, Gentlemen,

Your most obedient servant,

Oxford, April 28, 1828. RICHARD WALKER.

LXIII. *On the Causes of Single and Erect Vision*[*]. *By* L*** M*** S*******.[†]

IN order to understand aright the reason of single and erect vision, it is necessary first of all to perceive the truth of certain metaphysical positions in relation to vision, without the establishment of which, confused ideas, hypothetical assumptions, and inconclusive reasonings on optical experiments and facts are presented to the mind, and tend to embarrass the simplicity of that truth which might otherwise be immediately revealed.

First,—Vision is a consciousness in the mind, and its next proximate cause must be a power equal to its production, and which unites it to the material world.

Secondly,—Vision of *one colour only* can never yield the vision of figure, because the proximate cause of the vision of figure is a line of demarcation formed by the sensation of the junction of *two colours.*

Thirdly,—The physical impulse producing such consciousness of colouring, is an equal proportional variety upon the retina of an eye; one eye alone being first supposed, as it is truly efficient to yield the idea of figure.

Fourthly,—An object cannot be in two places at the same time.

Fifthly,—An object cannot exist and put forth its action *where it is not.*

These premises being supposed to be granted, let the question be asked, Why with two eyes given, two objects are not seen, although there be but one object given externally?

The answer (when supported by the foregoing premises, and conjoined with certain optical facts with which all who are conversant with the subject, are acquainted) will be, *because there is not presented to the mind that variety of colouring which is necessary and alone efficient as the next proximate cause of vision;* that is, *there are not two lines of separate demarcation between two objects, but one line of demarcation*

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[*] Communicated by a friend of the Author.

[†] Author of "An Essay on the Relation of Cause and Effect;" and of "Essays on the Perception of an External Universe," &c.

cation only is presented, as in ONE *eye supposed.* Should it be asked, whence is it that such a proportional variety is not presented to the mind? The answer which the premises and optical experiments equally support, is, *because the impulse upon each eye (when the axes of both are directed to the same point or object,) being precisely alike, there is no variety of colouring painted upon either eye, equal to the production of that variety of perception, necessary to yield the ideas of two objects separated from each other, between their interior and horizontal edges.*

Let the letter A, for instance, be painted upon *one* eye, and the perception of its figure arises in the mind, from the points of distinction between the black letter and the white around it: there is a sense of difference created. Place it on similar points of two retinæ, and each point of the figure painted on each retina will yield to the mind but *one* point of conscious black against *one* point of conscious white; and not *two* points of black against *two* points of white, because there is no intervening white painted on either retina, which can yield a consciousness of the separation of the two A's to a distance from each other, thus, A——A.

The white space between the two A's is not painted on either retina. How then can any idea of it arise in the mind?

If, in order to render these ideas more intelligible, we analyse with still greater nicety the question, why we see duplicates of similar figures with *one* eye only supposed, it will at once appear obvious why we can perceive but *duplicates* of such figures, instead of *quadruplicates*, when *two* eyes are used.

Now if *one* eye should see but one colour only, it is supposed to be granted that there could be no sense of any defined figure whatever: one impulse therefore yields not figure.

If one and the same colour should be seen by two eyes, it must still be acknowledged there would be no figure: two *similar* impulses therefore cannot give the sense of figure to the mind. Now upon *one colour* (say a purple ground) painted upon one retina, mark a scarlet circle ○; a sense of one figure will immediately arise from two varieties of colour being carried to the mind, viz. a line of demarcation to the purple ground by the scarlet circle: *two* impulsions, or *two* varieties of colouring, are therefore necessary to the perception of *one* figure. Again, if with *one* eye given, I wish to see *two* scarlet circles upon the purple ground, what must I do? Will *four* impulsions yield *two* similar figures? I answer, No. There must be *five* impulsions in order to convey to the mind the sense of *two* figures: there must be $\overset{2}{\textcircled{1}}\!-\!\frac{5}{5}\!-\!\overset{4}{\textcircled{3}}$; that is, the impression of the purple ground must be repeated in *two* different parts of the sentient retina; *two* scarlet lines must be thence impelled, and these made obvious by *the intervening horizontal impulse between the circles;* for could the intervening space be absorbed, and each point of scarlet coalesce with each point of scarlet, and purple with purple, there would then be but *four* impulsions of colouring, but *four* varieties, which would be inefficient to the observation of *two* figures. A coalescence of similar points of colour may perhaps produce a superposition or increment of colour, so as to create a superior brilliancy in the appearance of the object; but a *coalescence of points* cannot give a sense of *the separation of points.* Therefore for the mind to have a sense of *one* figure, there must be two consciousnesses of colour; and to have a sense of *two* figures, there must be *five* distinguishable consciousnesses of colour. However often *one* colour only be repeated, there will be no figure; however often *two* similar colours be repeated, (no intervening one being supposed,) but *one* figure will arise; whilst to entertain a sense of *two* figures, *five* impulsions are necessary. I hold it therefore as an axiom in the laws of vision, *that the repetition of similar impulsions of colour will not yield a number of figures equal to the number of such impulsions; but that the number of figures perceived, arises from those proportional intervals of the impulsions of colour, which must vary in a certain ratio to the number of figures impelled.*

However, therefore, the number of eyes may be multiplied, the mind can have no consciousness of any additional number of figures, whilst only similar impulsions of colour are yielded to it.

Let us enter into some further detail. If *one* scarlet circle on the purple ground be painted on the corresponding points of *two* retinæ, and thence impelled to the mind, there can still only arise the sense of *one* scarlet circle; for there have existed but *four* physical varieties on the retinæ, and but *four* varieties have been impelled to the mind: and it has been proved that *five* physical varieties are necessary to exist upon a retina, or upon retinæ however numerous, in order to impel corresponding consciousnesses to the mind, and which would be necessary for the mental apprehension of *two* figures. Let the figures without an intervening horizontal colour be supposed to be marked thus, $\overset{2}{\textcircled{1}}\frac{0}{0}\overset{4}{\textcircled{3}}$. The intervening horizontal colouring necessary to separate the painting of *two* figures on the retinæ, is not painted on either retina; *it does not exist;* and therefore no conscious separation of *two* figures can possibly arise to the mind. There exists indeed a certain space between the two eyes outwardly on the face, but the colouring of this intervening space is not painted on either retina, and therefore cannot be noticed by the mind; nor is there any intervening horizontal purple presented to the mind: Each point of scarlet does but coalesce with each point of scarlet, and each point of purple on *one* retina, with each point of the similar purple on the *other* retina; there is no surplus intervening purple on either retina, and it has been shown that "repetitions of similar impressions of colour do not yield to the mind the sense of an equal number of figures."

But should the scarlet circle be painted on points of the retinæ which do *not* correspond, then there will necessarily arise the sense of *two* figures; because in that case dissimilar impulsions of colour are carried forward to the mind, and yield that proportional variety which determines the observation of conscious duplicates: for the pictures are then immediately painted upon different parts of their respective retinæ, and the distance between two objects "will be proportional to the arch of either retina, which lies between the picture on that retina, and the point corresponding to that of the picture on the other retina*." On this intervening arch a surplus quantity of purple would be interiorly and horizontally painted, and would thence separate the two scarlet figures: *five* impulsions would carry *five* conscious colours to the mind, and *two* separated figures would immediately be observed.

An illustration of these ideas, and especially of this last statement of Dr. Reid's, might very easily be imagined, by conceiving two small terrestrial globes to be painted with precisely similar colours: Let them be rectified to the same degree of latitude and longitude, and similar colours only will appear on the visible surfaces of each; turn them both so many degrees to the east or west, still only similar colours will present themselves; but let one of the globes remain at rest, and turn the other any number of degrees of longitude to the west, a new country will arise to the east on the globe so moved, whose *variety* of colouring must necessarily prevent the notice of a *mere uniformity of colouring* on the two globes, and which variety will separate the appearance of any given country on them, "*proportionally to the number of degrees marked on the arch of the horizon,*" through which the globe so moved had passed. Inspire the colouring on their surfaces with a simultaneous single sensibility, and it will immediately be perceived, that no consciousness of the existence of any two similar countries could arise, so long as the surfaces were regulated to the same degree of latitude and longitude; but the moment they were separated by an alteration of the longitude of either, or both, the sense of that newly arisen continent, or sea, would divide the sense of the remainder.

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* See Dr. Reid's Inquiry, ch. vi. sec. 13.

The following passage is extracted from the Encyclopedia Britannica, and is a quotation from Dr. Wells's Essay on Single Vision. "If the question be concerning an object at the concourse of the optic axes, it is seen single; because its two similar appearances in regard to shape, size, and colour, coincide with each other through the whole of their extent."

This opinion thus expressed, comes nearer to my meaning than any other with which I am acquainted; nevertheless Dr. Wells's argument on the subject (and which is too long to insert here,) is as fallacious as that of any of his predecessors; inasmuch as it assumes *an hypothetical law of vision*, in order to establish that *coincidence of shape, size, and colour* upon which he perceived single vision did necessarily depend.

This laborious argument of his, is as entirely needless as it is futile; because it proceeds upon the supposition that objects are *seen by the mind, beyond the mind*, at the angle formed by the axes of the eyes, in their direction to the same point of distance.

Now when, on the one hand, colour (conscious visible colour) is admitted to be *in* the mind, and never to proceed again *out* of it, in any line, or at any angle to form an object; and on the other hand, by those demonstrable laws of optics established by Sir I. Newton and others, "that when the axes of the eyes are directed to a given point or figure, the said figure is painted on corresponding points of the retinæ,"—then the *coincidence* which Dr. Wells speaks of, must of necessity take place; and such coincidence can only determine a consciousness of single vision to the mind; or in other words, can only determine those similar appearances of colour, on which visible size and figure ultimately and alone depend: for the centres coincide with the centres, and the edges with the edges, of the figures; without any variety, or interval of colouring between the interior and horizontal edges,—the conscious sense of which is absolutely necessary in order to induce a *sense* of *variety* or plurality of figure. When the figure is painted upon points of the retinæ which do not correspond, then there must necessarily arise a *sense of two figures;* because the centres *not coinciding* with the centres, nor the edges with the edges, *there exists a surplus colour* in one eye which divides the interior and horizontal edges of the two figures. This surplus colouring is determined to the retina by some exterior object, which by the shifting of the axis finds a place on which to paint its rays, "and is equal to the arch between the picture of the given figure on that retina, and the point corresponding to that of the picture on the other retina;" and which surplus colouring must determine a proportional consciousness to the mind, observing thereby the same rule which determines the "notice of two similar figures, when one eye only is used;...*when the apparent distance of two objects seen with one eye is proportional to the arch of the retina which lies between their pictures,*" and on which an interval of colouring is necessarily painted, *but which circumstance Dr. Reid did not consider it material to notice* *.

The optical facts to which I have alluded are very shortly and very well expressed in Dr. Reid's "Inquiry†:" The passages in the chapter from which I have partially quoted, and which it may be as well to give entire, are the following; and I repose on them as stated facts, not containing either hypothesis, opinion, or reasoning.

"First,—When the axes of both eyes are directed to one point, an object is seen single; and in this case the two pictures which show the objects single, are in the centres of the retinæ. Now in this phænomenon it is evident that the two centres of the retinæ are on corresponding points.

"Secondly,—Pictures of objects seen double, do not fall upon points of the retinæ similarly situate with respect to the centres of the retinæ.

"Thirdly,—The apparent distance of two objects seen with one eye, is proportioned to the arch of the retina which lies between their pictures: in like manner the apparent distance of two appearances seen with two eyes, is proportioned to the arch of either retina, which lies between the picture on that retina and the point corresponding to that of the picture on the other retina."

These facts are valuable for many reasons; but on no account more so, than because they serve to explain the manner by which nature yields the knowledge of external tangible figure, and the proportional motion which is in relation to it, by means of corresponding varieties of colour.

Dr. Reid's *arguments* (although he was in possession of these facts, which might have afforded premises for better reasoning) are altogether inconclusive, not to say puerile; and that on account of his steady adherence to the main object with which he set out upon his "Inquiry," namely, to show upon the principles of *common sense* whence comes the knowledge we have of the existence of an external universe: Following up these

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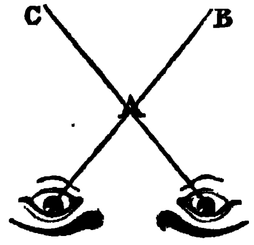
* See Dr. Reid's Inquiry, ch. vi. sec. 13.

† Ibid.

principles, he placed *visible* figure beyond the body, at a distance from the perceiving mind, denying it to consist either in a sensation, impression, or idea,—and as possible to be *seen* without the intervention of colour*.

It appears to me strange, when contradiction is stamped upon the very expressions which convey these ideas, that Dr. Reid's notions should seem to be the data for the reasonings of the author of the "*Explanation of Erect and Single Vision,*" published in the "Library of Useful Knowledge."

I must however, in common honesty, here take notice of an objection which I have known to be made to the views I entertain on this subject: it is, "that we see objects in different directions by either eye, when the other is alternately opened or closed." This objection appears to me perfectly nugatory, when it is considered, that both eyes being opened together, they are allowed by the condition of the question to be *directed to one point;* in which case *neither* of them can be directed *to any point beyond that point;* it would be a contradiction in terms to admit it. The axes cannot *cross* each other, and look *at points beyond the given point,* and that with a separate consciousness in the mind of so doing; for then these would not be merely *one* given point, but *three* given points; and the figure, the cause of whose single vision is in question, would be supposed to be placed, and at the same time supposed *not* to be placed, at the junction of the axes. For instance, when two eyes are directed to A, the left eye cannot be turned to B, look by itself at B, and the right eye at the same time be made to look by itself at C. Experience shows this to be an impossibility; but when either eye is shut, the other may be moved in any direction we please. However, were I in error in this statement, the argument of my objector would by no means be conclusive against my doctrine of single vision, provided only that A be placed at the junction of the axes; for the utmost which could happen would be, that A plus B, plus C would appear to the mind; but *not* two A's (two B's and two C's), because there would still be only a superposition, or increment of the colouring of A.



The central point of the colour of A would coincide on each retina; the whole of the rest of the colouring in relation to it would be painted on corresponding points, and coincide on their respective retinæ; and there could in no wise arise that proportional variety of colour, painted between the interior horizontal edges of the two A's, which is necessary to yield the ideas of their separate figures.

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* See Dr. Reid's "Inquiry," ch. vi. sect. 12. p. 135. 12mo.

But to return "to the explanation of the Cause of Erect and Single Vision, published by the Society for the Diffusion of Useful Knowledge*;"—it appears to me to be as much at variance with a sound, metaphysical, demonstrable conclusion concerning the nature of the perception of visible figure by the mind, as are the authors to whom I have alluded; and as much so with an acknowledged law,—with a proved physical fact, in respect to the *time* required for the motion of light.

The author of the "Explanation of the Cause of Erect and Single Vision," says†: "As the lines of *visible direction* cross each other at the *centre* of visible direction, an erect object is the necessary result of an inverted image;" but this is not the same thing with THE PERCEPTION *of an erect object.* If it be said the word *perception* is understood though not expressed, then the mind is supposed to see the very erect object, out of itself, at a distance from itself; that is, the mind feels colour, perceives visible figure (its result), *there, where it is not,* which is impossible.

Again, it is a known fact, that the light emitted from the sun, employs about eight minutes in its journey to the earth. Now let an object be seen at that distance in an erect position, but the moment after its light is effused, let it be obliterated: the mind will still see it erect, eight minutes after its annihilation, how then shall it signify the drawing of any rays back through a centre, towards the place of an obliterated object, which *once* stood there erect? The author's explanation is little more than the very circumstance in question, re-stated by an inversion of words. "An erect object (at a distance) is the result of an inverted image on the retina by the crossing of rays at a centre;" is merely saying over again, *that an inverted image on the retina is the result of an erect object at a distance,* when rays cross at a centre.

The question still remains untouched and unexplained; namely, why does the mind perceive an erect image, the result of an inverted image, which inverted image is the proximate cause of vision, and not the erect object which might be obliterated without affecting the mental consciousness of it? The only answer appears to me to be that, which I have formerly stated in my "Essay on Single and Erect Vision;" viz. "*Inversion of figure is merely a* RELATIVE *quality: when all rays from every object within the sphere of vision become inverted on the retina, there truly can be no mental consciousness of any in-*

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* Optics, part ii. "Library of Useful Knowledge." † Ibid.

version whatever: for there is no relative variety by comparison with any other set of similar images; and they will necessarily bear a given relative proportion to the ideas of motion and tangibility; and which ideas, taken collectively, include all the ELEMENTS *we have of the knowledge of the position, figure, and colour of objects**."

No doubt the relations of these in indefinite modifications are perceived by the judgement, as well as innumerable associations of them by the imagination; and thence the large use of vision in the world; thence the warm affections which are approved of by the understanding, or delighted in by the fancy.

But instead of taking this simple and easy mode of viewing the subject, philosophers, when they discuss the reason of erect vision, really suppose (although they may not be willing to allow it in so many words), that mental vision arises from, and is occupied about, *two* sets of objects at the same time; viz. the *external objects in nature*, and the *inverted images of them on the retina:* whereas the external object becomes virtually null and void immediately upon the rays of light being emitted from it.

The idea of inversion is the result of the comparison of the line of demarcation of one object with that of another of a similar kind placed in a contrary direction to it. But as in the picture on the retina, the line of demarcation of each particular image touches the line of demarcation of the rest, in the same manner and after the same proportion as their corresponding objects do in external nature; so no such comparison can take place: for *one* set of images only is painted, and these in precisely the same relative positions to each other as are their counterparts. The mind therefore necessarily perceives the same positions with respect to each other; for no *two* objects of a kind present themselves, by which a comparison can take place.

Philosophers, therefore, when they compare the image on the eye of an ox, for instance, with the object in external nature of which such image is the reflection, forget that both together make but one picture on their own eyes: For any given object forms on the human eye an inverted image, and the mind sees it erect; but the image on the eye of the ox (which is already inverted) makes on the eye of the person who observes it, an image again inverted that is erect, and the mind perceives it *inverted.*

In this latter case there is a comparison of the line of demar-

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* See "Essays on the Perception of an External Universe," &c. by Lady Mary Shepherd. 1827. Essay, xiv. p. 408.

cation of one object with that of another of a similar kind, placed in a contrary direction to it. In the former case, *two* objects do not present themselves, but only *one* of a kind, and that surrounded by each and every line of demarcation, precisely in the same relations to each other as are those of external nature. The same observation holds good when drawings are used with two images on them, placed contrariwise to each other; as an arrow without the retina, and an arrow within the retina. Did the arrow within the retina *feel* along with the surrounding lines of colouring, there could be no *sense* of an inverted arrow; for there would exist no reference to another arrow, which reference is only made by the observer, who is looking on *two* arrows.

Observations analogous to these must be made on the attempted explanation " of the cause of single vision," by the same author, who says*, " Because the lines of visible direction from similar points of the image (on *one retina*) meet the lines of visible direction from similar points of the other *image* upon the *other retina*, each pair of similar points must be seen as *one* point." How so, when the mind sees not *out* of itself *at the junction of the points*, and when if the object which sent forth the rays were annihilated, there would still result a *single vision from separated points of colour painted on* SEPARATED RETINÆ *at a distance from each other;* such duplicate separate figures on the retinæ being the proximate cause of the single vision of the object, and *not the junction of similar points*, when rays are drawn *back* again from the retinæ to such points of junction.

The question still recurs, and is still untouched and unexplained; *Why are pairs of points perceived by the mind as single points?* No doubt the determination of rays upon the retinæ in such a manner that when drawn back again they will meet at a central point, is a property closely connected with the method of vision; but it is rather *a corollary or consequence of the manner of the entry of the rays at the pupil of the eye* by which equal arches are subtended upon the retina, than the efficient cause of either *single* or *erect* vision. I again ask, Why are *two* objects on the retinæ perceived as only *one* object by the mind? For it is *not* a junction of *external* points which is perceived, but *two* sentient retinæ determine two separate images (equally perfect in their form, equally brilliant in their colouring), as but *one* image to the mental capacity of perception. Is not the answer, *Because there are no points of*

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* Optics, part ii. " Library of Useful Knowledge."

colouring painted on either retina, by which the separation of their forms can be distinguished?

Press the axis of either eye sufficiently to the right or left, a larger quantity of colouring will immediately be painted upon one retina than upon the other, which will separate their interior and horizontal edges, and *two* images will thence immediately and necessarily arise upon the perception of the mind.

I feel convinced that the more these ideas are contemplated, and the more clearly they are apprehended, the better will they serve to elicit the reason of several other phænomena concerning vision, which it has hitherto been considered difficult to explain; and what is of still greater importance, they may throw some light upon those which belong to every analogous operation of the human senses and intellect.

LXIV. *On the Reaction of effluent Water, and on the Maximum Effect of Machines. By* Mr. P. Ewart[*]. *With Notes relating to the Theory of Barker's Mill. By* J. Ivory, *Esq., M.A. F.R.S.*[†]

THE following important proposition relating to this subject, is laid down by Daniel Bernoulli in his "Hydrodynamics," page 278. If a jet of water I (fig. 1.) issue from the side of a vessel A, with the velocity which a body would acquire in falling freely from the surface B to C, he says the *repulsion* of the water in the opposite direction to the jet will be equal to the weight of a column of water, of which the base is equal to the section of the contracted vein, and the height equal to 2 BC.

This question respecting the amount of what has been termed the "reaction of the effluent water," derives additional interest from the circumstance of its having particularly engaged the attention of Sir Isaac Newton, and from his having given a solution

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[*] From a paper "On the measure of moving force," in the Memoirs of the Lit. and Phil. Society of Manchester, second series, vol. ii. 1813.

We insert this extract because it treats, correctly we believe, of subjects which have engaged the attention of many eminent mathematicians (in former times as well as recently), whose reasonings and conclusions on the points in question are at variance with each other. These discrepancies are to be regretted, inasmuch as some of the essential points in the application of the principles of mechanics to practical purposes are involved in them.—Edit.

[†] Communicated by the Author.

We are glad to lay before those of our readers who have attended to the various intricate theories that have been offered of the action of Barker's Mill, Mr. Ivory's Notes on that subject.—Edit.

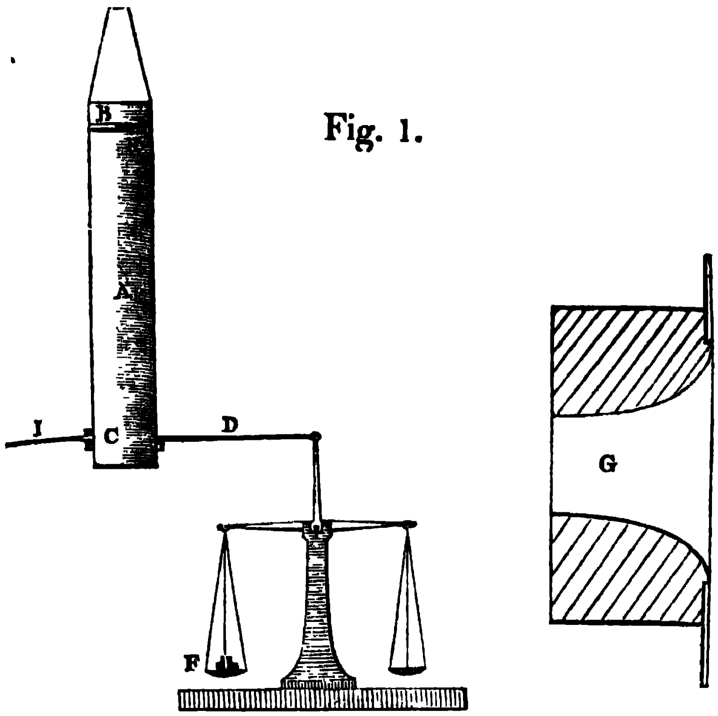

Fig. 1.

solution of the problem in the first edition of the "*Principia*," which he materially altered in the succeeding editions. In the first edition (book 2nd, prop. 37.) he infers, that the reaction is equal to the weight of a column of water of which the base is equal to the area of the orifice, and the height equal to that of the surface of the water above the orifice. In the succeeding edition, the subject is more fully discussed in the 36th prop. of the second book, where he infers (cor. 4.) that, when the area of the surface B is indefinitely large compared with that of the orifice, the reaction is, what it was afterwards in a different manner demonstrated to be by D. Bernoulli. Sir Isaac Newton further observes, that he found, by admeasurement, the area of the orifice in a thin plate to be to that of the section of the contracted vein, at the point of its greatest contraction, in the ratio of $\sqrt{2} : 1$ nearly. He takes the reaction, therefore, to be greater than what he understood it to be when he published the first edition, in the ratio of $\sqrt{2} : 1$ nearly. He refers, however, more to experiment than to theory for a solution of this question; and many valuable experiments have since been made on effluent water; yet I cannot find that the results of any direct experiments have been published which go to determine the precise amount of this reaction.

Sir Isaac Newton suggested (*Principia*, first edit. p. 332.) a method by which the reaction may be easily measured. If the vessel be suspended like a pendulum, he observes, it will recede from the perpendicular in the opposite direction to the jet.—I have made some experiments on a vessel suspended in that manner; and in order to ascertain the reaction as accurately as possible, I made use of a balance-beam furnished with a perpendicular arm of the same length as the horizontal arms, as represented at fig. 1. The scales were exactly balanced, and the end of the rod D made just to touch the side of the vessel.—The orifice was then opened, and the water in the vessel was kept uniformly at the same height by a stream falling gently on the plate E. The scale F having been raised by the reaction of the jet, weights were put into it till it was brought exactly to the position in which it was before the orifice was opened. The diameter of the vessel was 7 inches, and the height BC exactly 3 feet. I tried orifices of various diameters from ·35 to ·7 of an inch. Their exact diameters were ascertained by a micrometer, and the time carefully observed in which 30 lbs. of water were discharged through each orifice.

When the orifice was made in a thin plate ($\frac{1}{30}$th of an inch in thickness), I found the reaction to be greater than that stated in Sir Isaac Newton's first conclusion, in the ratio of 1·14 to 1. There was some variation in the results of the experiments. The greatest reaction, however, was as 1·16 to 1, and the least as 1·09 to 1, which fall far short of Sir Isaac Newton's last inference. The velocity of the water at the orifice (ascertained by observing the time in which 30 lbs. were discharged) was less than that which a body would acquire in falling freely from B to C, in the ratio of ·6 to 1.

I found no constant ratio to subsist between the diameter of the contracted vein and that of the orifice; and observing considerable opacity in the jet at the contracted vein, I concluded it to be divided into a number of different filaments, and I gave up all hopes of ascertaining the actual area of the section of the stream at that place by measuring its diameter. After repeated trials I found that when the water issued through a contracted hole, of the shape represented at G, the jet was quite transparent, and the reaction (taking the mean of 12 experiments with 4 different orifices) was less than the weight of a column of water of twice the height of the head and diameter of the smallest part of the hole, in the ratio of ·865 to 1. The least reaction was as ·85 to 1, and the greatest as ·88 to 1. By measuring the quantity of water delivered in a given time, I found the velocity of the jet, at the smallest part of the orifice, to be less than that which a body would acquire in falling freely from B to C, in the ratio of ·94 to 1. The highest ratio was as ·95 to 1, and the lowest ·89 to 1.

From these results it appears, that when the contracted vein is not opaque, and when its velocity is nearly equal to that which is due to the head, the reaction is nearly equal to what it was concluded to be by Sir Isaac Newton and M. Dan. Bernoulli; and the great apparent difference between Sir Isaac Newton's first and second conclusions arises from his having been misled by some experiments to which he alludes. He says—"Per experimenta vero constat, quod quantitas aquæ, quæ, per foramen circulare in fundo vasis factum, dato tempore effluit, ea sit, quæ cum velocitate prædicta," [viz. the velocity due to the head] "non per foramen illud, sed per foramen circulare, cujus diametrum est ad diametrum foraminis illius ut 21 ad 25, eodem tempore effluere debet*." We must presume, however, that he refers to experiments made by others; for if he had made them himself, he would, no doubt, have arrived at the same results which have since been so well established by various authors, and he would have stated the above ratio to be as 19·5 to 25 nearly.

But his demonstration of the reaction requires that the velocity at the contracted vein shall be equal to that which is due to the head. Now that velocity cannot be determined by measuring the imperfectly contracted vein in cases of water spouting through a hole in a thin plate.

We may safely indeed infer, that, in such cases, the velocity is considerably less than what is due to the head. For, the jet being opaque, some moving force must be expended in separating the particles from each other, and the distance to which the jet from such an orifice is projected on a horizontal plane, confirms that inference. The demonstration, therefore, of the reaction, can be properly applied to such cases only as those where the water, issuing through a tube properly contracted, acquires the velocity nearly which is due to the head, and in those cases the experimental results agree, as I have stated, remarkably well with the demonstration.

These results agree also with the explanations which have been given of *moving force*†. If we suppose the velocity of the jet to be equal to that which is due to the head, and the vessel to move uniformly in the opposite direction CD with the same velocity; the water will be at rest as it issues.

Let a represent the area of the smallest section of the orifice. Then while the vessel has moved through a space = 2 BC, a quantity of water represented by $a \times 2\text{BC}$ has descended from B to C, and has been brought to rest. But the reaction is $= a \times 2\,BC$, and this multiplied by $2\,BC$, the space through which it has acted, gives $a \times \overline{2\,BC}|^2$ for the amount of the moving force produced, which is exactly the quantity of moving force necessary to raise the column $a \times 2\,BC$ to the height BC, and to project it with the velocity $2\,BC$. For, a moving force $= a \times 2\,BC \times BC$ will raise that column from C to B, and an equal moving force will generate the velocity $2BC$ in the same column, therefore $2a \times 2BC \times BC = a \times \overline{2BC}|^2$ is the whole moving force necessary to restore that column to the place and condition in which it was before it began to descend; and as no moving force has been expended in producing change of figure, that quantity of moving force must be found in the reaction of the water through the space which the vessel has moved while the water descended and was brought to rest.

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* *Principia*, edit. ii. lib. 2. prop. 36.

† By *moving force* is meant the product of the pressure into the space through which it acts, or of the quantity of water into the height through which it falls. The same sense in which the term is used by Euler.

Fig. 2.

Upon the same principle an easy and simple explanation may be given, I apprehend, of the action of the hydraulic machine called Barker's Mill. Let AB (fig. 2.) be the perpendicular tube, and BC the horizontal arm; let v express, in feet per second, the rotatory velocity of the arm at the orifice C, and let the water be supposed to issue with the velocity due to the pressure*. Put $g = 16\frac{1}{12}$ feet.

If BC be a cylindrical tube, and if q represent the quantity of water it contains from B to C, the centrifugal pressure upon a section of the arm at C, will be $\frac{qv^2}{4g\,BC}$; and whatever the length BC may be, the diameter remaining the same, q being as BC, the centrifugal pressure at C will always be as v^2; and it will be equal to the pressure of a perpendicular column of water whose height in feet is $\frac{v^2}{4g}$. Then if h express in feet the height AB of the water in the vertical tube, $h + \frac{v^2}{4g}$ will be the whole pressure at C; and if a express in feet the area of the most contracted section of the orifice, $2a\left(h + \frac{v^2}{4g}\right)$ will

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[*] It is here understood that the areas of the sections of the perpendicular tube and of the horizontal arm shall be indefinitely large when compared with the area of the orifice.

express the reaction, which being multiplied by v, the space through which it acts in a second, gives $2av\left(h+\frac{v^2}{4g}\right)$ for the *total* moving force of the arm in a second. But a part of this moving force is expended in producing the rotatory motion of the water, and in raising it to the height $\frac{v^2}{4g}$. For, if we suppose a perpendicular tube CP to rise from the arm at C, the surface of the water in that tube would stand at P, PR being $=\frac{v^2}{4g}$. Now if instead of letting the water escape at C, it be allowed to flow over the perpendicular tube at P, and fill another similar perpendicular tube adjoining it, and issue from an orifice at the bottom of that tube, the effect must be the same as if it issued at C, and a moving force must be expended at C, sufficient to generate the velocity v, in the water which passes, and also to raise it from R to P.

The pressure at C being equal to the weight of a column of water whose height is $h+\frac{v^2}{4g}$, (that is $=$ AB$+$PR), the velocity with which the water issues will be $\sqrt{4g\left(h+\frac{v^2}{4g}\right)}$ or $\sqrt{4gh+v^2}$. Let V express that velocity, then aV will express the quantity of water which passes in a second; and $2aV\frac{v^2}{4g}$ will express the moving force necessary to generate the velocity v, in that quantity of water, and to raise it from R to P. That quantity of moving force being deducted from the total moving force of the arm, leaves $2av\left(h+\frac{v^2}{4g}\right)$ $-2aV\frac{v^2}{4g}$ for the *effective* moving force of the arm in a second.

That this is the effective moving force, may be shown also in another manner, as follows:

The ***absolute*** velocity of the water after it has left the machine will be $V-v$, and $\frac{(V-v)^2}{4g}$ will be the head which would produce that velocity; which being multiplied by aV, the quantity of water delivered in a second, gives $aV\frac{(V-v)^2}{4g}$ for the moving force which remains with the water after it has left the machine.

If that be deducted from aVh, the whole moving force of the water, there will remain $aVh-aV\frac{(V-v)^2}{4g}$ for the *effective* moving force, which will be found to be equal to $2av\left(h+\frac{v^2}{4g}\right)$ $-2aV\frac{v^2}{4g}$, the *effective* moving force stated above.

The theory of this machine has occasionally occupied the attention of many distinguished mathematicians, and M. Euler has given two elaborate treatises on its principles in the Memoirs of the Berlin Academy for 1750, p. 311; and for 1751, p. 271. His demonstrations relating to this subject are very complicated, and they do not appear to have been adopted by succeeding authors*.

Mr. Waring, of America, has given quite a different theory, which has been approved of by several good writers on hydraulics. He concludes that the greatest effect will be produced when the velocity of the orifice is half that of the issuing water; and that this effect will be nearly the same as that of a well-constructed undershot water-wheel†.

The explanation which I have offered of the action of the water on this machine is different from any other that I have had an opportunity of consulting. I offer it, therefore, merely as an attempt to solve an intricate problem.

If it were possible for the water to issue with the velocity due to the pressure, it is obvious, if my explanation be right, that although a very large proportion of the moving force of the water may be communicated to the machine, moving with a moderate velocity, the maximum of effect can only be obtained by an infinite velocity. But when the water issues with a velocity which is less than what is due to the pressure, as must always be the case in practice, the velocity at which the maximum of effect is produced, may be found as follows. It should first be ascertained by experiment how near the issuing velocity can be brought to that which is due to the pressure. From the experiments which I have made, I have been led to conclude that no greater issuing velocity can possibly be obtained from a machine of this kind than what is due to ·8 of the pressure. If this conclusion be correct, it follows, that, whatever may be the issuing velocity of the water, a moving force, equal to $\frac{1}{4}$ of the moving force which is necessary to generate that velocity in the water, when falling freely, is expended in producing change of figure; that is, in forcing the water through the tubes and through the orifice C; and if the velocity of the machine be such that $PC = 5\,AB$, the issuing velocity will be equal to the velocity of the orifice, and the whole moving force of the water in descending from A to B will be expended in producing change of figure.

For, the head due to V, the issuing velocity, will in this case be PR, which is also the head due to v, the velocity of the orifice. We shall therefore have $V = v$; and if CP represent the total moving force necessary to raise the water from C to P, $CR = AB$ will represent that part of it which is expended in producing change of figure. The greatest velocity, therefore, that the orifice, when the machine meets with no resistance, can acquire, will be $\sqrt{4g \times 4h}$.

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* M. Euler says, "In employing the same quantity of water, and the same fall, this machine will produce an effect nearly four times greater than the ordinary machines."

† American Philos. Trans. vol. iii. p. 191 and 192.

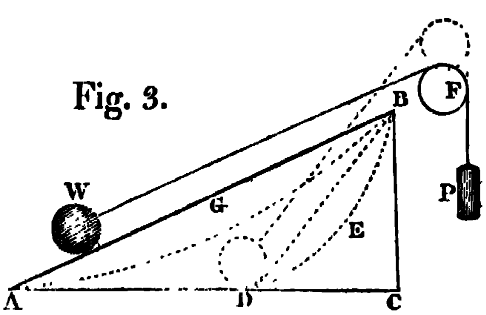
When the velocity of the orifice is less than that, V will be greater than v; and $V - v$, the absolute velocity of the water after it has left the machine, will be $\sqrt{\cdot 8(4gh+v^2)} - v$. The head or the moving force expended in producing that velocity will be as $\dfrac{\left(\sqrt{\cdot 8(4gh+v^2)}-v\right)^2}{4g}$.

The moving force expended in producing change of figure will be as $\cdot 2\left(h+\dfrac{v^2}{4g}\right)$. Now when the sum of these two quantities, or $\dfrac{\left(\sqrt{\cdot 8(4gh+v^2)}-v\right)^2}{4g} + \cdot 2\left(h+\dfrac{v^2}{4g}\right)$, is a minimum, we shall find $v = \sqrt{2gh(\sqrt{5}-1)} = 6\cdot 3056\sqrt{h}$ for the velocity of the orifice when the machine produces a maximum of effect; and in that case the above sum becomes $= \cdot 4472h$.

We shall therefore have $h - \cdot 4472h = \cdot 5528h$ for the maximum of effect, supposing h to represent the whole moving force of a given quantity of water descending from A to B. This effect is considerably greater than that which the same quantity of water would produce if applied to an undershot water-wheel, but less than that which it would produce if properly applied to an overshot water-wheel.

Respecting the maximum of effect produced by machines, I wish to observe, that in the actual construction of machines it is necessary to aim at a maximum quite different from that which is usually proposed in books on the theory of mechanics. This will perhaps be best explained by examining the simple case where a given weight P, (fig. 3) connected with another W, by a string passing over the pulley F, descends vertically and raises W, without friction, from the horizontal line AC along the inclined plane AB. If we make AB : BC : : 2 W : P, W will be raised to B in the least time;

Fig. 3.



time*; and upon this principle, the maximum of effect in machines is usually demonstrated in theory. In practice, however, the object is not merely to raise W to B in the *least time*, but to raise it with the least expenditure of *moving force*. When it is raised in the least time, P must descend through a space = AB, but when it is raised with the least moving force, P descends through a space = $\frac{1}{2}$ AB only. For, if we make BD = $\frac{1}{2}$ AB, and let W ascend along any concave surface DEB, of which BD is the chord, it will be raised to B by the descent of P through a space = BD, and it will be at rest when it arrives at B. This is so obvious, that it would be superfluous to give a demonstration of it. It appears then, that twice the quantity of moving force which is absolutely necessary to raise W to B, must be expended if it is to be raised by P in the least time. To determine the curve by which W will ascend from D to B in the least time, is an intricate problem, and I do not know that it has ever been solved†; but a practical approximation to it in any particular case may be easily found. A well constructed steam-engine for raising water exhibits in every stroke a practical example of the same problem. At the commencement of the stroke, a very great pressure of steam is thrown upon the piston, and this pressure is gradually diminished, so that at the end of the stroke there is a considerable preponderance in the opposite direction. In consequence of this regulated pressure of the steam, the motion of the machine resembles the uniform vibrations of a pendulum, and the moving force of the steam is applied to the greatest advantage.

By proceeding on the principle that when W is raised to B in the least time, the maximum of effect is produced, many erroneous conclusions have been drawn respecting the proper construction of machines. It is laid down for example, on this principle, that " In an overshot water-wheel, the machine will be in its greatest perfection, when the diameter of the wheel is two-thirds of the height of the water above the lowest point of the wheel ‡." But it is very well known that there would be lost, by that construction, nearly one-third of the moving force of the water, which is saved by making the wheel one-half larger in diameter, and by making its velocity much less than what is required by the above rule.

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* If the ascent be made in the least *possible* time, W must ascend not along the plane AB, but along a concave surface AGB.

† This difficult problem, we understand, has lately been solved by the Rev. E. Sibson of Ashton, in Makerfield, Lancashire, and the solution will appear in the next volume of the Memoirs of the Lit. and Phil. Society of Manchester.—Edit.

‡ Gregory's Mechanics, vol. i. p. 447.

NOTES relating to the Theory of *Barker's Mill.* *By* J. IVORY, *Esq. M.A. F.R.S.*

I. *On the Pressure caused by the Centrifugal Force.*

The centrifugal pressure may be investigated in the manner following.—The whole pressure at the orifice is the sum of the variable pressures of all the molecules, or infinitely small portions of the fluid, in the length of the arm. Let r be the length of the arm between the orifice and the axis; $g = 16\frac{1}{12}$ feet; v, the absolute velocity of the orifice in feet: then if dx (or $\dot{x}$) be a small portion of the fluid in the arm at the distance x from the axis, the centrifugal pressure of dx will be

$$\frac{x^2 v^2}{.} \times \frac{dx}{2xg} - \frac{v^2 . x\,dx}{2g r^2},$$

and the pressure of the prism of the fluid, of which the length is x, will be $= \frac{v^2 x^2}{4g r^2}$; and the whole pressure of all the fluid in the length r, will be $= \frac{v^2 r^2}{4g r^2} = \frac{v^2}{4g^2}$.

The same thing may be more shortly stated, thus: The centrifugal force of every small portion of the fluid in the arm being as the distance from the axis, we may assume that every portion is acted upon by the force which takes place at the middle of the arm, or at the distance $\frac{1}{2} r$ from the axis. Now this force is $= \frac{v^2}{4gr}$; which multiplied by r, the sum of the portions of the fluid, gives $\frac{v^2}{4gr} \times r = \frac{v^2}{4g}$, the same as before.

And since we have,

$$\frac{v^2}{4g} = \frac{r^2}{4g} \times \left(\frac{v}{r}\right)^2;$$

$\frac{v}{r}$ being the *angular velocity* of the machine, or the angle through which it turns in a second; it follows that the angular velocity being the same, the centrifugal pressure varies as the square of the length of the arm.

Some authors, and in particular Bossut (*Hydrod.* tom. i. § 429), reckon the centrifugal pressure, not in the whole length r, but in the distance between the upright tube and the orifice in the horizontal arm. Let r' be the radius of the upright

right tube; then, according to the authors mentioned, the centrifugal pressure at the orifice will be equal to

$$\frac{v^2}{4g} \times \frac{r^2 - r'^2}{r^2},$$

being the difference between the pressures at the distances r and r', from the axis.

II. *Velocity with which the Fluid issues from the horizontal Arm, supposing that v is the* absolute Velocity *of the Orifice.*

The velocity required is produced by the pressure of the fluid in the upright tube increased by the centrifugal pressure. Wherefore if h denote the length of the upright tube, supposed to be kept constantly full, the sum of the two pressures mentioned will be $= h + \frac{v^2}{4g}$; and, if V denote the velocity of the effluent water, we shall have,

$$V^2 = 4g\left(h + \frac{v^2}{4g}\right) = 4gh + v^2.$$

And, if we put $v^2 = 4gf$, then

$$V^2 = 4g(h + f).$$

III. *Demonstration of Daniel Bernoulli's Proposition respecting the Reaction of effluent Water* (or, *Cor.* 2. *prop.* 36. *lib.* 2. Principia).

Suppose that water issues from a small orifice in the side, or bottom, of a vessel which is kept full; let k be the height of the surface above the level of the orifice: then, $2\sqrt{gk}$ is the velocity in a second with which the water issues; and, if a be the area of the orifice, the quantity of water discharged in a second is equal to the prism $a \times 2\sqrt{gk}$; and, as the water issues with the velocity $2\sqrt{gk}$, the quantity of motion in the water discharged in a second is equal to,

$$a \times 2\sqrt{gk} \times 2\sqrt{gk} = 2ak \times 2g;$$

and the same quantity of motion; viz. $2ak \times 2g$, is evidently the reaction of the water projected in a second. But the prism or weight, $2ak$, by falling, produces a quantity of motion equal to $2ak \times 2g$ in a second: wherefore the reaction of the projected fluid is equal to the weight $2ak$; that is, to the weight of a prism of the fluid having its base equal to the orifice a, and its altitude equal to $2k$.

This proposition supposes that the water has acquired its complete velocity of projection, due to the head k. Before the efflux commences, and while the fluid is at rest, the weight $a\,k$ is sufficient to counteract the tendency to begin motion.

IV. *Computation of the Moving Force of Barker's Mill, or of the impulse produced by the Reaction of the projected Water; supposing that h is the length of the upright Tube kept constantly full, and v the absolute Velocity of the Orifices in the horizontal Arms.*

This machine has generally two horizontal arms diametrically opposite; but it may have any number of such arms with an orifice in each. Whatever be the number of the arms, I shall suppose that all the orifices are at the same distance, r, from the axis; and I shall use the symbol a to denote the sum of the areas of all the orifices.

The velocity V with which the water issues from the orifices is known by the formula,

$$V^2 = 4gh + v^2 = 4g(h+f).$$

And, since the water is propelled from the orifices with the velocity V, and has acquired from the machine the velocity v in the opposite direction, the velocity with which it is projected from the arms, is equal to $V-v$: but aV is the quantity of water so projected in a second; wherefore the momentum of the projected water is equal to $aV\times(V-v)$; and the same expression is equal to the impulse communicated to the machine by reaction. Wherefore, since the impulse is exerted at the end of the lever r, its effect to turn the machine, or the motive force, is equal to

$$r \times aV \times (V-v). \qquad (1)$$

We may likewise measure the motive force by the momentary impulse multiplied by the space through which it acts; and the force accelerating the machine will be,

$$v \times aV \times (V-v). \qquad (2)$$

These two formulæ seem to be the most elementary expressions of the motive force of this machine.

Mr. Ewart's Formula.

Let us put $$\frac{aV\times(V-v)}{2g} = P;$$

then, $aV \times (V-v) = P \times 2g$; wherefore the reaction is equal to the momentum which the weight P acquires in a second by falling; in other words, the momentary impulse of reaction is equal to the pressure of the weight P. The motive force of the machine, according to the formula (2), is therefore equal to $P \times v$. Now

$$P \times v = 2av \times \frac{V^2}{4g} - 2aV \times \frac{v^2}{4g};$$

and, by substituting the value of V^2,

$$P \times v = 2av\left(h \times \frac{v^2}{4g}\right) - 2aV\,\frac{v^2}{4g}.$$

Euler's Formula.

In the formula (1), put $2\sqrt{g(h+f)}$ and $2\sqrt{gf}$ for V and v; then the impulse to turn the machine will be,

$$2ar \times 2g\left(h + f - \sqrt{hf + f^2}\right),$$

which is Euler's formula.

N. B. It must be observed that Euler makes gravity the unit of forces; that is, he denotes it by 1, and expresses the velocities by the square roots of the heights: and, in order to make the foregoing expression agree with Euler's assumption, we must make $2g = 1$; because $2g$ has been taken for the measure of the force of gravity.

Bossut's Formula.

This author considers the machine in a form more complicated than we have here contemplated; but, when we make allowance for the peculiar mechanism he supposes, the result of his investigation will be found to agree with the expression (1).

V. *Effect of the Machine.*

We have found that the moving power of the machine is $P \times v$; and

$$P \times v = v \times \frac{aV \times (V - v)}{2g} = aV \times \frac{v^2}{2g} \times \left(\frac{V}{v} - 1\right).$$

Now $a \times V$ is the prism of water that issues from the orifices in a second, which quantity we shall denote by Q; then the equation,

$$Q = a \times V \qquad \text{(A)}$$

will express the relation between the water expended, the velocity of expenditure, and the sum of the areas of the orifices. Further, put

$$x = \frac{v}{V} = \frac{\sqrt{f}}{\sqrt{h+f}},$$

and x will be a number less than 1: then

$$f = \frac{x^2 h}{1-x^2}$$

$$\frac{v^2}{2g} = 2f = \frac{2x^2 h}{1-}$$

$$- - 1 = \frac{1-x}{-};$$

wherefore, by substitution,

$$P \times v = aV \times \frac{v^2}{2g} \times \left(\frac{V}{v} - 1\right) = Qh \times \frac{2x}{1+x}.$$

If we make $x = 1$, which supposes that f and v are infinitely great, then $P \times v = Qh$; and the effect of the machine would be equal to the whole mechanic power in the quantity of water Q falling from the head h. This is an unattainable limit; but the nearer x is to 1, the greater will be the moving force of the machine; which seems to be the only general rule we can have to guide us in the construction of this machine. Having pitched upon the most convenient value of x; then,

$$V = \frac{2\sqrt{gh}}{\sqrt{1-x^2}},$$

$$v = 2\sqrt{gh} \times \frac{x}{\sqrt{1-x^2}},$$

$$a = \frac{Q}{} \times \sqrt{1-x^2},$$

effect of the machine, $P \times v = Qh \times \frac{2x}{1+x}$. If $x = \frac{1}{2}$, that is, if V be double of v, then the effect of the machine is $\frac{2}{3} \times Qh$.

In what goes before we have taken the full velocity due to the head, which is always greater than the real velocity in practice. But although the real velocities are less than according to theory, yet they still nearly follow the same proportion; that is, their squares are as the pressures, or as the heights of the head. We may therefore assume

$$V^2 = 4g \, . \, m\,(h+f),$$

m being a quantity less than 1, to be determined experimentally. It is evident that this assumption does not affect the equation (A). We shall now have,

$$v = 2\sqrt{gf},$$

$$V = 2\sqrt{gm(h+f)},$$

$$x = \frac{v}{V} = \frac{\sqrt{f}}{\sqrt{m(h+f)}},$$

$\frac{v^2}{2g}$

$$\frac{v^2}{2g} = 2f = 2h \times \frac{mx^2}{1-mx^2}$$

$$P \times v = aV \times \frac{v^2}{2g} \times \left(\frac{V}{v} - 1\right) = Qh \times \frac{2m(x-x^2)}{1-mx^2}.$$

Now it is obvious that the expression $\frac{x-x^2}{1-mx^2}$ is susceptible of a maximum; and, by applying the usual rule, the value of x answering to the maximum will be found; viz. $x = \frac{1-\sqrt{1-m}}{m}$. This expression shows that, whatever fraction of unit m stands for, x is contained between the limits $\frac{1}{2}$ and 1*; so that, when the machine works to the greatest advantage, v is greater than $\frac{1}{2}$ V and less than V. With a given value of m, the rules for constructing the machine so as to work to the greatest effect, are contained in the following formulas, in which $n = \frac{1}{m}$, viz.

$$x = \frac{1-\sqrt{1-m}}{-},$$

$$v = 2\sqrt{gh} \times \frac{x}{\sqrt{n-x^2}},$$

$$V = 2\sqrt{gh} \times \frac{1}{\sqrt{n-x^2}},$$

$$a = \frac{Q}{2\sqrt{gh}} \times \sqrt{n-x^2},$$

effect of the machine, $P \times v = Qh \times (1 - \sqrt{1-m})$†. It now appears that, m falling short of 1, the effect of the machine decreases rapidly. If $m = \cdot 8$, then $P \times v = Qh \times 0\cdot 5528$, or $P \times v = \frac{5}{9} \times Qh$, nearly.

If $m = \frac{1}{2}$, that is, if half the pressure were lost in forcing the water through the tubes, the effect of the machine would be reduced to $Qh \times 0\cdot 293$, or nearly $\frac{3}{10} \times Qh$.

$$m \qquad \cdot \;\; \frac{}{8} \;\top\; \frac{}{16} + \frac{5m^3}{128} + \&c.$$

when $m = 0$, $x = \frac{1}{2}$; when $m = 1$, $x = 1$.

†

$$x = \frac{1-\sqrt{1-m}}{m},$$

$$mx^2 = 2x - 1,$$

$$1 - mx^2 = 2(1-x)$$

$$\frac{2m(x-x^2)}{1-mx^2} = \frac{2m(x-x^2)}{2(1-x)} = mx = 1 - \sqrt{1-m}.$$

LXV. *On*

LXV. *On the Figure of the Earth, as deduced from Measurements of the Meridian.* By J. IVORY, *Esq. M.A. F.R.S.*[*]

IN my last communication I have shown that the five portions of the meridian which have been most exactly measured, belong to one and the same elliptical spheroid, although they occupy very dissimilar situations on the earth's surface. The almost perfect precision with which the elliptical elements agree with the actual measurements, is indeed not a little remarkable, and seems hitherto to have escaped notice. In returning to this subject, which is of great importance in the question about the figure of the earth, my intention is to discuss it more thoroughly, by examining it in every point of view that may throw light upon it.

The equations deduced from the five measurements are as follows (Phil. Mag. for May, pp. 345, 346):

$$
\begin{aligned}
188510 &= \Delta\,(3{\cdot}117500 - 6{\cdot}2261\,.\,\epsilon + 3{\cdot}095\,.\,\epsilon^2)\\
598630 &= \Delta\,(9{\cdot}895891 - 18{\cdot}1985\,.\,\epsilon + 6{\cdot}163\,.\,\epsilon^2)\\
751567 &= \Delta\,(12{\cdot}370302 - 6{\cdot}2811\,.\,\epsilon - 10{\cdot}466\,.\,\epsilon^2)^{\dagger}\\
172751 &= \Delta\,(2{\cdot}840172 - 0{\cdot}3844\,.\,\epsilon - 2{\cdot}166\,.\,\epsilon^2)\\
98870 &= \Delta\,(1{\cdot}622022 + 0{\cdot}8378\,.\,\epsilon - 0{\cdot}022\,.\,\epsilon^2).
\end{aligned}
$$

In solving these equations I before supposed that the elements sought were approximately known; but I shall here proceed by a more direct method, which is independent of any previous knowledge of the elements. For this purpose, assume,

$$\Delta = \mathrm{R}\,(1 + \mathrm{A}\epsilon + \mathrm{B}\epsilon^2),$$

R, A and B being quantities to be determined: substitute the value of Δ in every one of the five equations, neglecting the powers of ϵ above the square; and, having added all the resulting expressions into one sum, equate the coefficients of ϵ and ϵ^2 to zero: then,

$$
\begin{aligned}
1810328 &= 29{\cdot}845887 \times \mathrm{R}\,; & \mathrm{R} &= 60655{\cdot}9\\
29{\cdot}8459 \times \mathrm{A} &= 30{\cdot}2523, & \mathrm{A} &= 1{\cdot}0136\\
29{\cdot}846\ \mathrm{B} &= 30{\cdot}252\ \mathrm{A} + 0{\cdot}396, & \mathrm{B} &= 1{\cdot}141
\end{aligned}
$$

and, consequently,

$$\Delta = \mathrm{R}\,(1 + 1{\cdot}0136\,.\,\epsilon + 1{\cdot}141\,.\,\epsilon^2).$$

It is obvious that R is the radius of the circle in which the sum of the lengths of all the arcs answers to the sum of their amplitudes. The element Δ now depends upon ϵ, which is the only unknown quantity. In order to find ϵ, substitute the value of Δ in every one of the five equations, bringing all the terms to one side after having divided by R; by which procedure we shall obtain these five equations,

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[*] Communicated by the Author.

[†] In the Phil. Mag. for May, the coefficient of ϵ^2 is 6·266; but the inadvertence does not affect any of the results.

$$\begin{aligned}
x^{(1)} &= {\cdot}009642 - 3{\cdot}0662 \,.\, \varepsilon + 0{\cdot}341 \,.\, \varepsilon^2 \\
x^{(2)} &= {\cdot}026612 - 8{\cdot}1680 \,.\, \varepsilon - 0{\cdot}992 \,.\, \varepsilon^2 \\
x^{(3)} &= {\cdot}020364 - 6{\cdot}2574 \,.\, \varepsilon + 2{\cdot}719 \,.\, \varepsilon^2 \\
x^{(4)} &= {\cdot}007875 - 2{\cdot}4944 \,.\, \varepsilon - 0{\cdot}684 \,.\, \varepsilon^2 \\
x^{(5)} &= {\cdot}007992 - 2{\cdot}4819 \,.\, \varepsilon - 2{\cdot}678 \,.\, \varepsilon^2
\end{aligned}$$

The symbols $x^{(1)}$, $x^{(2)}$, &c. stand for the errors, or rather, for the quantities which the want of perfect exactness in the experimental numbers, makes it necessary to supply in order that the same value of ε may produce an equality in all the five expressions. The best mode of solution is to employ the method of the least squares, or to determine ε so as to satisfy this equation, viz.

$$x^{(1)} \frac{dx^{(1)}}{d\varepsilon} + x^{(2)} \frac{dx^{(2)}}{d\varepsilon} + \&c. = 0.$$

Thus we have,

$$\begin{aligned}
0 = &({\cdot}009642 - 3{\cdot}0662 \,.\, \varepsilon + 0{\cdot}341\, \varepsilon^2)(-3{\cdot}0662 + 0{\cdot}682\, \varepsilon) \\
&+ ({\cdot}026612 - 8{\cdot}1680\, \varepsilon + 0{\cdot}992\, \varepsilon^2)(-8{\cdot}1680 + 1{\cdot}984\, \varepsilon) \\
&+ \&c.
\end{aligned}$$

All the operations being performed, this final equation will be obtained, viz.

$$0{\cdot}413829 = 127{\cdot}666 \,.\, \varepsilon + 4{\cdot}795 \,.\, \varepsilon^2;$$

from which we get $\varepsilon = {\cdot}00324$. And if this value of ε be substituted in the foregoing expression of Δ, we shall find $\Delta = 60655{\cdot}9 \times 1{\cdot}003295 = 60855{\cdot}7$, or 60856. The direct method of calculation here followed, has therefore brought us to the same elements as before, which, as has already been shown, represent all the five measurements with very small errors*.

It is easy to verify the value that has been found for Δ; namely, by combining the original equations so as to eliminate the terms containing ε, or so as to render the same terms so small that they may be neglected. Thus, if the expression of the French measurement be multiplied by 2·9, we shall have,

$$2179544 = \Delta(35{\cdot}8739 - 18{\cdot}2152 \,.\, \varepsilon - 30{\cdot}351 \,.\, \varepsilon^2);$$

and, if the Indian expression be subtracted from this, the remainder will be,

$$1580914 = \Delta(25{\cdot}9780 - 0{\cdot}0167 \,.\, \varepsilon - 36{\cdot}514 \,.\, \varepsilon^2),$$

from which we get $\Delta = 60856$ as before. We are therefore sure that there is no uncertainty in the values that have been assigned to the two elementary quantities.

The elements of the elliptical meridian being known, we may thence deduce its curvature at certain fixed points; and, in order to judge more accurately of the near agreement of the measurements with the theory, we may compare them separately with the curvature of that portion of the meridian to which they are nearest in situation. For this purpose I shall use the notation $D(\lambda)$ to denote the length of a degree of which the middle point is placed in the latitude λ. The extreme latitudes of the degree will therefore be $\lambda+\frac{1}{2}$ and $\lambda-\frac{1}{2}$; and, in the formula (A)*, we shall have $n = 1°$, $p \sin n = 1°$, $\cos n = 1$, $m = 2\lambda$; consequently,

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* Phil. Mag. for May, p. 346.

$$D(\lambda) = \Delta\left\{1 - \varepsilon\left(\tfrac{1}{2} + \tfrac{3}{2}\cos 2\lambda\right) + \varepsilon^2\left(\tfrac{1}{16} + \tfrac{15}{16}\cos 4\lambda\right)\right\}. \quad \text{(C)}$$

In this expression put λ successively equal to 0°, 45°, 90°; and we shall have, for the length of 1°,

Fathoms.

At the equator, $D(0°) = \Delta(1 - 2\varepsilon + \varepsilon^2) = 60462{\cdot}4$

At latitude 45°, $D(45°) = \Delta(1 - \frac{1}{2}\varepsilon - \frac{7}{8}\varepsilon^2) = 60757$

At the pole, $D(90°) = \Delta(1 + \varepsilon + \varepsilon^2) = 61054$

Let us now compare the arcs measured in Peru and India, with the curvature of the meridian at the equator. From the expressions just set down, we get,

$$\Delta = D(0°).(1 + 2\varepsilon + 3\varepsilon^2);$$

and if we substitute this value in the two measurements mentioned, we shall get these three equations, the last being the sum of the other two, viz.

$$188510 = D(0°)\ (3{\cdot}11750 + 0{\cdot}0089.\varepsilon - 0{\cdot}005.\varepsilon^2)$$

$$598630 = D(0°)\ (9{\cdot}89589 + 1{\cdot}5933.\varepsilon - 0{\cdot}546.\varepsilon^2)$$

$$787140 = D(0°)\ (13{\cdot}01339 + 1{\cdot}6022.\varepsilon - 0{\cdot}551.\varepsilon^2)$$

In the first of these expressions the terms containing ε may be neglected, and $D(0°)$ will be found equal to 60468, or near 6 fathoms too long, which arises from the Peruvian arc being about 20 fathoms too long. If we substitute the value of ε in the two remaining expressions, we shall get 60461·3 and 60462·9 for the value of $D(0°)$, hardly different from the length previously deduced from the two elements.

The French arc is nearly bisected by the parallel of 45°; and it must be compared with the curvature at that latitude. Now,

$$\Delta = D(45°).(1 + \frac{\varepsilon}{2} + \frac{9}{8}\varepsilon^2);$$

and, this value being substituted in the expression of the measurement in question, we shall obtain,

$$751567 = D(45°).(12{\cdot}3703 - 0{\cdot}0960.\varepsilon + 0{\cdot}310\,\varepsilon^2).$$

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* Phil. Mag. and Annals for May, p. 344.

Here the factor on the right-hand side is but slightly affected by the terms containing ε, which reduce it to 12·37; and D (45°) is found equal to 60757, the same value derived from the two elements, and very nearly the same that results from the actual measurement without any assumption about the figure of the earth*. It is therefore certain that the elements which have been found represent with great exactness both the whole arc between Dunkirk and Formentera, and also its mean curvature. I have likewise found that the same elements represent, with small errors, the partial arcs between Dunkirk and Montjouy, and Montjouy and Formentera. But it is well known that the four sections of the arc between Dunkirk and Montjouy are very irregular; and the causes of the irregularity have not been very well explained. Whatever these causes may be, they extend their influence to the measurements that have been made in England. Unless I have been misinformed, a re-measurement between Greenwich and Paris has lately been executed, the results of which, when made public, will probably help to clear up the perplexing anomalies that occur in the comparison of the partial arcs.

We may now compare the Swedish, or most northerly, arc with the curvature at the pole. We have,

$$\Delta = D\,(90^\circ) \times (1-\varepsilon);$$

and, by substitution, we get,

$$98870 = D\,(90^\circ)\,.\,(1\cdot622022 - 0\cdot7842\,.\,\varepsilon - 0\cdot860\,.\,\varepsilon^2).$$

From this equation, taking the foregoing value of ε, D (90°) will be found equal to 61051, or only three fathoms less than the value deduced from the two elements; and even this difference will disappear, if we take into account the error of the arc itself.

If in the expression (C) we make

$$\text{Cos}\ 2\,\lambda = -\tfrac{1}{3},\ \lambda = 54^\circ\ 44',$$

we shall get $\cos 4\,\lambda = -\frac{7}{9}$, and

$$D\,(\lambda) = \Delta(1 - \tfrac{2}{3}\,\varepsilon^2) = \Delta.$$

It appears, therefore, that a degree of the meridian in Britain, very nearly midway between York and Edinburgh, is equal to a degree of the earth's equator. Although the Trigonometrical Survey has been extended between the extreme north and south points of the island, yet, as far I know, the calculations have not been completed except for a small part of the meridian between Dunnose and Clifton. We cannot therefore verify what has just been shown by comparing it with any actual measurement in the proper latitude; but we may in some measure be enabled to judge of its exactness by employing the portion of the meridian that has been calculated. Let λ' represent the mean latitude between Dunnose and Clifton, then

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* Phil. Mag. and Annals for May, pp. 346, 347.

$$\lambda = 54^\circ\ 44'\ \ 0''$$
$$\lambda' = 52\ \ \ 2\ \ 20;$$

and, by means of formula (C), we shall readily obtain,

$$D(\lambda')-D(\lambda) = \tfrac{3}{2}\,\Delta\,\varepsilon\,(\cos 2\lambda - \cos 2\lambda'),$$

the term containing ε^2 being insensible. Now, by dividing the length of the English arc by its amplitude, we get $D(\lambda) = 60824{\cdot}1$; and hence,

$$D(\lambda') = 60824{\cdot}1 + 26{\cdot}6 = 60850{\cdot}7,$$

which is only five fathoms less than the exact value of Δ, and this deficiency is caused by the small error of the arc, which is too short.

It follows, from the foregoing minute examination, that the five arcs are represented with great precision by the elements that have been found; and further, that each arc, taken separately, agrees accurately with the curvature of that portion of the meridian in which it is situated. And there cannot be a more satisfactory way of proving that the meridian of the earth is an ellipsis, than by showing that it coincides with that figure, at the equator, at the mean latitude of 45°, at the parallel of 54° 44′ where the curvature is identical to the equatorial circle, and at the pole. We have briefly endeavoured to accomplish this task as far as the measurements in our possession put it in our power to do so; but the proof would have been much more complete, if we could have confirmed it by the length of the meridian through the whole extent of Britain.

To the fixt points on the meridian that have already been mentioned, we may add another intermediate between the equator and the parallel of 45°. In the expression (C), put,

$$\text{Cos}\ 2\lambda = +\tfrac{1}{3},\ \lambda = 35^\circ\ 16';$$

then, $$D(\lambda) = \Delta\,(1-\varepsilon-\tfrac{2}{3}\varepsilon^2) = \Delta\,(1-\varepsilon).$$

At this latitude, which is the complement of 54° 44′, the radius of curvature of the meridian is equal to half the polar axis. The length of the degree depends upon the elements of the ellipsis; and it may serve as a quantity of reference for judging of the consistency of any measurement made near its parallel of latitude. In the present state of this research there is no measurement that can be compared with it. The nearest to it is the degree of La Caille at the Cape of Good Hope, in latitude 33° 18′; but this is so disproportionately great, that no use can be made of it.

May 12, 1828. J. IVORY.

LXVI. *On the characteristic Equation of Curve-surfaces capable of being evolved in a Plane.* By M. FAYOLLE.

To the Editors of the Philosophical Magazine and Annals.

Messieurs,

CE n'est pas sans surprise que j'ai lu, dans votre dernier N°. du *Philosophical Magazine*, le passage suivant, p. 334:

$$\frac{d^2 z}{d x^2} \cdot \frac{d^2 z}{d y^2} - \left(\frac{d^2 z}{d x d y}\right)^2 = 0,$$

"an equation which, indeed, has long been known, but in the opinion of Mr. Gauss *has never yet been rigorously demonstrated.*"

Les géomètres savent que Monge et Meusnier ont donné des démonstrations *rigoureuses* de cette équation différentielle partielle du second ordre, qui appartient aux surfaces développables. Pour s'en assurer, je renvoie vos lecteurs aux volumes 9 et 10 de la collection des *Mémoires des Savans étrangers.*

Ce 15 Mai, 1828. FAYOLLE,

Ancien chef d'étude à l'école Polytechnique.

LXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Being enabled to resume our regular reports of the proceedings of this Society, we commence at present with a list of the papers read before the Society during the present session, which have not hitherto been noticed in the Philosophical Magazine.]

Dec. 20, 1827.—RESEARCHES to discover the faculties of pulmonary absorption with respect to charcoal. By G. Pearson, M.D. F.R.S. —A Catalogue of nebulæ and clusters of stars in the southern hemisphere, observed at Paramatta in New South Wales. By James Dunlop, Esq. in a letter addressed to Sir Thomas M. Brisbane, Bart. Communicated by Mr. Herschel.

Jan. 10, 1828.—On the life of plants and animals. By Sir G. Gibbs, M.D. F.R.S.—Observations on the comparative magnetic intensity shown by a horizontal needle at the bottom and on the tops of mountains at Port Bowen and Spitzbergen. By Captain Henry Foster, R.N. F.R.S.

17.—On Capt. Parry's and Lieut. Foster's experiments on the velocity of sounds. By Dr. G. Moll, Professor of Natural Philosophy in the University of Utrecht. Communicated by Capt. Henry Kater, V.P.R.S.—An account of a series of experiments, made with a view to the construction of an achromatic telescope with a fluid concave lens, instead of the usual lens of flint glass. In a letter addressed to the President. By P. Barlow, Esq. F.R.S.

24.—On the structure and use of the capsulæ renales. By Sir Everard Home, Bart. V.P.R.S.—Abstract of a meteorological journal kept at Benares in the years 1824, -25, and -26; with remarks. By James Prinsep, Esq. Communicated by Dr. Roget.—Description of a percussion rifle, igniting by a spring instead of a lock. By Lieut.-Col. Miller, F.R.S.*

31. Feb. 7.—An account of trigonometrical operations in the years 1821, -22, and -23, for determining the difference of longitude between the royal observatories of Paris and Greenwich. By Capt. Henry Kater, V.P.R.S.

14.—On the mode in which the nerves belonging to the organs of sense terminate. By Sir E. Home, Bart. V.P.R.S.—Experiments on heated iron, in reference to the magnetic and electric fluids. By William Ritchie, Esq. A.M. Rector of the Royal Academy of Tain. Communicated by Captain Sabine.

April 24.—A paper was read, containing An account of experiments on the elastic curve. By B. Bevan, Esq. Communicated by the President.

In inquiries on the strength of materials, it is often desirable to know the real nature of the curve assumed by a prismatic rod, when acted upon by the weight of its own parts. This curve has generally been stated to be the parabola; but repeated observation has led the author to doubt the accuracy of the theory from which this conclusion has been deduced; and with a view, therefore, to determine, by direct trial, the real form of the curve, he instituted a series of experiments on prismatic rods, of various substances, and of various depths and lengths, some fixed at one end, and others supported at both ends, in a horizontal position. In every instance he found the actual curve to differ from a parabola, and the deviations in the several points examined were such as indicated a regular and determinate species of curve. No modification of the exponent of the order of the parabola was adequate to express the relation of the coordinates with sufficient accuracy in all cases. He found, however, after many trials, that the following formula, which is that of the common hyperbola, gave a very near approximation in all practical cases, namely, $Ax^2 + Bx = y^2$

The accurate determination of the elastic curve is a subject of some importance in practical mechanics; since the rules at present used by mathematicians and engineers for determining the modulus of elasticity of different materials, are founded upon the parabolic theory, and must therefore be liable to error.

May 1.—A paper was read, intitled, " A description of a vertical floating collimator, and an account of its application to astronomical observations, with a circle, and with a zenith telescope. By Capt. Henry Kater, V.P.R.S.

The construction of the instrument which forms the subject of this paper is a material improvement on that of the horizontal floating collimator, of which an account was given by the author in the Philosophical Transactions for 1825. Its superiority is derived from its adaptation to the vertical, instead of the horizontal, position; by which the sources of error arising from the necessity of transferring the instrument to different sides of the observatory, and of taking the float out of the mercury and replacing it at each observation, are wholly obviated. The vertical floating collimator has the further advantage of being adapted for use, not only with a circle, but also with a telescope, either of the refracting or reflecting kind. Such a telescope, furnished with a wire micrometer, and directed to the zenith, becomes a zenith telescope, free from all the objections to which the zenith sector, and the zenith telescope, with a plumb-line, are liable.

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* See Phil. Mag. and Annals, vol. iii. p. 277.

The instrument itself is supported on a square mahogany stand, which slides on two parallel beams, fixed at the upper part of the observatory, in the direction of the meridian, and which has a circular aperture in the centre, having at its edge a projecting rim of iron, to admit of the passage of the telescope. The telescope having a focal length of eight inches, and an aperture of one inch and a quarter, is supported in the vertical position by a bridge, connecting it with a circular iron ring, ten inches and six-tenths in diameter, which floats in mercury. The mercury is contained in a circular iron trough, the central aperture of which is sufficiently large to allow of its turning freely round the rim which rises from the margin of the aperture of the stand. The object-glass of the telescope is placed at its lowest end, and its focus is occupied by a diaphragm, composed of two brass plates, each cut so as to form an angle of 135 deg. and placed opposite to each other, so that the angular points are brought to an accurate coincidence, thus leaving on each side intervening spaces, which form vertical angles of 45 deg. each. The telescope below, whether belonging to a circle or a zenith telescope, is to be directed so that the image of these angles shall be bisected by the micrometer wire; for which purpose the diaphragm of the collimator is illuminated by a bull's-eye lantern, placed at a convenient distance upon one of the beams crossing the observatory; the light being reflected downwards by a plane mirror placed in a screen, with a suitable aperture immediately above the collimator. The collimator is then to be turned half round in azimuth, the motion being facilitated by rollers, and limited at its extent by two catches, which receive a projecting wire fixed to the outer circle of the trough. When in this situation, the observation of the diaphragm by the telescope, and the bisection of its angles, are to be repeated, and the mean of the two positions will indicate the exact point of the zenith. Minute directions are given by the author for the construction of all the parts of the collimator, and for their proper adjustments; together with an account of the precautions to be taken in the employment of the instrument. The time required for completing the determination of the zenith point by its means need not exceed two minutes; and if to this be added the time necessary for a second set of observations of the same kind, for the purpose of verification, and of a nearer approach to accuracy, the whole time required will not be more than five minutes, during which it is not probable that any sensible disturbance can have taken place in the position of the instrument from changes of temperature.

Tables are given containing registers of numerous series of experiments made, both by the author and by several of his friends, with a view to determine the stability of the instrument, and the degree of reliance that can be placed in the results. In the first series, out of sixty independent determinations of the zenith point, there are twenty-five, the error of each of which does not exceed one-tenth of a second; thirty-seven under two-tenths; forty-seven under three-tenths; fifty-five under four-tenths; three between four and five-tenths; and two a little above half a second. But the author thinks it probable that the greater part of these errors, minute as they are, must be attributed to want of power in the micrometer, which power is directly as the focal length of the object-glass, or mirror, of the telescope to which it is attached, and which necessarily limits the precision of which it is capable.

The author next gives the results of some experiments with a collimator made for Captain Foster, having a float of only five inches in diameter, and with a telescope five inches long: the errors, generally, do not amount to more than two-tenths of a second.

He then enters into details as to the manner of using the vertical floating collimator in astronomical observations, beginning with the portable azimuth and altitude circle described by the Rev. F. Wollaston in his *Fasciculus Astronomicus*, and applicable to other similar instruments. The new collimator affords, also, the most perfect method of adjusting the line of collimation of a mural circle, or of placing it at right angles to the axis.

The author next proceeds to describe the method of applying the instrument to the zenith telescope. In comparing the observations made by the zenith sector belonging to the Board of Ordnance, with the zenith telescope used in conjunction with the vertical floating collimator, the mean of errors in the former case was $+0''{\cdot}54$ and $-0''{\cdot}75$; in the latter, $+0''{\cdot}44$ and $-0''{\cdot}66$. From observations made on γ *Draconis*, the zenith distance of which at Greenwich is $0° 2' 6''{\cdot}36$, and at York Gate $0° 0' 35''{\cdot}67$, the difference of latitude between the two places was found to be $0° 2' 42''{\cdot}03$, that of Greenwich being $51° 28' 38''{\cdot}96$, and of York Gate $51° 31' 20''{\cdot}99$. The decimals of a second, by the azimuth and altitude circle and horizontal floating collimator were ·94; by the same instrument and the vertical floating collimator ·76, and by the zenith telescope, and the vertical floating collimator ·99: the mean being $\cdot 9''$.

From the greater degree of precision attainable by the employment of the vertical floating collimator, from the facility of its construction and application, and the time saved by using it, the author deems it not unreasonable to infer, that, ere long, the use of the level and plumb-line in celestial observations will be wholly abandoned.

LINNÆAN SOCIETY.

May 4.—The reading of Mr. Morgan's paper On the mammary organs of the kangaroo was continued, containing further particulars of the dissection of these parts, as well as of the muscles attached to the marsupial bones, in the adult and impregnated animal. These bones, with their ligamentous and muscular connections, were described, and several errors in Sir Everard Home's published account of these parts were pointed out. The author then stated his own opinions respecting the use of these structures. He stated that the marsupial bones are formed, 1st, for the purpose of giving that firm support to the superincumbent abdominal viscera which the narrow pelvis of the animal is incapable of affording while in the erect posture; and, 2ndly, for the purpose of constituting a fixed point of resistance against which the mammæ are squeezed by the muscular girdle already described as inclosing those glands between their fibres. By this arrangement the female is enabled to empty by compression the excretory ducts of its mammæ, and thus to force their secretions into the mouth of the imperfectly organized young, which during the earlier periods of its existence appears incapable of extracting a nutritious fluid from that part by the usual means.

It appears that the secretion of this fluid (or milk) takes place only in the larger and lower gland, and that its ejection through the inferior and longer teat is assisted by a muscular investment which incloses the ducts throughout their whole course from the gland to the extremity of the nipple. The existence of this structure has been noticed by M. Geoffroy St. Hilaire, who has assigned to it the same use. Under this compressing muscle of the lower (or as Mr. Morgan has named it, the true marsupial) teat, a congeries of vessels which principally consisted of veins was described as forming a plexus around the central fasciculus of ducts. These veins, together with those of the gland, were stated to occasion a considerable distention of the mammary organ during the time of suckling, in consequence of the congestion which must necessarily occur in the vessels at that period, from the pressure made upon their main trunks by the action of the compressing muscle of the mamma; for it has been found that the size of the organ on such occasions exceeds that which a loaded state of the ducts only could produce. The mammæ were found, as in the virgin animal, to consist in double glands on each side, the upper and smaller presenting the same anatomical characters as in the former instance; its excretory ducts, however, in their course towards the upper nipple were found to be inclosed in an indistinct muscular sheath, and there was a faint indication of the existence of a plexus of vessels similar to that which was found in the lower or true marsupial teat. This smaller mammary organ is considered by the author as analogous to the supernumerary mammæ and teats of other mammiferous animals, since the lower or true marsupial mammary glands and their teats appear to perform exclusively the office of preparing a nutritious fluid for the support of the young animal.

May 24.—This day the Anniversary Meeting of the Society took place, at which Edward; Lord Stanley, was elected President, in the room of the late Sir J. E. Smith; and Edward Forster, J. E. Bicheno, and R. Taylor, Esqs. respectively re-elected to the offices of Treasurer, Secretary, and Under Secretary.

GEOLOGICAL SOCIETY.

March 21.—Benjamin Silliman, M.D. LL.D. of Yale College, North America, was elected a Foreign Member of this Society. Francis Finch, Esq. and Thomas Winter, Esq., both of West Bromwich, Staffordshire, were elected Fellows of this Society.

A paper was read, intitled "Topographical and Geological Notices, from information collected during the Expedition to the North-west coast of America under the command of Captain Franklin; by John Richardson, M.D. F.R.S., &c."

The expedition under Captain Franklin having arrived at their intended winter-quarters on the shore of Great Bear Lake, examined in 1825 the vicinity of that lake, and the course of the Mackenzie River from thence to the sea. The author subsequently accompanied Captain Franklin down the river, as far as Point Separation, in lat. 67° 38′; from whence the latter proceeded westward to lat. 70° 26′, long. 148° 52′:—the extreme western point seen by the expedition being in long. 149° 37′ west. Dr. Richardson at the same time went eastward to the mouth of the Copper Mine River, and thence returned overland and across the Great Bear Lake, to the head-quarters.

This paper contains an account of the specimens collected, and the geological observations made by both divisions of the party; and gives in considerable detail a description of the vicinity of Great Bear Lake, with a more general one of the banks of the Mackenzie and of the coast to the East of it; to which are subjoined some observations respecting the country previously passed over by the expedition, between Lake Superior and Fort Franklin. The distances traversed being, in latitude, about 23 degrees N. of Lake Superior; and in longitude, altogether about 80 degrees;—60 degrees to the west of Lake Superior, and 20 degrees on the coast, eastward from the mouth of the Mackenzie. The total extent passed over in America by the expedition, in going and returning, was about 14,000 miles; and that surveyed and laid down for the first time on the maps, is about 5000 miles.

The author however mentions, that a very limited portion of his time could be devoted to geological researches; the ground being for eight months in the year covered with snow; and the other objects of the journey demanding his principal attention during the short summer.

The country described consists, in general, of three or four formations, or series of beds, which occupy well marked divisions:—

1. The most western division comprehends the Rocky Mountains, which appear to be composed of primitive rocks; and the course of the ranges is from about S.E. to N.W.; the faces of the hills to

the eastward being abrupt, but the slope towards the W. more gradual. These mountains join the sea on the west of the Mackenzie; and at their termination are divided into four groups or chains, to which Captain Franklin has given the names of *Richardson's*, *Buckland's*, the *British*, and *Romanzoff* chains. The land again becomes lower to the west of the chain last mentioned, and continues to be so from thence to the remotest point arrived at; no prominent elevations having been observed to the west of long. 146°.

2. Another very extensive tract of primitive rocks in the north of America has nearly the same direction with the range of the Rocky Mountains, but the two ranges converge towards the north; the distance between them being, in lat. 50°, 700 miles;—about 220 miles where it was traversed by Captain Franklin, in going from Hudson's Bay to Lake Winipeg;—and in lat. 66° only 200 miles. This eastern primitive tract consists principally of granite and gneiss; it exhibits great uniformity of character, contains no very elevated ground, and is in fact traversed by several rivers which arise in the Rocky Mountains. It is flanked on both sides by extensive calcareous tracts.

3. The north-eastern extremity of the Rocky Mountain chain, near the mouth of the Mackenzie, consists of grauwacke and other transition-rocks, interposed apparently between the primary and the calcareous districts. In some of the other places described, a rock resembling the old-red-sandstone of England, occupies a similar situation.

4. The tract that intervenes between the Rocky Mountains and the eastern primary band above mentioned, consists principally of calcareous strata, and is remarkable from its including, throughout, a series of great lakes or lake-like rivers, with which a very large proportion of the surface is occupied, and the bottoms of which appear in several instances to be below the level of the sea. This intermediate calcareous band was traced in one place by the author, to the width of about 280 miles from the eastern primary tract; and one of its highest summits, about a mile from Bear Lake, was supposed to be about 950 feet above the sea. The limestone of which this district is composed, as well as that of the calcareous tract on the east of the primary band above mentioned, presents considerable uniformity of character: the ridges of hills are nearly parallel to those of the Rocky Mountains; and a very large proportion of the rocks observed by the author, was found to be magnesian limestone,—apparently belonging either to the magnesian limestone formation of England, or to our mountain-limestone, which it is well known includes in Europe numerous beds of dolomite.

The fossils also of this calcareous formation, are of the same genera with those of our mountain-limestone and of the magnesian beds in the north of England; including corallines, productæ, terebratulites, and a cardium: and in several places the calcareous beds contain a large proportion of chert and flinty slate. The correct determination of the relations of this great calcareous tract, is one of the chief points of interest remaining for future research, in the country described by the author; for while he agrees with other geologists in assigning a portion of it, (as in the vicinity of Lake Winipeg,) to the mountain-limestone of Europe, he justly remarks that in other places the quantity of gypsum, in connection with copious salt springs, and great abundance of petroleum, together with the occurrence of soft marly-sandstone, and beds of breccia interstratified with those of dolomite, and above all, the fact that dolomitic limestone is by far the most common and extensive rock in the deposit, would lead to its identification with the zechstein of continental geologists,—the magnesian limestone of the North of England.

5. Above the limestones, and in some cases, it would appear, alternating with the dolomite, is a very extensive deposition of sandstone, bituminous-shale, and slaty-clay (which last exhibits in some places the peculiar structure denominated cone-in-cone) containing nodular ranges of clay-iron-stone and beds of lignite. The shales include impressions of ferns, lepidodendrons, and other vegetable remains; and among the fossils of this formation was also found an ammonite, supposed by Mr. Sowerby to belong to a part of the oolitic series of England. It deserves inquiry therefore, whether this may not be the equivalent of the carboniferous strata which form a portion of the oolitic series in Yorkshire, and at Brora in Scotland.

The series of beds above described occurs extensively in the course of the Mackenzie River, and on the shores of the Great Bear Lake; and from its being found also on the northern coast, at a distance of about 300 miles from thence, and in a direction precisely corresponding, it not improbably occupies the intervening country.

About Cape Bathurst (lat. 70° 36′, long. 127° 35′) cliffs of alum shale form the coast for more than 60 geographical miles, and are described as resembling those of Whitby in Yorkshire.

6. On the promontory of Cape Lyon are extensive ridges of columnar trap associated with limestone and slate-clay; and greenstone is of frequent occurrence there and in some other places. Porphyry also, forms low conical hills in the high ground between the Copper Mountains and Bear Lake.

7. Near the western boundary of the limestone, and not far from the base of the Rocky Mountains, there occur at intervals, from lat. 50° to 69° N. extensive (*tertiary?*) deposits, consisting generally of sandstone, gravel, clay more or less bituminous, and brown wood-coal. In some spots the wood-coal was replaced by an excellent pitch-coal, the fractured surface of which is marked with very peculiar concentric semicircular depressions; and it is interesting to know that this coal, which would be excellent fuel for a steam-vessel, occurs on the coast of the Polar sea near the Mackenzie in considerable quantity. This formation contains layers of a variety of pipe-clay which is eaten by the natives, and is said to sustain life for a considerable time. The deposit at the mouth of Bear Lake River includes some beds of impure porcelain earth. The author found occasionally much difficulty in distinguishing the sandstones and shales of this deposit, from those of the formation mentioned above in Section 5.

8. Among the indications of other strata more recent than the magnesian limestone, was a loose fragment of soft limestone found

at the mouth of Babbage River, on the coast west of the Mackenzie, containing the species of Cyclas (C. medius) which occurs extensively in the weald-clay of England.

This memoir, which will be published in full in the Appendix to Captain Franklin's Narrative of the expedition, is illustrated by maps and drawings, and accompanied by a catalogue in detail, of the specimens referred to, which have been presented to the Geological Society.

April 18.—William Hutton, Esq. of Newcastle-upon-Tyne, Beriah Botfield, Esq. of Christchurch Oxford, and William Parker Hamond, Esq. of St. John's College Cambridge, were elected Fellows of this Society.

A Paper was read, "On the fossil remains of two new species of Mastodon, and of other vertebrated animals, found on the left bank of the Irawadi; by William Clift, Esq. F.G.S. F.R.S., conservator of the Museum of the Royal College of Surgeons."

The author having been requested to describe the fossil remains which the zeal and liberality of Mr. Crawfurd have transferred from the deserts of the Irawadi to the Museum of the Geological Society, confines himself strictly to zoological and anatomical details; and following the system of Cuvier, commences with the

Pachydermata proboscidifera.—The only genus of this order indicated by the remains is the Mastodon; and of this there are two species, Mastodon latidens and Mastodon elephantoides, not only commanding attention from their novelty, but from the beautiful gradation which they exhibit between the mastodons already described and the elephant. On comparing the teeth of Mastodon latidens with those of the mastodon of the Ohio (M. giganteum) the denticules are found to be more numerous, and less distant, and the interstices less deep than in those of the latter. The teeth, in short, begin to assume the appearance of those of the elephant. On advancing to Mastodon elephantoides, these features of similarity are more strongly developed: the many-pointed denticules are still more numerous and more compressed; and the structure, were it not for the absence of crusta petrosa, becomes almost that of the tooth of the elephant. In both, though the teeth are formed upon the principle by which the tooth of the mastodon is distinguished from that of the elephant, the crown of the tooth wears away more like that of the elephant than that of the other mastodons.

The species are thus characterized:

Mastodon latidens.—Mastodon dentibus molaribus latissimis, denticulis rotundatis, elevatis. Palato valdè angusto.

The dentition very much resembles that of the elephant. The molar tooth is gradually pushed forward, and rises as the fangs are added, according to the demand occasioned by the abrasion of the exposed crown, and the consequent absorption of the anterior fang; the posterior part of the tooth not having yet cut the gum, while the anterior portion is completely worn away. Before it are seen the relics of the preceding tooth, the place of which the tooth in use was progressively supplying.

The lower jaw in this species is less square and deeper than it is in M. giganteum.

The tusks, judging from the alveoli, must have been of equal volume with those of the largest living elephant.

The following is the measurement of some of the remains of M. latidens.

Extreme breadth of fragment of cranium (upper jaw with the greatest part of both grinders)	1 Ft.	3 In.
Length of ditto	1	8
Extreme length of right anterior grinder (6 denticuli and the spur)	0	8¼
Extreme breadth at third denticulus	0	4
Circumference of lower jaw, measured over the grinding surface of the tooth	2	4
Extreme length of tooth	0	11¾
Extreme breadth	0	4½
Circumference of the lower extremity of right femur	2	2
Same, round the condyles	2	4

Mastodon elephantoides.—M. dentibus latis; denticulis numerosis, compressis.

This species must have been smaller than the last. There is a fine example of the lower jaw, showing the tooth in the highest degree of perfection. The tooth is 11 inches long and 3½ inches broad, has no less than ten denticules, and each of these denticules is mamillated with small points; five being the smallest, and eight the greatest number on any one denticule. In front of this tooth is seen the remnant of the preceding one, worn down and disappearing; and behind it is the cavity wherein the young tooth, intended as a successor to that in existence, was in the course of formation. The denticules are much more compressed than those in the species last described; they are closer together, and the whole tooth approaches still more nearly to that of the elephant, while the jaw is in unison with the appearance of the tooth.

Pachydermata ordinaria.—In this group we have the remains of the genera Sus, Hippopotamus, and Rhinoceros. Of the first there is only a single specimen, consisting of a small portion of the lower jaw, containing one molar tooth and the fragment of another. Of the second there are but few fragments, nor are they sufficiently characteristic to warrant a definition of the species, which must have been comparatively small. Of the third there is a portion of the upper jaw, containing two molar teeth; and portions of the lower jaw with molares, which seem to approach nearer to those of the rhinoceros of Java than to those of any other living species.

Ruminantia.—In this group we have fragments of the ox and of the deer.

REPTILIA.

Chelonia, Cuv.—(*Testudinata,* Bell).—There are many fragments of a large species of trionyx, and some of an emys. But the remains are not sufficiently defined for specific description.

Sauria.—Fam. *Crocodilidæ.*—Of this family we have the remains of two genera; viz. a Leptorhynchus allied to, if not identical with, the great gavial; and a crocodile resembling Crocodilus vulgaris. Of the former there are portions of the lower jaw and several vertebræ; of the latter, there is the anterior termination of the lower jaw, which must have belonged to a very large individual.

The specimens, in general, do not appear to have undergone any mineral change, with the exception of being abundantly penetrated with iron, and are very brittle. This last circumstance, arising from the loss of their animal gluten, indicates great antiquity, and that they have not been imbedded in any very compact soil; unlike the teeth of the mastodon of the Ohio, which lie in a strong blue clay, and have almost as much animal matter as is to be found in a recent tooth.

The bones are almost in every instance broken; and from the firmness of texture of most of them, the direction and cleanness of the fracture, and the sharpness of its edges, the injury, which must have been the result of an immense power operating with sudden violence, appears to have taken place at the period, or very soon after the period, of the destruction of the animal.

A paper was next read "On a collection of vegetable and animal remains, and rocks, from the Burmese Country, presented to the Geological Society by J. Crawfurd, Esq," by the Rev. W. Buckland, D.D. V.P.G.S. F.R.S. &c.

Mr. Crawfurd collected these specimens during his voyage up the Irawadi in a steam-boat, on an embassy to Ava, in the latter part of the year 1826. The author considers them to be of high importance, as affording an answer to the curious, and till now undecided question, whether there be, or be not, in the southern regions of Asia, any remains of fossil quadrupeds analogous to those which are found so widely dispersed in the diluvium of northern Asia, and of Europe and America.

The evidence which Mr. Crawfurd has imported, consists of several chests full of fossil wood and fossil bones, and of specimens of the strata that are found along the course of the Irawadi, from Prome up to Ava, being a distance of nearly 500 miles. The greater part of the fossil wood is beautifully silicified; other specimens of it are calcareous; they are mostly portions of large trees, both monocotyledonous and dicotyledonous, and were found along the whole valley of the Irawadi from Ava to Prome. The bones were all collected from a small district near some wells of petroleum, about half way between these towns, and on the left bank of the river. From Mr. Clift's examination, it appears, that although we have among them no remains of fossil elephants, we have the same fossil pachydermata that are found associated with elephants in Europe; namely, rhinoceros, hippopotamus, mastodon, and hog. We have also two or three species of ruminantia resembling the ox, antelope and deer, with the addition of the gavial and alligator, and two freshwater tortoises, namely, trionyx and emys.

The teeth of the mastodon belong to two unknown species of that genus, both of them approaching in size to the largest elephant. Mr. Clift has designated them by the names of *Mastodon latidens* and *M. elephantoides*. The teeth are from animals of all ages; and there are many fragments of ivory, derived probably also from the mastodon.

The remains of the mastodon are by far the most abundant in this collection, and amount to about 150 fragments.

Of the rhinoceros there are about 10 fragments.

Of a small species of hippopotamus, 2.

Of the hog, 1; and of the ox, deer and antelope, about 20.

Of the gavial and alligator, about 50.

Of the emys, 20; and trionyx, 10.

One fragment of emys is so large, that the animal of which it formed a part, must have been several feet in width.

The state of preservation of these bones is very perfect, from their being penetrated with hydrate of iron, and thereby rendered strong. Not one of them is silicified, though they have been erroneously stated to be so, in some of the periodical journals.

The district in which they were found is a little North of the town of Wetmasut, and is composed of barren sand-hills and beds of gravel intersected by ravines, and cemented occasionally into a breccia by carbonate of lime, and sometimes by hydrate of iron. Over the surface of these hills were scattered the fragments of bones and wood, some quite naked and loose, others half buried in the sand and gravel. Many fragments of wood lay also at the bottom of the ravines. About one-third of the bones have been slightly rolled; and the rest had all been broken before they were lodged in the places where Mr. Crawfurd found them, and where they appear to have been dispersed and buried, by the action of the same waters that produced the diluvial sand and gravel, whence they have since been washed out, and left bare by the action of rains and torrents.

Concretions of sand and gravel adhere to many of the bones, but they contain no traces of shells, and differ mineralogically from all the rock specimens in this collection, which we recognize as belonging to tertiary and freshwater strata.

Indications of freshwater formation were found in one spot only, not far from the fossil bones, and they consist of a marly blue clay, abounding with shells of a large and thick species of Cyrena.

The tertiary rocks are: 1st, a dark slaty limestone, containing many shells, that have been identified by Mr. Sowerby with those of the London clay. 2nd, a yellow sandy limestone containing shells, and resembling the calcaire grossier, and 3rd, a soft greenish sandstone resembling the sandy beds of our plastic clay formation.

This London clay and *calcaire grossier* afford an additional locality of these strata to those indicated by the specimens described by Mr. Colebrooke, in vol. i. Part 1, 2nd series of the Geological Transactions, —which had already established the existence of this formation in the N.E. border of Bengal.

Mr. Crawfurd states distinctly, that it is impossible to refer the situation of the bones, or the origin of the hills containing them, to any operations of the existing river: these hills are sixty feet above the level of its highest flood; the effect of its actual operations, he observes also, is distinctly visible in the shifting islands of mud and sand that abound along the whole course of the river within this high-flood level, and in the great alluvial delta that extends from a little below Prome to Rangoon and the gulf of Martaban.

The recent bones and recent wood which he observed to be stranded on some of these islands, were not in a state of progress towards becoming mineralized, but were falling rapidly to decay.

The existence of so many animal remains analogous to those that occur in the diluvium of Europe, in a matrix which so nearly resembles that diluvium, and which so decidedly differs from the alluvium, and freshwater, and tertiary strata of the adjacent country, seems to authorize us to refer this matrix to a similar diluvial deposit in the valley of the Irawadi, reposing irregularly upon the tertiary and other stratified rocks, that form the basis of that district.

Besides the tertiary strata above enumerated, there are specimens of grauwacke and transition-limestone from several distant points in the valley of the Irawadi between Prome and Ava, which render it probable that the fundamental rocks of this valley belong to the transition series.

On the north of Ava there are chains of primitive mountains abounding with statuary marble, associated, as usual, with hornblende and mica slate.

We may therefore consider it as now established, on the authority of Mr. Crawfurd's notes and specimens, that the Burmese country not only contains the remains of fossil animals above enumerated, but also affords examples of the following geological formations, which can be identified with those of Europe; namely—

1. Alluvium.
2. Diluvium.
3. Freshwater Marl.
4. London Clay and Calcaire grossier.
5. Plastic Clay, with its sands and gravel.
6. Transition limestone and grauwacke.
7. Primitive marble and mica slate.

On the same evening, after the ordinary business of the Society had been transacted, a special general meeting was held, when the President having stated, that the Lords Commissioners of his Majesty's Treasury had been pleased to transfer to this Society some of the apartments in Somerset House, formerly used as the Lottery Office, and lately in the possession of the Royal Society; and that a sum not less than 1000*l.* would be required for preparing the said apartments for the reception of the Society, and the removal of their Library and Collections:—

It was resolved unanimously,

I. On the motion of Davies Gilbert, Esq. M.P., Pres. R.S., seconded by Henry Warburton, Esq., M.P.,—That the thanks of this Society be given to the Right Honourable the Lords Commissioners of his Majesty's Treasury, for the grant which they have been pleased to make to this Society, of apartments in Somerset House.

II. On the motion of the Rev. Dr. Buckland, Professor of Geology at Oxford, seconded by the Rev. A. Sedgwick, Woodwardian Professor at Cambridge,—That the thanks of this Society be given to Davies Gilbert, Esq. the President, and to the Council of the Royal Society, for their aid and cooperation in obtaining from the Lords of his Majesty's Treasury a grant of the apartments in Somerset House.

III. On the motion of Robert Ferguson, Esq., seconded by Leonard Horner, Esq.,—That a Subscription be immediately entered upon, to defray the expense of the necessary repairs in the apartments recently granted to the Society in Somerset House, and of the removal thereto.

May 2.—At a special general meeting holden this day at one o'clock, for the purpose of electing a Member of the Council in the room of Ashhurst Majendie, Esq.; and also for electing a Secretary in the room of R. I. Murchison, Esq., and a Foreign Secretary in the room of Henry Heuland, Esq., who had retired from their respective offices;

It was resolved unanimously,

I. That the thanks of this Society be given to Ashhurst Majendie, Esq., retiring from the Council.

II. That the thanks of this Society be given to Henry Heuland, Esq., for his long services in the office of Foreign Secretary, and for the high regard which he has always manifested for the welfare of the Society.

III. That the thanks of this Society be presented to R. I. Murchison, Esq. on his retiring from the office of Secretary.

A ballot having been held for electing a Member of Council in the room of Ashhurst Majendie, Esq., the scrutineers reported that Dr. Henry Burton was duly elected.

A ballot having been held for electing a Secretary in the room of R. I. Murchison, Esq., and a Foreign Secretary in the room of Henry Heuland, Esq., the scrutineers reported that Dr. Burton was elected Secretary; and that R. I. Murchison, Esq., was elected Foreign Secretary.

At the Ordinary Meeting holden on the same evening, John Claudius Loudon, Esq., of Porchester Terrace, Bayswater; and Thomas Copeland, Esq., of Golden Square, were elected Fellows of this Society.

An extract of a letter was read from Lieutenant William Glennie, R.N., dated Mexico, May 6th, 1827, intitled "The Ascent of Popocatapetl."

Many contradictory reports having long existed respecting the volcanic nature of this mountain, the author felt desirous of ascertaining its actual condition in person.

The ascent commenced during the month of April 1827, from the village of Ameca, situated in the province of Puebla, and near the N.W. foot of the volcano, at an elevation of 8216 feet above the level of the sea, and distant 14 leagues from Mexico.

The author describes the sides of the mountain as thickly wooded with forests of pines, extending to the height of near 12,693 feet, beyond which altitude vegetation ceased entirely. The ground consisted of loose black sand of considerable depth, on which numerous fragments of basalt and pumice-stone were dispersed. At a greater elevation, several projecting ridges, composed of loose fragments of basalt, arranged one above another, and overhanging precipices 600 or 700 feet deep, presented formidable impediments to the author's progress; and, in one direction only, a ravine was observed to pass through these ridges, having its surface covered with loose black sand, down which fragments of rocks ejected from the crater continually descended.

After twelve hours of incessant fatigue the author gained the highest point of the mountain on the western side of the crater, 17,884 feet above the sea; at which station the mercury in the barometer subsided to 15·63 inches, and the temperature indicated by the attached and detached thermometers, was respectively 39° and 33° Fahr. at 5 o'clock P.M., when exposed to the direct rays of the sun. The plain of Mexico was enveloped in a thick haze, and the only distant objects visible at that time, were the volcanoes of Orizaba and Iztaccihuatl. The crater of Popocatapetl appeared to extend one mile in diameter, and its edges of unequal thickness descended towards the east. The interior walls consisted of masses of rock arranged perpendicularly, and marked by numerous vertical channels, in many places filled with black sand. Four horizontal circles of rock differently coloured were also noticed within the crater; and from the edges of the latter, as well as from its perpendicular walls, several small columns of vapour arose smelling strongly of sulphur. The noise was incessant, resembling that heard at a short distance from the sea shore during a storm; and at intervals of two or three minutes the sound increased, followed by an eruption of stones of various dimensions; the smaller were projected into the ravine before mentioned, the larger fell again within the crater.

The sensations experienced by the author were analogous to those usually felt by travellers at considerable elevations; viz. weariness, difficult respiration, and headache, the latter inconvenience having been first perceived at a height of 16,895 feet. Tobacco smoke and spirituous liquors were also found to produce an unusually rapid effect upon the sensorium.

At the same meeting a letter was read from J. B. Pentland, Esq., addressed to W. H. Fitton, M.D. P.G.S., respecting the fossil remains of some animals from the N.E. border of Bengal.

The author has discovered among the mutilated fragments of bones obtained from the tertiary deposits on the Bramahpootra River in the small state of Cooch-Behar,—presented to the Society some years ago, by David Scott, Esq., and referred to in a former volume of the Transactions *,—the remains of four distinct species of mammalia, making an interesting addition to the list already published by Mr. Colebrooke, viz.—

1. A species of the genus Anthracotherium of Cuvier, which the author proposes to distinguish by the name of A. Silistrense,—a specific denomination derived from one of the many names by which the

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* Geol. Trans. 2nd Series. vol. i. p. 135.

great Bramahpootra river appears to have been designated by ancient geographers.

2. A small species of the order Ruminantia allied to the genus Moschus.

3. A small species of herbivorous animal referable to the Pachydermata, but more diminutive than any of the fossil or living species of that family at present known.

4. A carnivorous animal of the genus Viverra.

ASTRONOMICAL SOCIETY.

March 14.—The first paper read this evening was the following: Ephemeris of the place of Encke's Comet, during the time of its reappearance at the end of the present year. Drawn up at the request of the Council of the Society. By F. Baily, Esq.

1828.	Æ.	Dec.
	h m s	° ′
Aug. 23	1 47 24	+22 43
27	1 47 8	23 20
31	1 46 12	23 57
Sept. 4	1 44 40	24 34
8	1 42 16	25 11
12	1 39 0	25 48
16	1 35 44	26 24
20	1 29 20	26 58
24	1 22 36	27 30
28	1 14 28	27 59
30	1 9 48	28 12
Oct. 2	1 4 48	28 23
4	0 59 16	28 32
6	0 53 24	28 40
8	0 47 4	28 45
10	0 40 16	28 47
12	0 33 4	28 47
14	0 25 24	28 43
16	0 17 24	28 36
18	0 9 0	28 25
20	0 0 16	28 9
22	23 51 12	27 50
24	23 41 52	27 25
26	23 32 24	26 56
28	23 22 44	26 23
30	23 13 0	25 44
Nov. 1	23 3 12	25 1
3	22 53 28	24 14
5	22 43 48	23 23
7	22 34 16	22 29
9	22 24 52	21 32
11	22 15 44	20 31
13	22 6 48	+19 29
Nov. 15	21 58 4	+18 25
17	21 49 36	17 20
19	21 41 20	16 13
21	21 33 16	15 5
23	21 25 20	13 57
25	21 17 36	12 48
27	21 9 56	11 38
29	21 2 20	10 27
Dec. 1	20 54 44	9 14
3	20 47 4	8 1
5	20 39 20	6 45
7	20 31 20	5 27
9	20 23 12	4 7
11	20 14 40	2 42
13	20 5 48	+ 1 14
15	19 56 28	− 0 20
17	19 46 40	1 58
19	19 36 20	3 42
21	19 25 32	5 32
23	19 14 20	7 28
25	19 2 48	9 28
27	18 51 16	11 31
29	18 40 0	13 35
31	18 29 28	15 38
1829.		
Jan. 10	18 4 24	24 14
13	18 10 16	25 50
16	18 20 12	26 57
19	18 31 4	27 43
21	18 39 20	28 4
26	19 0 40	28 24
31	19 21 4	28 19
Feb. 9	19 53 52	−27 34

The first part of the above ephemeris (to the end of the year 1828) is taken from Mr. Encke's own computations for Paris time $0^d.3$, as inserted in Professor Schumacher's *Astronomische Nachrichten*, No. 123. That part of it which belongs to the ensuing year 1829, is taken from a letter addressed to the President of this Society by Dr. Olbers, and is computed for the time of midnight at Paris. The positions are computed for every third or fourth day only, at the beginning and end of the ephemeris, and for every second day towards the middle: this being quite sufficient to enable the observer to find the place of the comet in the heavens. The original computations are extended only to minutes of space; but the right ascensions are here converted into the nearest second of time, for the convenience of the observer.

[It being desirable that as many observations should be made of this comet as possible, particularly in the southern hemisphere, where it will probably be seen after its return from the sun, an extra number of copies of the above ephemeris have been struck off for distribution, not only amongst the Members of the Society, but also for circulation in various parts of the world where the comet is likely to be seen. And Capt. Foster has kindly undertaken to distribute them as much as possible in those parts of the southern hemisphere at which he may touch during the scientific voyage in which he is about to embark.]

There was next read a paper, "On finding the rates of time-keepers;" by E. Riddle, Esq. In this communication the author observes, that there are many persons fond of astronomy who are not possessed of a transit instrument, or who have not a convenient situation in which to place it; and many others, such as nautical men in particular, who have not the means of using one, who are desirous on many accounts, of knowing the *rates* of their chronometers, independent of the absolute time which they indicate. The method of equal altitudes on *each side* of the meridian is the course usually pursued on such occasions: but Mr. Riddle proposes another mode, often much more convenient in practice and equally correct in its results; viz. by taking equal altitudes of a fixed star on the *same side* of the meridian, on successive nights. It is well known that a star will, at the same sidereal hour, arrive at the same altitude, for several succeeding nights: the only difference which occurs, arising from the small change in the aberration, and also from the variation in the refraction. The former is insensible, if the interval between the observations be not too long; and for the latter, appropriate tables are given. The best time for the observation of such stars is when they are due east or due west; and any known star may be chosen for the purpose. All we have to do therefore, is to note the difference of the two consecutive times at which the star attains the same altitude (whatever it be) on the same side of the meridian; and if that difference be *less* than $3^m\ 55^s,91$, the chronometer (presuming that it is regulated to mean solar time) will have *gained*; and if *more*, it will have *lost* so much in a sidereal day. And, if the observations are made at an interval of n days, the nth part of the difference between the times of observation, compared with $3^m\ 55^s,91$, will in like manner give the mean rate for that interval; and if this quantity be multiplied by 1,0027, it will give the rate for a mean solar day. The author concludes his paper with several practical examples of the method; and also with a formula for reducing a series of observations made on any one night, to the same altitude as shown by a series made on any other night: whereby the whole become strictly comparable.

Lastly, there was read the following communication from the Rev. Thomas John Hussey, to Francis Baily, Esq.

"In forming a correct catalogue of the stars situated between 15° N. and 15° S. declination, and extending from 13^{h} 56^{m} to 15^{h} 4^{m} R.A. from the catalogues of Piazzi and Bradley, and the observations of Lalande and Bessel, Mr. Dawson and myself have been much struck with the difference between the places of particular stars, as laid down by Piazzi and Bradley, and the places assigned by reducing the observations of M. Bessel. The results obtained from the *Histoire Céleste* come as nearly to the standard catalogue as can be expected, considering the nature of the instrument employed for determining the zenith distances, and thence the declination in which principally the differences exist: some few of these between observations of the same star are rather unaccountable, others may be attributed to errors of the press; they are as follow.—*Histoire Céleste*,

			h	m	s		°	′	″		
Page	154	Æ.	14	42	$\overline{44}$·5	ZD.	57	2	12	read	34^{s}
—	155	—	14	13	43	—	50	$\overline{2}$	36	—	12′
—	155	—	14	59	38·5	—	50	30	16	—	13^{h}
—	155	—	14	32	52·4	—	$\overline{59}$	5	42	—	49°
—	156	—	14	37	47	—	50	45	34	—	58″
—	167	—	15	2	36	—	38	47	6	—	27′
—	289	—	14	44	33·5	—	61	10	8	—	32″
—	291	—	14	52	7·3	—	46	$\overline{59}$	0	—	58′
—	340	—	14	45	$\overline{56}$·5	—	54	20	49	—	50″
—	471	—	14	46	$\overline{18}$	—	39	5	22	—	8^{s}

Between the places shown by the observations of Lalande and those of M. Bessel, even where there are more than one observation by each of these astronomers, there are differences more or less considerable in *almost* every instance; occasionally, however, there is a surprising coincidence. On this subject we may hereafter trouble you with some remarks, my present object being to point out the differences between the standard catalogue of M. Piazzi and a catalogue reduced to 1800 from the observations of M. Bessel, of the only stars comprised within the above limits, which have been observed more than once by M. Bessel, so as that the place of the star may be considered as pretty nearly ascertained by this indefatigable astronomer alone. Their number amounts to 31, and the results are exhibited in the following table. The differences are expressed in space.

No.	Mag.	Right-Ascension.		Diff.	Declination.		Diff.	No. of Obs.	When observed.	
		Piazzi.	Bessel.		Piazzi.	Bessel.				
		h m s	h m s	″	° ′ ″	° ′ ″	″			
1	8	13 56 25,87	13 56 25,60	− 4·10	+11 47 14·60	+11 47 13·24	−1·37	2	1822—3	
2	8	57 39,71	57 41,29	+23·69	−11 36 26·20	−11 36 29·51	+3·31	2	1824	+0·9872 annual proper motion?
3	8	59 39,27	59 39,68	+ 6·20	+11 12 22·50	+11 12 17·59	−4·91	2	1822—3	
4	8	14 0 25,10	14 0 25,47	+ 5·49	−10 59 58·70	−11 0 1·45	+2·75	3	1824	
5	8	36,90	37,04	+ 2·09	+ 1 45 9·00	+ 1 45 4·15	−4·86	2	1822	
6	8·9	54,84	54,65	− 2·91	+ 5 21 15·40	+ 5 21 12·76	−2·64	2	1822	
7	7	1 54,80	1 55,11	+ 4·64	− 8 57 8·00	− 8 57 12·90	+4·90	3	1824	
8	8	2 53,47	2 53,60	+ 1·97	+ 8 57 20·00	+ 8 57 19·06	−0·94	2	1823	
9	7·8	3 57,10	3 57,11	+ 0·08	− 5 0 37·00	− 5 0 34·58	−2·43	2	1822—4	
10	7	4 6,50	4 6,96	+ 6·83	+12 56 30·20	+12 56 26·13	−4·08	2	1822	
11	6	5 3,40	5 3,47	+ 0·99	+11 2 59·00	+11 2 50·41	−8·59	3	1822—3	
12	6	11 40,87	11 41,10	+ 3·42	− 1 3 58·00	− 1 4 3·47	+5·47	2	1822	
13	8	12 4,70	12 4,85	+ 2·19	− 6 50 29·00	− 6 50 34·76	+5·76	3	1824	
14	6	13 32,70	13 33,05	+ 5·20	+ 9 21 50·20	+ 9 21 51·37	+1·17	2	1823	This star is double, and the observations are of the one which is of Mag. 6.
15	7	14 9,67	14 9,66	− 0·09	+ 9 9 36·40	+ 9 9 32·79	−3·61	3	1822—3	
16	7	14 28,24	14 28,40	+ 2·36	+ 9 0 19·10	+ 9 0 14·07	−5·03	3	1822—3	
17	7·8	17 51,90	17 51,99	+ 1·35	− 9 5 56·00	− 9 6 1·53	+5·53	3	1824	
18	8	22 11,70	22 11,89	+ 2·81	+ 5 2 4·70	+ 5 2 0·91	−3·80	2	1822	
19	8	23 29,87	23 30,17	+ 4·54	+ 7 11 4·50	+ 7 10 56·33	−8·18	2	1822—3	
20	8	25 37,72	25 37,54	− 2·71	− 8 43 56·00	− 8 43 50·56	+4·56	2	1824	
21	8	26 43,96	26 44,15	+ 2·78	− 3 0 43·70	− 3 0 50 92	+7·22	2	1822	
22	7·8	27 24,07	27 24,39	+ 4·88	− 4 40 27·50	− 4 40 19·28	−8·22	2	1822—4	
23	8·9	28 29,94	28 30,42	+ 7·23	− 4 54 46·50	− 4 54 52·48	+5·98	2	1822—4	
24	9	30 27,54	30 27,39	− 2·25	− 5 35 29·20	− 5 35 32·11	+2·91	2	1823—4	
25	7·8	37 18,80	37 19,06	+ 3·83	+ 2 53 2·05	+ 2 52 55·77	−6·74	2	1822	Bessel's obs. quite close.
26	8	41 30,13	41 30,11	− 0·35	−14 33 23·40	−14 33 26·45	+3·05	2	1824	
27	7·8	43 9,30	43 9,44	+ 2·05	− 8 15 31·00	− 8 15 35·93	+4·93	2	1824	
28	4·5	50 18,20	50 18,34	+ 2·13	− 7 42 55·80	− 7 43 2·41	+6·61	2	1824	
29	8	52 58,50	52 59,09	+ 8·84	− 6 46 37·70	− 6 46 43·25	+5·55	2	1823—4	
30	9	53 40,74	53 41,22	+ 7·23	−14 12 20·20	−14 12 25·61	+5·41	2	1824	Bessel's obs. quite close.
31	8	58 2,07	58 2,32	+ 3·75	−13 13 29·50	−13 13 32·33	+2·83	3	1824	

"In many of the above cases it is probable that the difference between Piazzi's catalogue for 1800, and one reduced to that epoch from Bessel's observations, may arise from error of observation: in others the almost identity of the observations seems to prove at once the skill of the observer, and a proper motion in the star; while again there is one (No. 2.) which possibly may be regarded as conclusive on the latter point. I would also suggest two instances of what appear errors of the press, or perhaps in reading off the instrument, in M. Bessel's zones.

Z. 154. Æ. $14^{h}\ 33^{m}\ 33^{s}\cdot7$. Dec. $12^{\circ}\ \overline{41}'\ 22''\cdot4+$ Unless 26′ be read for 41′ this star of the sixth magnitude, 32 Bootis, will differ from Piazzi's catalogue $+14'\ 56''\cdot88$.

Z. 162. Æ. $14^{h}\ 37^{m}\ 10^{s}\cdot3$. Dec. $\overline{9}^{\circ}\ 55'\ 36''\cdot8+$ Unless 8° be read for 9° this star of the 7·8 magnitude will differ from Piazzi $1^{\circ}\ 0'\ 0''\cdot39$."

April 11.—A paper was read "On the construction of large Achromatic Telescopes." By A. Rogers, Esq.—In this paper the author describes a new construction of an Achromatic Telescope, the object of which is to render a small disc of flint glass available to perform the office of compensation to a much larger one of crown, and thus to render possible the construction of telescopes of much larger aperture than are now common, without hindrance from the difficulty at present experienced in procuring large discs of flint glass. It is well known that in the ordinary construction of an achromatic object-glass, in which a single crown lens is compensated by a single one of flint, the two lenses admit of being separated only by an interval too small to afford any material advantage in diminishing the diameter of the flint lens, by placing it in a narrower part of the cone of rays, the actual amount of their difference in point of dispersive power being such as to render the correction of the chromatic aberration impossible, when their mutual distance exceeds a certain limit.

This inconvenience Mr. Rogers proposes to obviate, and obtain the advantage in question, by employing as a correcting lens, not a single lens of flint, but a compound one consisting of a convex crown, and concave flint, whose foci are such as to cause their combination to act as a plane glass on the mean refrangible rays. Then it is evident that by reason of the greater dispersive power of flint than of crown glass, this will act as a concave on the violet, and as a convex on the red rays; and *that*, the more powerfully, according as the lenses separately have greater powers or curvatures. If then, such a compound lens be interposed between the object-glass of a telescope, supposed to be a single lens of plate or crown glass, and its focus, it will cause no alteration in the focus for mean rays, while it will lengthen the focus for violet, and shorten it for red rays. Now this is precisely what is wanted to produce an achromatic union of all the rays in the focus; and, as nothing in this construction limits the powers of the individuals composing the correcting lenses, they may therefore be applied any where, that convenience may dictate; and thus, theoretically speaking, a disc of flint glass, however small, may be made to correct the colour of one of crown, however large.

But this construction possesses other and very remarkable advantages. For, first, when the correcting lens is approximately constructed on a calculation founded on its intended aperture, and on the refractive and dispersive indices of its materials, the final and complete destruction of colour may be effected not by altering the lenses by grinding them anew, but by sliding the combination nearer to, or further from, the object-glass, as occasion may require, along the tube of the telescope by a screw motion, till the condition of achromaticity is satisfied in the best manner possible. And, secondly, the spherical aberration may in like manner be finally corrected by slightly separating the lenses of the correcting glass, whose surfaces should for this purpose be figured to curvatures previously determined by calculation, to admit of this mode of correction—a condition which the author finds to be always possible.

Mr. Rogers explains his construction by reference to a diagram, and states the rule for the determination of the foci of the lenses of the correcting glass in a formula which may be thus interpreted. "The focal length of either lens of the correcting lens is to that of the object-glass, in a ratio compounded of the ratio of the square of the aperture of the correcting lens to that of the object-glass, and of the ratio of the difference of the dispersive indices of crown and flint glass, to the dispersive index of crown:"—for example, to correct the colour of a lens of crown or plate glass of nine inches aperture, and fourteen feet focal length (the dimensions of the celebrated telescope of Fraunhofer at Dorpat) by a disc of flint glass three inches in diameter, the focus of either lens of the correcting lens will require to be about nine inches. To correct it by a four inch disc will require a focus of about sixteen inches for each.

The author then remarks, that it is not indispensable to make the correcting glass act as a plane lens. It is sufficient if it be so adjusted as to have a shorter focus for red rays than for violet. If, preserving this condition, it be made to act as a concave lens, the advantage procured by Mr. Barlow's construction of reducing the length of the telescope with the same focal power, is secured: and he considers, moreover, that by a proper adaptation of the distances, foci, &c. of the lenses, we might hope to combine with all these advantages, that of the destruction of the secondary spectrum, and thus obtain a perfect telescope.

There was also read a portion of a paper "On the Occultation of δ Piscium observed in Blackman-street, in the month of February 1821. References to recorded observations of occultations in which peculiarities have been apparently seen, either at the moon's limb, or upon her disc; together with an enquiry how far certain hypotheses seem adequate to account for the phenomena. By James South, Esq."

PROCEEDINGS AT THE FRIDAY-EVENING MEETINGS OF THE ROYAL INSTITUTION OF GREAT BRITAIN.

April 25.—The lecture-room subject was practical sculpture, and was delivered by Mr. Sievier. It consisted of an account of the various processes adopted by the sculptor from the commencement of modelling in clay, to the last touch required to complete his subject in marble; and the various operations of modelling, casting, chiseling, and the use and theory of the different instruments used, were explained and illustrated by examples. For these purposes clay-models, plaster-casts, and blocks of marble in different progressive states, were placed upon the table, upon which the workmen carried on their several operations.

May 9.—Mr. Faraday gave a lecture on the production of musical sound, with an explication of the principles of action in some new musical instruments. He stated his information to be derived altogether from Mr. Wheatstone, to whom, of course, the new facts brought forward belonged. The nature of the pitch of sounds was first explained, and stated to be the quality by which musical sounds are distinguished from mere noise. The credit of the first philosophical account of this quality was attributed to Gallileo, who proved that it depended altogether upon the number of impulses in a given time. An experiment by that philosopher, apparently forgotten in modern times, but described in his Dialogues, was repeated:—it consisted in drawing the point of a blade over a metal plate so as to produce sound, and counting or comparing the dots which are always produced upon the plates, and which are the records of the number of vibrations necessary to the pitch of sound produced.

Hook's experiment, in which the teeth of a revolving wheel were made to beat against a card, was repeated, and the pitch of sound shown to correspond with the number of impulses in a given time. An extension of this experiment by Mr. Wheatstone was also described, in which the harmonics of a stretched string were produced by holding it against the wheel; the string producing sound whenever the velocity of the wheel was such that the impulses of the teeth corresponded in frequency with the vibrations of the string or of any of its aliquot parts.

Robison's mode of producing musical sounds by air passing through a stop-cock rapidly revolving, was then explained and illustrated; afterwards, Cagniard de la Tour's syren was set in action; and from these were drawn the explanation of the principles of two new musical instruments from Germany, the Munt-harmonica and the Acol-harmonica.

May 2 and 16.—On these evenings Mr. Knowles, of the Navy Board, gave an account of the rise, progress, and present state of naval architecture. To make his historical observations more clear, he first stated the elements concerned in the structure of ships, as stability, floatation, masts and yard, &c.&c., and then traced the state of the navy from the reign of Alfred to the present day; pointing out the times and progression of its increase; the period when any improvement had taken place; and more minutely entering into the important alterations introduced of late years by Sir W. Rich, Sir Robert Seppings, and others. All the observations relating to construction were fully illustrated by numerous models from the magnificent collection belonging to the Navy Board.

LXVIII. In-

LXVIII. *Intelligence and Miscellaneous Articles.*

ORIGIN OF TRAP ROCKS.

[The following is an abstract of a Paper on this subject by Mr. S. Solly, read before the Royal Society on the 6th of March last.]

WHILE Mr. Solly was examining the hills of porphyry near Christiania, he discovered a low, detached, quadrangular rock of trap of the species called *diorite*, which exhibited in a manner that had never before been noticed, except in the experiments of Gregory Watt, the gradual development of crystallization commencing with globules of green pyroxene and white felspar, sometimes enveloping each other, and varying in size from that of a small pea to extreme minuteness; they were thickly strewed on a perpendicular side of the rock near the ground, and at a distance had the appearance of cryptogamia.

On a close examination he found some of the globules stretched out, and in other parts close to the former; the felspar had shot into parallel lines, which here and there had united to form rhombs. He likewise found the pyroxene standing out upon the surface in regular crystals, exhibiting a curious variety of modification; some of them crossing each other like the double crystals of harmotome.

To his communication of this fact, Mr. Solly has prefixed some remarks on the probable history of rocks of trap, derived from a comparison of the different situations in which he has met with them. From observing their partial resemblance to lava, and that they are most frequently met with in coal-fields, Mr. Solly infers that they owe their origin to internal combustion; and as little doubt seems to remain that our beds of coal were formerly bogs, which have been compressed and protected by a covering of clay and sand, he conjectured that other masses of bog and combustible strata lying more exposed have been converted into trap. The position of the Swedish rocks, (from whose quadrangular form this name is derived,) strengthened this conjecture, which was confirmed by his meeting with a stone perfectly similar in all its characters, recently formed by the spontaneous combustion of substances which perfectly correspond with some of the strata* found beneath the Swedish rocks.

Mr. Solly introduced the subject with observations on the assistance geology ought to derive from the discoveries in experimental science. Various observations on currents are interspersed through this paper, to explain the difference of stratification in different situations. From the insulated character of the formations in countries intersected by granite, Mr. S. infers that the latter cannot be a rock of posterior formation as the Scotch Plutonists imagine.

An explanation is given of some instances of high temperature in the lower part of deep mines, from which erroneous conclusions have been drawn. Mr. S. touches on the subject of electrical as well as chemical action beneath the surface of the earth, and is of opinion that the gaseous products of beds of coal have assisted in converting their covering of loose sand and clay into coal sandstone and clay ironstone.

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* They are sufficiently impregnated with bitumen and sulphur to be used as fuel or roasted for alum. See *Thomson's Travels.*

As the subject of internal changes is of practical importance to the miner, Mr. S. recommends its investigation. The garnets and zeolites found in shale by Mr. Henslow, are decided instances of internal changes, and coincide with some of the facts previously noticed by Mr. S.

His distinction between lava and trap, which we more generally call Basalt, and in Scotland is chiefly known by the name of Whin, is confirmed by the authority of Sir James Hall: "Calcareous spar frequently occurs in whinstone, either in veins, or in detached nodules, but is never found in lava, and could not exist in a volcanic stream. It is generally supposed that some lavas of Ætna contain calcareous spar and zeolites; but this I conceive to be a mistake. It is true I have seen that many rocks of Ætna contain these substances in abundance, but in my opinion these rocks are not lavas, but have flowed subterraneously like our whins, and are the same with them in every respect. A particular district of Ætna, comprehending the Cyclopian Isles, and the country round La Trizza, and the Castle of Jaci, is decidedly of this description; and vestiges of this kind occur in other parts of the mountain. In one place fossil coal has been found, and in another we saw marine shells. In the neighbourhood of Bronte, we observed a high ridge formed of strata of sandstone and limestone, partly overflowed and concealed by recent lava, but so placed as to render it evident that its construction formed no inconsiderable part of the mountain."

Mr. S. has heard disciples of Werner mention, that from its contiguity to basalt, he supposed Ætna was erected on the site of a coal mine: the additional coincidences in Sir James Hall's description seem to prove that coal is a part of the fuel of Ætna. Breislack, who has spent many years in examining Vesuvius, is of opinion that it is fed by the bituminous strata of the Appenines.

The principal objection to this opinion has been removed by Sir Humphry Davy, whose genius has been scarcely more conspicuous in anticipating the discoveries made by the galvanic battery, than in drawing those inferences from them which have assisted his contemporaries in bringing other discoveries to light. Thus our knowledge of the properties of iodine is a natural consequence of his observations on the nature of chlorine: these and every other agent of combustion ought to be enumerated in discoursing on the infinite variety of chemical and electro-chemical operations which may take place in the vast laboratory of Vesuvius. Since with the assistance of the electrical energies of iron we can make potash yield up its oxygen, is it not possible that the earths may be reduced to a metallic state by a similar process in the bowels of the mountain? These considerations show there is no necessity for any contrivance or reservoir to keep up the supply of elastic oxygen. By placing at the command of the philosopher powers which are always close at hand, and are capable of producing tremendous results,—by teaching him to conjure up inflammatory spirits from the surface of the "vasty deep," and to turn water into fire,—Sir H. Davy has shown it is not necessary that Pluto should ascend from the lowest depths of his empire to produce the phænomena of volcanos, and much less so to bring about a chemical union between silex, alumina, lime, and iron, the chief constituents of basalt or trap. *Nec Deus intersit dignus nisi vindice nodus.*

ANALYSES OF TOURMALINES.

M. Gmelin has analysed a great many varieties of this mineral; the method adopted is the following:—The mineral reduced to a fine powder is mixed with carbonate of barytes, and strongly heated. The mass is afterwards treated with a sufficient quantity of muriatic acid to dissolve it entirely, and the solution is evaporated upon a sand bath to dryness. M. Gmelin ascertained by direct experiment that at this temperature the quantity of boracic acid volatilized is so minute that it may be neglected without any sensible error. The silica is obtained in the usual manner, by treating the residuum of evaporation with water. Carbonate of ammonia is added to the solution, and after filtration, and evaporation to dryness, the residuum is gradually heated to low redness. In this manner, no boracic acid can be lost, because it is combined with ammonia; and thus during calcination at a red heat, no acid or aqueous vapour is evolved, as in the decomposition of sulphate of ammonia. The residuum after being weighed, is washed with alcohol mixed with a little muriatic acid; the alcohol being separated is burnt; the operation is repeated until the alcohol does not give a green flame. All the boracic which is combined with the ammonia is thus obtained. The residuum again heated and dried, and the loss of weight determines the quantity of boracic acid.

Tourmalines are divided by M. Gmelin into three classes: the first of which contains lithia:—I. Red Tourmaline from Rôsna in Moravia; sp. gr. 2·96 to 3·02. II. Red Tourmaline from Perm in Siberia; sp. gr. 3·059. III. Celadon Green Tourmaline from Brazil; sp. gr. 3·079.

	I.	II.	III.
Boracic acid	5·74	4·18	4·59
Silica	42·13	39·37	39·16
Alumina	36·43	44·00	40·00
Oxidulous oxide of iron	..	..	5·96
———— manganese	6·32	5·02	2·14
Lime	1·20		
Potash	2·41	1·29	
Lithia	2·04	2·52	3·59 with potash.
Volatile matter	1·31	1·58	1·58
	97·58	97·96	97·02

Tourmalines which contain potash or soda, or both together, without lithia, and without a notable quantity of magnesia. The following are the varieties :—I. Black Tourmaline from Bovey in Devonshire, found with quartz and phosphate of lime; sp. gr. 3·246. II. Black Tourmaline from Eibenstock in Saxony; sp. gr. 3·123. III. Green Tourmaline from Chesterfield, North America, sp. gr. 3·102.

	I.	II.	III.
Boracic acid	4·11	1·89	3·88
Silica	35·20	33·05	38·80
Alumina	35·50	38·23	39·61
Oxidulous oxide of iron	17·86	..	7·43
Protoxide of iron	..	23·86	
———— manganese	0·43 with magnesia	..	2·88 †
Magnesia	0·70 with manganese	0·86	
Lime	0·55	3·17 *	4·95
Soda	2·09	0·45	0·78
Loss in the fire	..		
	96·44	101·51	98·33

Tourmalines which contain a considerable quantity of magnesia.—Four specimens were analysed :—I. Black Tourmaline from Käringbricka, a province of Westmanland in Sweden; sp. gr. 3·044. II. Black Tourmaline from Rabenstein in Bavaria; sp. gr. 3·113. III. Black Tourmaline from Greenland; sp. gr. 3·062. IV. Deep Brown Tourmaline, from the mica slate of St.-Gothard.

	I.	II.	III.	IV.
Boracic acid	3·83	4·02	3·63	4·18
Silica	37·65	35·48	38·79	37·81
Alumina............	33·46	34·75	37·19	31·61
Magnesia......... ..	10·98	4·68	5·86	5·99
Oxidulous oxide of iron	9·38	17·44	5·81	7:77
Oxide of manganese ..	....	1·89	trace.	1·11
Potash .... }	2·53	{ 0·48	0·22	1·20
Soda .... }		{ 1·75	3·13	
Lime	0·25	trace.	....	0·98
Loss in the fire	0·03	....	1·86	0·24
	98·11	100·49	96·48	90·89

M. Gmelin is at a loss to what cause to attribute the deficiency in the last analysis. He thinks the tourmaline from St.-Gothard should be again examined; especially as the loss in Bucholz's analysis is still greater.—*Ann. de Chimie et de Phys. Nov.* 1827.

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* With potash and a trace of magnesia. † With a trace of magnesia.

A NEW METHOD OF SEPARATING MANGANESE FROM LIME AND MAGNESIA.—BY PROFESSOR STROMEYER.

We are informed in a letter from Professor Stromeyer, that he has found the following method successful in procuring the complete separation of manganese from magnesia and lime. To an acid liquid containing the peroxide of iron together with manganese, lime, and magnesia, the carbonate of soda is added in the usual manner, so as to precipitate the first, while the three latter oxides are held in solution by an excess of carbonic acid; and in order to prevent any manganese from falling, the actual precipitation of the iron is effected by the bicarbonate instead of the carbonate of soda. After acidulating the filtered solution and concentrating it by evaporation, a current of chlorine gas is transmitted through it. On neutralizing the free acid by the gradual addition of bicarbonate of soda, the manganese subsides in the form of the red oxide, being thus completely separated from the magnesia and lime.—*Brewster's Journal, April* 1828.

SINGULAR ACTION OF PHOSPHORIC ACID ON ALBUMEN.

In his essays on the animal fluids Berzelius stated, that a solution of albumen is not precipitated by phosphoric acid; whereas Engelhart, in his interesting researches on the colouring matter of the blood, found that albumen is coagulated even in a dilute solution by phosphoric acid. As Engelhart was at Stockholm last winter, he and Berzelius inquired into the cause of difference in their statements, and discovered that they were both right. A solution of phosphoric acid, which had been kept some time in the laboratory, did not precipitate a solution of albumen; but phosphoric acid, recently prepared either by the action of nitric acid on phosphorus or by direct combustion, caused an abundant precipitate. On further examination, it was found that phosphoric acid, recently ignited, always throws down albumen; but that after being kept in solution for a few days, it loses that property. The coagulating power is restored by heating the acid to redness, but disappears again after the interval of a day.

The cause of these phænomena is by no means apparent. It does not depend on a higher degree of oxidation, for the change ensues in close vessels equally as by exposure to the air. Perhaps, says Berzelius, there may be some peculiar compound of water and the acid, which is not formed at the moment of solution, and which has not the property of precipitating albumen.—*Annales de Chimie et de Physique*, xxxv. 110.

Remark on the preceding notice, by Dr. Turner.—On comparing the facts observed by Berzelius and Dr. Engelhart, with the formation of the pyrophosphate of soda, described by Mr. Clark in the fourteenth Number of this Journal, it appeared probable that phosphoric acid heated to redness may be converted into pyrophosphoric acid, or undergo that change which enables it to form a white salt with silver. To put this supposition to the test of experiment, some fragments of phosphorus were treated in a platinum crucible by nitric acid, and the product heated to redness. The solution of the resulting pure phosphoric acid precipitated a dilute solution of albumen; but when carefully neutralized by carbonate of soda, and then mixed with a solution of the nitrate of silver, the common yellow phosphate subsided. Consequently, it was not in the state of pyrophosphoric acid.—*Brewster's Journal, April* 1828.

LIST OF EARTHQUAKES WHICH OCCURRED IN 1827.

Jan. 2.—At Mortagne (Orne) and the environs. A violent shock of short duration, accompanied with an intense noise. Chimneys and household furniture were thrown down. The commotion reached as far as Alençon. The day was cloudy, the weather thick and stormy, which is not usual at that time of the year.

Feb. 9.—At seven o'clock in the evening; in the north-west part of Wales and the Isle of Anglesea. The shocks continued from forty seconds to a minute; they were sufficiently violent to overturn several pieces of furniture. A noise was heard like that of a heavy laden cart going on the stones.

April 2.—At Bevers, at twenty minutes past one in the morning; two strong consecutive shocks. The inhabitants of Basse-Engadine assert that they counted twenty similar shocks during the winter.

May 29.—At Vajaca, in Mexico; two slight shocks.

June 3.—At Martinique; a slight shock.

June 12.—At Tehenacan, in Mexico, at half-past one o'clock; a violent shock, with a frightful noise. Many buildings damaged.

June 16.—At Aquila, in the kingdom of Naples; a shock at five o'clock in the morning.

June 21.—At Palermo, at eleven o'clock in the morning. Four strong shocks in the space of seven seconds; it was an oscillatory motion from the west to the east.

Aug. 14.—At Palermo, at 2 p.m. Several shocks; they continued about eighteen minutes, with very short intervals; the motion was always oscillatory.

Sept. 18.—At Lisbon. A slight shock.

Oct. 10.—At Zurich, and all the shores of the lake. At twelve minutes before 3 p.m., a strong shock.

Oct. 15.—At Jassy, at eight in the evening. Two violent shocks, directed from north to south, and accompanied by a subterraneous noise; two or three days after, the heat was very great.

Oct 30.—At Corsica, in the cantons of Taravo, Taliano, and Sartène. Two shocks at twenty minutes past 5, a.m.

Nov. 30.—At Pointe-à-Petre, Guadaloupe, at three in the morning. Violent earthquake. At Mariegalante it was preceded by a strong and sudden storm.—*Ann. de Chimie et de Phys. Nov.* 1827.

RED RAIN SUPPOSED TO ARISE FROM BUTTERFLIES.

The following narrative seems curious and important in connection with the various accounts of red rain. It is extracted from *Gassendi's Life of Peiresc*, p. 110-113. "Through the whole of this year (1608) nothing

thing gave M. Peiresc greater pleasure than his observations upon the *bloody rain*, said to have fallen about the beginning of July. Large drops were seen both in Paris itself upon the walls of the cemetery of the greater church, which is near the walls of the city, upon the walls of the city, and likewise upon the walls of villas, hamlets, and towns for some miles round the city. In the first place, M. Peiresc went to examine the drops themselves, with which the stones were reddened, and spared no pains to obtain the means of conversing with some husbandmen beyond Lambesc, who were reported to have been so astonished at the shower, as to leave their labour and fly for safety into the neighbouring houses. This story he ascertained to be without foundation. To the explanation offered by the philosophers, who said that the rain might have come from vapours, which had been raised out of *red* earth, he objected that evaporated fluids do not retain their former hues, as is plainly exemplified in the colourless water distilled from red roses. Nor was he better satisfied with the opinion of the vulgar, countenanced by some of the theologians, who maintained that the appearance was produced by demons, or witches, shedding the blood of innocent babes. This he thought was a mere conjecture, scarcely reconcileable with the goodness and providence of God. In the meantime an accident happened, which discovered to him, as he thought, the true cause of the phænomenon. He had found some months before a chrysalis of a remarkable size and form, which he enclosed in a box. He thought no more of it, until, hearing a buzz within the box, he opened it, and perceived that the chrysalis had been changed into a most beautiful butterfly, which immediately flew away, leaving at the bottom of the box a red drop of the size of a shilling. As this happened about the time when the shower was supposed to have fallen, and when a vast multitude of those insects was observed fluttering through the air in every direction, he concluded that the drops in question were some kind of excrementitious matter emitted by them, when they alighted upon the walls. He therefore examined the drops again, and remarked, that they were not upon the upper surfaces of stones and buildings, as they would have been, if a shower of blood had fallen from the sky, but rather in cavities and holes, where insects might nestle. Besides this, he took notice that they were to be seen upon the walls of those houses only, which were near the fields, and not upon the more elevated parts of them, but only up to the same moderate height at which the butterflies were accustomed to flutter. In this way he explained the story, told by Gregory of Tours, of a bloody shower seen at Paris in the time of Childebert, at different places, and upon a house in the vicinity of Senlis; and another said to have fallen in the time of King Robert, about the end of June, the drops of which could not be washed out by means of water, when they had fallen upon flesh, garments, or stones, but might be washed out from wood; for the time here stated was the season for the butterflies; and he showed that no water could wash out these red marks from stones. After discussing these and similar arguments in the presence of much company at the house of his friend Varius, they determined to inspect the appearance together, and, as they wandered through the fields, they saw many drops upon stones and rocks, but only in hollows or upon sloping surfaces, and not upon those which were presented to the sky." The butterfly observed by Peiresc was probably the *Papilio C. album*, or common butterfly. It has been observed to deposit the same red fluid in England.

WEATHER IN PARIS.

The following was the state of the weather during the last year in Paris.

Rain	146 days.
Snow	21
Hail or hoar frost	6
Frost	59
Thunder	21
Very cloudy	178

Ann. de Chimie et de Phys. Dec. 1827.

ANALYSIS OF ALCOHOL, ÆTHER, &C. BY MM. DUMAS AND BOULLAY.

In the Philosophical Magazine and Journal for April, I gave some account of the paper by the above-named chemists, and proposed to offer some remarks upon it in the present Number: the Royal Institution Journal for April contains some observations so much to the point which I had intended to notice, that I shall content myself with copying them.—R. P.

"MM. Dumas and Boullay then consider the formation and nature of the sulphuric acid, stating the opinions of MM. Vogel and Gay Lussac, that it is a compound of hyposulphuric acid with a vegetable matter; and also that of Mr. Hennel, that it is an acid in which half the saturating power of the sulphuric acid present is neutralized by the hydrocarbon in combination. With the latter opinion they also class that entertained by Mr. Faraday relative to the nature of sulphonaphthalic acid. MM. Dumas and Boullay consider the question, and decide in favour of the former opinion; after which they say they have observed facts which are better explained by the latter. We think it a pity that these philosophers did not refer to Mr. Hennel's paper in the Philosophical Transactions for 1826, p. 240, where they would have gained the knowledge of a compound produced by the action of sulphuric acid and alcohol, of which they seem at present altogether ignorant, and which would have probably caused serious alterations in their views, at least with regard to the sulphovinic acid. We refer to what is known in London, and sold at Apothecaries' Hall, by the name of *oil of wine*; not the hydrocarbon referred to by MM. Dumas and Boullay, but a *neutral* compound of sulphuric acid with hydrocarbon, containing, with the same proportion, of sulphuric acid, twice as much hydrocarbon as the sulphovinic acid. It is of this compound that Mr. Hennel speaks in the following passage, which we cannot refrain from quoting: 'Mr. Vogel, who has particularly described some of these salts (sulphovinates), and I believe also M. Gay Lussac, have supposed that this loss of saturating power arises from the formation of hyposulphuric acid, and that the hyposulphates and sulphovinates only differ in the latter containing some æthereal oil, which in some way acts the part of water of crystallization. It is evident that the properties of oil of wine *cannot* be thus explained; and it appears to me more probable that the power of combination *which hydrocarbon is shown to be possessed of* in oil of wine is effective in neutralizing half the acid of the salts formed from it (sulphovinates) as before described.'"

TEST OF THE PRESENCE OF AMMONIA.

M. Plessin having occasion to ascertain whether the action of a salifiable base upon a body containing azote was simply that of evolving ammonia previously existing, or that of forming ammonia by combination, endeavoured to find a base, which would effect the former object but not the latter. Potash, lime, magnesia, and many other bodies, do both; but the hydrated oxide of lead answered the purpose very well. It gives no indication of ammonia when put into contact with azotated substances, not containing that alkali; even urea is not effected by it; but being put in contact with an ammoniacal salt, ammonia was instantly evolved, and rendered evident by the visible fumes which arose upon the approximation of a little acetic acid.—*Annales de Chimie,* xxxvi. p. 377.

LIST OF NEW PATENTS.

To Jane B. Lowrey, wife of T. S. Lowrey, of Exeter, for her improvements in the manufacture of hats and bonnets.—Dated the 25th of March 1828.—6 months allowed to enrol specification.

To E. Cowper, of Clapham-road Place, Lambeth, for improvements in cutting paper.—26th of March.—6 months.

To F. de Fourville, of Piccadilly, for improvements on filtering apparatus.—26th of March.—6 months.

To T. Lawes, of the Strand, for an improved thread to be used in the manufacture of bobbin net lace.—29th of March.—6 months.

To H. Marriott, of Fleet-street, and Augustus Siebe, of Princes-street, Leicester-square, for certain improvements in hydraulic machines.—29th of March.—6 months.

To Peter Taylor, of Hollinwood, Lancaster, for certain improvements in machinery for hackling, dressing or combing flax, hemp, &c.—29th of March.—6 months.

To John Davis, of Leman-street, Goodman's-fields, for an improvement (communicated from abroad) in boiling or evaporating solutions of sugar and other liquids.—29th of March.—6 months.

To C. Harsleben, of New Ormond-street, Esq., for improvements in machinery to be used in navigation and the propelling of ships.—3rd of April.—6 months.

To S. W. Wright, of Webber-street, Lambeth, engineer, for improvements in the construction of wheel carriages.—15th of April.—6 months.

To J. G. Ulrich, of Cornhill, for his improvements on chronometers. 19th of April.—6 months.

SCIENTIFIC BOOKS.

Just Published.

On the Methods of determining Terrestrial Longitudes by the Moon's Right Ascension, as deduced from her Altitudes and Culminations. By John Crisp, Captain in the Madras Army. In quarto, Price 6s.

It is the intention of the Medico-Botanical Society of London, to print their Transactions in the shape of an octavo Quarterly Journal, in conjunction with those of the Academy of Minute Anatomy at the London Ophthalmic Infirmary. The first Number of this Journal will appear shortly illustrated with coloured Engravings in quarto.

The Astronomical Doctrine of a Plurality of Worlds irreconcilable with the Popular Systems of Theology, but in perfect harmony with the True Christian Religion. By the Rev. S. Noble.

A Series of Treatises on the principal Branches of Manufacturing Chemistry, by Mr. Astley of Edinburgh, is about to be published. The manufacture of Common Salt will form the subject of the first, which will shortly be published separately, comprising full details of its history, physical, chemical, commercial, and œconomical; with suggestions for the material improvement of the manufacture, and a full digest of the results of the experiments in the use of salt by agriculturists, since the repeal of the duty.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1828.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·28 April 30. Wind NE.—Min. 29·10 April 8. Wind S.E.
Range of the index 1·18.
Mean barometrical pressure for the month .... 29·736
Spaces described by the rising and falling of the mercury........,... 5·550
Greatest variation in 24 hours 0·380.—Number of changes 18.
Therm. Max. 69° April 29. Wind S.E.—Min. 35° April 3. Wind N.
Range 34°.—Mean temp. of exter. air 51°·33. For 30 days with ⊙ in ♈ 48·22
Max. var. in 24 hours 20°·00—Mean temp. of spring water at 8 A.M. 50°·89

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 24th.............. 88°
Greatest dryness of the air in the afternoon of the 28th 42
Range of the index... 46
Mean at 2 P.M. 60°·0 —Mean at 8 A.M. 65°·0—Mean at 8 P.M. 74·4
—— of three observations each day at 8, 2, and 8 o'clock......... 66·5
Evaporation for the month 2·20 inches.
Rain near ground 2·725 inch.

Summary of the Weather.

A clear sky, 2; fine, with various modifications of clouds, 11½; an overcast sky without rain, 9½; rain, 7.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
20	12	26	0	15	25	22

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
1	5	0	5½	1½	10	3½	3½	30

General Observations.—The first five days of this month were dry and cold; afterwards it rained more or less every day till the 26th : but the last four days were dry and warm, in which more progress appeared to have been made in the blooming and growth of fruit and vegetation, than during the preceding fortnight.

The North-east and North-west winds on several days were blighty, the effects of which may be traced in the formation of the young wall-fruit, &c. The 4th and 22nd were very cold days, whose maximum temperature in the shade was only 46 and 47 degrees, which is three or four degrees colder than some of the nights. The first swallow appeared here in the morning of the 21st, and the nightingale was first heard on the 11th instant.

At noon of the 27th three winds prevailed in different directions: the first next the earth, as pointed out by a vane, was from South-east; the next, which was ascertained by keeping the eye in a line with a fixed object whose bearing on the horizon was known, and observing the motion of the passing *cumulus* clouds directly from the same point, was from South-west by South; and the upper one, ascertained in the same way by observing the direction of plumose *cirri*, was from North North-west.

The planet Venus, which has now the appearance of a half-moon, was conspicuous with the naked eye at the same time; and its apparent distance from the Sun's centre, as measured by a sextant, was 44° 8′, which is only a few minutes of a degree less than it will be at its greatest elongation on the 19th of next May.

The mean temperature of the external air this month is 1½ degree higher than the mean of April for the last twelve years.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are four solar and two lunar halos, one rainbow, one meteor, and eight gales of wind, or days on which they have prevailed; namely, one from the South-east, two from the South, four from the South-west, and one from the North-west.

REMARKS.

London.—April 1, 2. Fine. 3, 4. Clear and cold. 5. Cloudy, with rain at night. 6. Cloudy. 7. Rain in morning: fine. 8. Fine. 9. Drizzly: fine. 10. Drizzly: very fine. 11. Fine: rain at night. 12. Showery. 13. Stormy, but fair. 14. Very fine: rain at night. 15. Wet morning: showery. 16. Cloudy: with showers. 17, 18. Wet. 19, 20. Cloudy. 21. Cold and showery. 22. Showery. 23. Fine: stormy and wet at night. 24. Very fine. 25. Fine: stormy and wet at night. 26. Clear and fine. 27—30. Very fine.

Boston.—April 1, 2. Fine. 3—5. Cloudy. 6. Rain. 7, 8. Cloudy. 9. Fine. 10. Cloudy. 11. Fine. 12. Cloudy. 13, 14. Fine. 15. Rain. 16. Fine. 17—20. Cloudy. 21. Rain. 22, 23. Cloudy. 24. Fine. 25. Cloudy. 26—30. Fine.

Penzance.—April 1. Clear: fair: heavy rain at night. 2, 3. Fair. 4. Clear: fair. 5. Clear: a shower. 6, 7. Clear: showers. 8. Rain. 9. Fair: showers. 10. Hail-showers: fair. 11. Rain. 12. Rain: showers. 13. Showers. 14. Fair: clear. 15. Rain: clear. 16. Clear: showers. 17. Showers. 18. Rain: clear. 19. Clear: showers. 20, 21. Fair: showers. 22. Fair. 23. Clear: rain. 24. Misty: rain. 25. Rain. 26. Clear: fair. 27—30. Fair: clear.—Rain-gauge ground level.

Meteorological Observations made at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1828.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost. 8½ A.M.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.										
April 1	30·305	30·146	30·20	30·00	30·24	29·94	29·87	54	40	52	45	52	38	44·5	SE.	SE.	SE.	calm	...	...	...	...	...
2	30·168	30·080	30·06	29·90	29·98	29·96	29·70	50	33	50	43	56	42	45·5	N.	SE.	NE.	E.	...	...	1·760	...	...
3	30·144	30·072	30·06	30·02	30·06	29·99	29·67	47	30	50	40	55	35	39	N.	NE.	NE.	N.	0·25	...	...	...	...
4	30·005	29·991	30·00	29·90	29·92	29·92	29·60	46	29	52	38	46	38	39	NW.	NW.	NE.	calm	...	...	...	...	...
5	29·864	29·711	29·90	29·86	29·85	29·71	29·36	47	36	51	44	51	43	44	W.	NW.	W.	NW.	...	·24	...	...	...
6	29·647	29·603	29·80	29·70	29·59	29·58	29·20	53	38	52	44	55	44	39·5	N.	NW.	NW.	E.	·25	·04	0·100	0·250	0·15
☾ 7	29·482	29·381	29·50	29·40	29·41	29·35	29·05	55	34	48	43	56	42	45	E.	NW.	W.	calm	...	·15	0·300	·060	·05
8	29·386	29·221	29·15	29·00	29·29	29·10	29·03	55	41	48	38	56	44	43·5	NE.	SW.	SE.	E.	...	...	0·455	·020	...
9	29·501	29·268	29·50	29·30	29·48	29·29	28·94	57	40	53	40	58	48	49	SW.	NW.	SW.	E.	·22	·04	0·205	·055	...
10	29·620	29·450	29·66	29·56	29·52	29·40	29·00	61	38	52	47	56	46	47	NW.	NW.	NW.	W.	...	·02	0·150	·055	...
11	29·793	29·731	29·76	29·74	29·76	29·73	29·26	59	47	53	46	57	48	49	SW.	SW.	W.	W.	...	·05	0·200	·140	...
12	29·777	29·578	29·56	29·50	29·60	29·53	29·15	60	49	56	48	56	48	55	SW.	SW.	SW.	calm	·18	·09	0·225	·280	·22
13	29·651	29·528	29·60	29·40	29·62	29·49	28·95	58	41	56	45	58	48	53	SW.	W.	SW.	W.	...	...	0·250	·005	·10
● 14	29·869	29·760	29·80	29·64	29·82	29·74	29·22	62	42	57	46	59	49	51	S.	SW.	SW.	W.	...	·14	...	·060	·13
15	29·784	29·679	29·60	29·50	29·73	29·60	29·30	57	45	56	48	55	49	48·5	S.	SW.	SW.	calm	·23	·10	0·290	·200	·20
16	29·649	29·275	29·48	29·30	29·58	29·34	29·08	58	45	54	48	53	48	52·5	SW.	SW.	S.	S.	...	·13	0·160	·100	·20
17	29·441	29·356	29·30	29·20	29·34	29·23	28·92	56	45	54	47	57	48	52	S.	SW.	SE.	W.	...	·28	0·270	·430	·16
18	29·498	29·360	29·60	29·50	29·40	29·27	28·94	57	42	51	46	57	48	54	S.	NE.	S.	calm	·17	·49	0·560	·430	·21
19	29·738	29·454	29·72	29·70	29·62	29·44	29·15	58	45	52	45	57	47	51	SE.	NE.	NW.	calm	...	...	0·340	·050	·19
20	29·877	29·778	29·76	29·72	29·76	29·64	29·46	53	44	54	45	55	46	48·5	NE.	NE.	NE.	E.	...	...	0·700	·030	...
21	29·752	29·677	29·80	29·80	29·68	29·62	29·30	49	40	48	45	56	43	46	N.	NE.	N.	NE.	·15	·07	0·230	·070	·14
☽ 22	29·845	29·700	29·86	29·82	29·67	29·60	29·20	51	42	50	43	47	45	43·5	N.	NW.	W.	W.	...	·18	...	·020	·14
23	29·883	29·780	29·90	29·86	29·82	29·74	29·26	52	45	54	45	57	49	46·5	NW.	W.	SW.	W.	...	·20	0·600	·240	·07
24	29·898	29·777	29·80	29·80	29·89	29·78	29·15	60	46	55	46	59	50	53	W.	W.	SW.	W.	·10	...	0·115	·080	·04
25	29·949	29·764	29·84	29·80	29·86	29·74	29·32	59	47	56	48	58	47	53	W.	SW.	SW.	SW.	...	·22	0·300	·140	·01
26	30·162	29·957	30·00	29·90	30·10	29·97	29·43	63	36	56	46	59	45	49·5	SW.	SW.	SW.	W.	...	...	0·290	·010	·28
27	30·295	30·257	30·12	30·10	30·24	30·22	29·77	67	41	59	47	63	49	56	S.	SW.	SE.	calm	·35	...	...	...	...
28	30·281	30·238	30·15	30·15	30·27	30·24	29·72	72	45	59	49	64	51	57	S.	SW.	SE.	W.	...	...	...	...	...
○ 29	30·233	30·220	30·20	30·17	30·20	30·19	29·60	75	48	61	49	69	52	63	S.	SW.	SW.	W.	...	...	...	...	...
30	30·383	30·290	30·25	30·25	30·28	30·25	29·73	68	38	62	52	66	47	57·5	N.	N.	NE.	E.	·30	...	...	...	...
Aver.:	29·862	29·736	30·25	29·00	30·28	29·10	29·31	57	41	62	38	69	35	49·1					2·20	2·44	7·500	2·725	2·29

INDEX TO VOL. III.

Acid, preparation of iodous, 146; acetic or vinegar, analysis of, 107; oxalic and citric, analysis of, 109; malic, analysis of, 110; phosphoric, its action on albumen, 462; saclactic, analysis of, 110.
Æther, analysis of, 309.
Air, currents of, Mr. Younge on Désorme's experiments on, 282.
Albumen, action of phosphoric acid on, 462.
Alcohol, analysis of, 309.
Alps, Mr. Bakewell on the thermal waters of, 14.
Ammonia, test of its presence, 466.
Analysis of organized substances, 33; of sugars, 99; of acetic acid, or vinegar, 107; of oxalic and citric acids, 109; of malic and saclactic acids, 110; of a salivary concretion, 146; of some vegetable products, 150; of alloys of bismuth, 230; of alcohol, æther, &c., 309, 465; of tourmaline, 460.
Astronomy, 26, 64, 136, 227, 377, 451.
Atmosphere and the gases, electricity of, 148.
Aurora borealis of 26th Sept., Dr. Forster on, 75.
Bakewell (R.) on the thermal waters of the Alps, 14; review of his introduction to geology, 289.
Barium, peroxide of, 152.
Barker's Mill, Mr. Ivory's notes relating to the theory of, 425.
Bevan's (B.) remarks on Mr. J. Taylor's rain-gauge, 29; notice of an error in Mr. Galbraith's mathematical tables, 153.
Bicheno (J. E.) on systems and methods in Natural History, 213, 265.
Bismuth, analysis of some alloys of, 230.
Books, new, 40, 77, 126, 234, 289, 368, 467.
Botany, 183, 223.
Brandes (Dr. R.) on a gelatinous substance found in a damp meadow, 271.
Bromide of selenium, 147.
Bromine, elementary nature of, 145; quantity of in sea water, 145; sale of, 146; bromine and water, power of in conducting electricity, 151.
Burney's (Dr.) meteorological observations, 78, 157, 337, 317, 398; Meteorological Journal for 1827, 312.
Cadmium, iodine in, 234.
Capillary action, Mr. Ivory on, 1.
Chama concamerata, Mr. Gray on the nursing pouch of, 117.
Clark (B.) on the insect called Oistros by the ancients, 283.
Combustion, heat given out during, 233.
Continuity, Dr. Roget on a violation of the law of, 118, 203.
Copper, examination of, 231.
Crystalline form of some salts, 27.
Currents of aëriform fluids, 74, 282.
Curve-surfaces, on the equation of, 436.
Davy (Sir H.) on the phænomena of volcanoes, 373.
Désorme, (C.) new phænomena of vapour observed by, 74; experiments on currents of air, Mr. Younge on, 282.
Diopside, Dr. Wackenroder's examination of, 349.
Earth, figure of the, 165, 206, 241; Mr. Galbraith on, 321; Mr. Ivory on, 343.
Earthquakes, 463.
Efflorescence, on, 231.
Electricity, 148, 151.
Ellipticity of the earth, Mr. Ivory on, 165, 206, 241; Mr. Galbraith on, 321.
Encke's comet, 451.
Entomology, 283, 368, 370.
Equations, linear, differential, Mr. Herapath on, 19, 210; reply to by and B., 96, 262; reply to by F.R.S. 97.
Ewart (P.) on the reaction of effluent water, and on the maximum effect of machines, 416.
Faraday (M.) on the fluidity of sulphur and phosphorus, 144.
Farey's (J.) treatise on the steam-engine, review of, 40.
Fayolle, M., on the equation of curve-surfaces, 436.
Figure of the earth, Mr. Ivory on, 165,

206, 241, 343, 431; Mr. Galbraith on, 321.
Fire-ball, 74.
Fluids, Mr. Tredgold's new theory of the resistance of, 249.
F.R.S.'s reply to F.R.S.L.'s remarks on compound interest, 30.
Galbraith's mathematical tables, notice of an error in, 153.
Gas, inflammable, from salt mines, employed for producing light, 233; natural gas lights at Fredonea, 233.
Gases and the atmosphere, electricity of, 148.
Gauss, (Prof.) account of his "*Disquisitiones generales circa Superficies Curvas*," 331.
Gay Lussac on efflorescence, 231.
Geology, 132, 188, 225, 243, 289, 291, 436, 441, 458, 463.
Geometrical problem, 234.
German Ocean, on the subsidence of, 188.
Giddy, (E. C.) meteorological results of observations made at Penzance for 21 years, 173.
Gilbert (D.) on the regular or platonic solids, 161.
Giraffa, Sir E. Home on, 46.
Gmelin, M., his analyses of tourmalines, 460.
Gray (J. E.) on the nursing pouch of the Chama concamerata, 117.
Haidingerite, a new mineral, 146.
Haworth (A. H.) on new succulent plants, 183.
Haytor, iron mine at, Mr. Kingston's account of, 359.
Heat, effects of, upon sulphur, 152; heat given out during combustion, 233.
Herapath (J.) on linear differential equations, 19, 210; replies to, 96, 97, 262.
Hordein, analysis of, 150.
Hünefeld (Prof.) on titaniferous iron-slag, 121.
Hutton's log tables, errors in, 154.
Hydrosilicite, a new mineral, 71.
Instruments, ancient, chemical examination of, 154.
Iodine in cadmium, 234.
Iodous acid, preparation of, 146.
Iron ore, green, analysis of, 234.
Iron-slag, titaniferous, Prof. Hünefeld on, 121.
Isopyre, a new mineral, 70.
Ivory (J.) on the theory of capillary action, 1; on the figure of the earth, 165, 206, 241, 343, 431; notes relating to the theory of Barker's Mill, 416.
Karphosiderite, a new mineral, 397.
Kendal, state of the barometer, &c. in for 1827, 236.
Kingston's (J. T.) account of the iron mine at Haytor, 359.
Lead, arseniate of, 234.
Mageough (W.) on a new method of mounting thermometers, 365.
Main (J.) on the phænomena of water-spouts, 114.
Manganese, new chloride of, 151.
—— separation of from lime and magnesia, 462.
Marshall, (S.) meteorological summary for 1827, 236.
Mastodontes, 444.
Membranes, motions of, Dr. Weber on Lavart's experiments on, 336, 342.
Mesitine-spar, a new mineral, 397.
Meteorology, 78, 157, 173, 236, 312, 398, 467.
Miller (Lieut.-Col.) on a percussion rifle, 277; on a percussion shell, 358.
Mineralogy, 70, 71, 146, 349, 397.
Natural History, Mr. Bicheno on systems and methods in, 213, 265.
Nixon (J.) on the heights of the principal beds of Ingleborough Hill and Moughton Fell, Yorkshire, 13; heights of the principal hills in the vicinity of Dent, Hawes, and Sedbergh, Yorkshire, 82, 189.
—— on errors in Hutton's log tables, 154.
Noutronite, a new mineral, 149.
Obituary of Sir J. E. Smith, 307, 391.
Oistros, of the ancients, Mr. Bracy Clark on, 283.
Osmelite, a new mineral, 71.
Patents, list of, 76, 155, 236, 311, 466.
Penzance, meteorological results of observations made at for 21 years, 173.
Peroxide of barium, 152.
Petrosilex, rose-coloured, 149.
Phillips (J.) on the geology of the vale of Pickering, 243.
Phillips, (R.) on ascertaining the purity of sulphate of quina, 111.
Phosphoric acid, its action on albumen, 462.
Phosphorus and sulphur, on the fluidity of, 144; crystallization of phosphorus, 154.
Platina-sand, Russian, 72; native platina, 232.
Popocatapetl, 449.
Potash, chromate of, and sulphate of zinc, Dr. Thomson on the decomposition of, 81.
Prout (Dr.) on simple alimentary substances, 31, 98.

Quina, Mr. R. Phillips on ascertaining the purity of, 111.

Rain-gauge, Mr. Bevan's remarks on Mr. J. Taylor's, 29.

Rain, red, 463.

Riddle (E.) on the occultation of β *Scorpii*, Sept. 25, 1827.

Rifle, a percussion, Lieut.-Col. Miller on, 277.

Robotham's (J.) geometrical problem, 234.

Roget (Dr.) on an apparent violation of the law of continuity, 118, 203.

Salivary concretion, analysis of, 146.

Sea water, quantity of bromine in, 145.

Selenium, bromide of, 147.

Sérullas (M.) on bromide of selenium, 147.

Sepiæ, Mr. Slight on certain habitudes of, 153.

Shell, a percussion, Lieut.-Col. Miller on, 358.

Slight (H.) on certain habitudes of sepiæ, 153.

Smith, Sir J. E., 291, 307, 374, 391.

Societies learned: Royal Society, 46, 128, 370, 436; Linnæan Society, 130, 223, 290, 374, 440; Geological Society, 132, 225, 291, 441; Astronomical Society, 64, 136, 227, 377, 451; Zoological Society, 68; Royal Geological Society of Cornwall, 138; Royal Academy of Sciences of Paris, 139, 305; Royal Institution of Great Britain, 229, 304, 391, 456; London Institution, 154.

Soda, new borate of, 146.

Solids, regular or platonic, Mr. Davies Gilbert on, 161.

Solly, Mr. S. on trap-rocks, 458.

Sound, Dr. Weber on the interference of, 342.

Starch, common and roasted, analysis of, 150.

Steam, new phænomena of, 74.

Steam-engine, on, 40, 219.

Stromeyer, Prof., on the separation of manganese from lime and magnesia, 462.

Subsidence of the German Ocean, 188.

Substances, simple alimentary, Dr. Prout on, 31, 98.

Succulent plants, new, 183.

Sulphur and phosphorus, fluidity of, 144; effects of heat upon sulphur, 152.

Surfaces, curve, Prof. Gauss on, 331.

Tautolite, a new mineral, 398.

Teschemacher (E. F.) on the crystalline form of some salts, 27.

Thermal waters of the Alps, Mr. Bakewell on, 14.

Thermometers, new method of mounting, Mr. Mageough on, 365.

Thomson (Dr.) on the decomposition of sulphate of zinc and chromate of potash, 81.

Tourmalines, analyses of, 460.

Trap-rocks, 458.

Tredgold (T.) on the steam-engine, 219; new theory of the resistance of fluids, 249.

Ultramarine, manufacture of, 232.

Vapour, new phænomena of, 74.

Vegetable products, analysis of some, 150.

Vision, single and erect, on the causes of, 406.

Wackenroder's (Dr. H.) examination of diopside, 349.

Walker (R.) on the artificial production of cold, 401.

Water and bromine, power of, in conducting electricity, 151.

Water-spouts, Mr. Main on, 114.

Weber (Dr. W.) on Savart's experiments on the motions of membranes, 336; on the interference of sound, 342.

Yorkshire, heights of the principal beds of Ingleborough Hill and Moughton Fell in, 13; heights of the hills in the vicinity of Dent, Hawes, and Sedbergh in, 82, 189.

Younge (R.) on Désorme's experiments on currents of air, 282.

Zinc, sulphate of, and chromate of potash, Dr. Thomson on the decomposition of, 81.

Zoology, 46, 68, 117.

END OF THE THIRD VOLUME.

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1828.

PIERS, HARBOURS, EMBANKMENTS, BREAKWATERS, BASINS, LOCKS, QUAYS, DOCKS, MILL-DAMS, ROADS THROUGH MORASSES, FOUNDATIONS OF LIGHT-HOUSES, AQUEDUCTS,

And other Works requiring great Expedition or Durability.

TO enter into a lengthened account of this invention, and the advantages to be obtained by its use in works of the nature above mentioned, would greatly exceed the limits to which the present notice must be confined; but it is conceived that a concise statement of the principle upon which the Caissons are to be constructed, will render their application sufficiently obvious to attract the attention of the public and lead to further investigation; when it is confidently anticipated that their use can be demonstrated to be not only practicable, but in many situations, decidedly superior to the employment of stone in respect both of economy and security.

The plan has already been submitted to the consideration of various individuals whose opinions it was desirable to obtain previously to laying it before the public; and the result has been, expressions of decided approbation and anticipations of complete success by many Gentlemen of the highest rank and most undoubted scientific knowledge.

The Caisson is a hollow metallic box, open generally both at the bottom and top, the thickness of the sides proportioned to the strength and gravity required, and the mode of uniting being by dovetail. It is proposed that each Caisson be seven feet in length, five feet in height, and from two to five feet in width, according to the nature of the work in which it is to be used. Caissons constituting foundations should be closed at bottom; and in raising one tier above another, each layer would become united to those immediately above and below it, by commencing the alternate vertical courses with a half Caisson.

The results of various calculations of the comparative expense of Granite and Cast-Iron Caisson Works, give, in some cases 20 per cent., in others 30 per cent., and even 50 per cent. and upwards in favour of the Caissons; and the advantage in the saving of time, which in works on the coast is obviously of the highest importance, it is estimated will be at least four-fifths in favour of the latter.

In common dock or canal banking the thickness required in the side of the Caisson toward the water would not exceed one inch*; but where exposed to a heavy sea the thickness may be increased (in proportion to the pressure) to one foot or more, the other sides being less, and the interior, in every case, filled with liquid lime and rubble or other suitable material to be found on the spot, so as to form a solid mass girt with metal.

For the better elucidation of the plan, a few of the more simple forms of Caissons and their mode of uniting, as exemplified in the construction of a pier, are engraved herewith.

Figure 1, is the plain oblong square with dovetails at the ends only, applicable to straight lines, either in banking exposed to the water or to the interior of heavy works, as cross-forts or bracings and buttresses to be buried in the earth. This form admits of little variation in its application and none in its strength or gravity beyond what may be gained by increasing the thickness of the sides.—Figure 2, is the most universal form which it appears possible to produce. It may be multiplied to any extent, yet is perfect in itself, requiring no change of form on the side to finish a work, and the ends may be conveniently completed by filling up the dovetail groove with half a portable dovetail.—Figure 3, is the radiated form which may be used in a waved line along the coast where great strength is required; it also applies to piers and bastions. The dotted projection letter *a*, is the half dovetail, which would be required to attach it to the cross-fort; or the radiated Caisson, should it be considered necessary to add another waved line, would give the effect of arch and counter arch.—Figure 4, is a radiated Caisson having extra dovetails for uniting the main line to the bastion. Any angle may thus be gained, simply by moving this Caisson in the bastion in the direction required.—Figure 5, is the termination of a pier with a bastion; the dotted line from *a* to *b* shews the boundary of the battering or sloped bank; the cross-forts are introduced at a suitable distance to ensure great stability; and the inner dovetailed grooves being left in the inner lines, will enable the engineer to add cross-forts or buttresses to any extent that may be required.

To those interested in any of the various Works to which this Plan is applicable and to whom its success would, undoubtedly, be of great importance, the present outline must of necessity be unsatisfactory in extent of information and variety of detail; but a second edition of a Pamphlet is now in the press and will in a few days be published by RIDGWAY and Co. Piccadilly; TAYLOR, Holborn, &c. &c. which will contain a full description of the Invention, accompanied by Plates, Tables of Comparative Expense, &c. A variety of Models have also been cast, which may be inspected at the Offices of Messrs. BUCKLAND and SMITH, 3, Furnival's Inn, London; any enquiries or communications addressed to whom (free of postage) will meet with attention, and of whom Licenses for the use of the Patent may be obtained.

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* "Silica, when mixed with the Oxide of Iron, forms a very fusible compound and prevents any farther oxidation of the Metal." Vide Parkes' Chemical Essays. Vol. 2. P. 492.

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THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

JULY 1828.

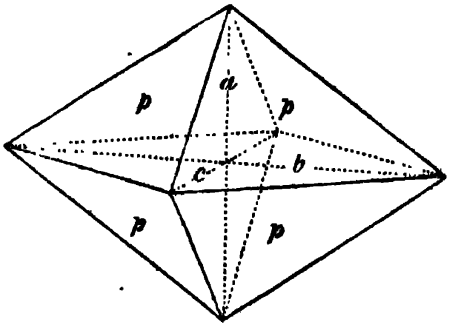
I. *On Herderite, a new Mineral Species.* *By* W. HAIDINGER, *Esq. F.R.S.E.**

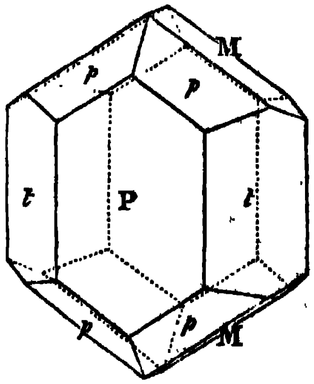
1. *General Description.*

FUNDAMENTAL form, a scalene four-sided pyramid, P=141° 16′, 77° 20′, 116° 3′. (Fig. 1.) Ratio of the axes $a:b:c = 1:\sqrt{2{\cdot}55}:\sqrt{0{\cdot}46}$.

Simple forms. $(\breve{P}-2)^4(o) =$, 149° 50′, ; $(\frac{4}{3}\breve{P}-2)^3(n) =$, 134° 35′, .; P(p); $(\breve{P}r+\infty)^5(t) =$ 115° 7′; $(\breve{P}+\infty)^6(s) = 42° 58′$; $\breve{P}r$(M) = 115° 53′; $\breve{P}r+\infty$ (r); $\bar{P}r+\infty$ (P).

Combinations observed. 1. $\breve{P}r \cdot P \cdot (\breve{P}r+\infty)^5 . \bar{P}r+\infty$. (Fig. 2.)





2. $\breve{P}r.(\breve{P}-2)^4.(\frac{4}{3}\breve{P}-2)^3.P.(\breve{P}r+\infty)^5.(\breve{P}+\infty)^6.\breve{P}r+\infty.$ $\bar{P}r+\infty.$

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* Communicated by the Author.

Cleavage distinct, parallel to the faces M, but interrupted; also perpendicular to the axis, the latter only in detached portions of very bright and even faces, and faint indications parallel to P. Fracture small conchoidal.

Surface, M very smooth, and delicately streaked parallel to its edges of combination with P, and resembling in this respect all the faces of the pyramids, *n*, *o*, and *p*, situated between them.

The faces *r* and *s* are very narrow, and somewhat curved. Those marked *t* and P, have a peculiar granulated aspect, but they are at the same time pretty smooth, particularly the latter.

Lustre, vitreous, slightly inclining to resinous. Colour several shades of yellowish- and greenish-white; streak white, strongly translucent.

Very brittle. Hardness = 5·0, equal to that of apatite. Specific gravity =2·985.

2. *Observations.*

1. I observed and examined the characters of this species in the summer of 1823, but deferred publishing the description of it, with a view of collecting further observations on other varieties of the same species, an expectation which was not realized. The only specimen of herderite, at present known, is in the Wernerian Museum at Freiberg. It was pointed out to me by M. Von Weissenbach, then keeper of the museum, as containing crystals, whose forms he could not exactly refer to those of apatite, among the varieties of which it was exhibited. The different aspect of the faces *p* and *t*, the former being smooth or but faintly streaked parallel to their intersections with P, while the latter are granulated, showed that the forms did not belong to the rhombohedral but to the prismatic system; and I did not hesitate in pronouncing the mineral to be a new species, which I requested permission to examine more minutely. This permission was very liberally conceded. Mr. Breithaupt, who was then present, and had himself at a former period placed the specimen in the cabinet of Werner, likewise concurred in acknowledging the species to be a new one.

Through the kind intercession of Mr. Reich, now keeper of the museum, I was favoured, during my stay at Berlin in the winter of 1825, with some fragments of the specimen for analysis, by Baron Von Herder, the present Ober-Berghauptmann, or director of every thing connected with mining proceedings in Saxony. It is in compliment to that nobleman, that I propose the name of *Herderite* for the species; and I feel particularly gratified in thus expressing to one of my earliest mineralogical friends, the acknowledgement of the many instances of his having communicated to me rare specimens for examination, particularly during my stay at Freiberg.

2. Herderite occurs imbedded in fluor, in the tin mines of Ehrenfriedersdorf, in Saxony. It resembles apatite, with which it was formerly confounded, in a remarkable degree; particularly some of those named asparagus-stone: such as the variety from Zillerthal, in Salzburg, and that from Hof in Gastein in the same country, which is found accompanying the axotomous iron-ore of Mohs, and still more so certain pale greenish-white masses of the same species, which occur, though in small quantity, along with the zoisite from the Saualpe in Carinthia. The resemblance among those species is sufficient to class the herderite in the genus *Fluor-haloide* of Mohs, in which it may be henceforth included as the "*prismati Fluor-haloide.*"

II. *Reply of* Drs. Tiedemann *and* Gmelin *to the Remarks of* Dr. Prout *inserted in the Annals of Philosophy (Second Series)*, vol. xii. p. 405: *Communicated by* Thomas Thomson, *M.D. F.R.S. &c. Regius Professor of Chemistry in the University of Glasgow.*

THE more satisfied we are of the obligations which the doctrine of digestion lies under to Dr. Prout, and the less intention we had to attack him unjustly, the more do we consider it as our duty to discuss the complaints which he has made, so far as we are concerned,—to defend ourselves, where we think ourselves in the right, and to acknowledge our mistakes where we think ourselves in the wrong.

Dr. Prout's complaints are the following:

1. *We have led our readers to believe that Dr. Prout denies the presence of every other free acid in the contents of the stomach, except the muriatic; which is not the case.*

Yet we could draw no other conclusion from Prout's paper*, than that he denied the presence of every other acid. For

a. He says (page 118), "the experiments above mentioned seemed to preclude the possibility of the presence of any destructible acid; and the only known fixed acids likely to be present were the sulphuric and phosphoric; the muriate of barytes, however, neither alone nor with the addition of ammonia, produced any immediate precipitate, showing the absence of these two acids in any sensible quantity, and still further confirming the results as before obtained."

b. Now unless the absence of other free acids be taken for granted, how can Prout's method of determining the free muriatic acid in the contents of the stomach be considered as answering the object in view? When free acetic acid, for example, is present, the potash employed to saturate the free acid will not only saturate the free muriatic acid, but also the free acetic acid. Now as Prout considers his method as quite accurate, he must necessarily presuppose the absence of all other free acids. Indeed Dr. Prout would have had reason to complain of us, if we had led our readers to believe that he admitted the presence of any other free acid in the liquid of the stomach;—acetic acid, for example (which is not the case in the paper to which we have alluded). For on such a supposition his method would be no longer accurate; and we should therefore have placed him in the situation of contradicting himself.

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* Phillips's Annals of Philosophy, vol. viii. p. 117.

Dr. Prout, in his remarks upon our treatise, has for the first time, so far as we know, admitted that he has several times found acetic acid along with muriatic acid in the liquid of the stomach. Thus our observations have been confirmed; and it follows as a consequence, that his method of determining the quantity of free muriatic acid in the stomach cannot be relied upon. That the free acetic acid always proceeds from the food, we cannot believe; as we have frequently obtained it by the distillation of the liquid in the stomach of animals which had long fasted, and whose gizzards had been stimulated by the swallowing of stones, &c.

2. *We have in our account of Prout's method passed by the most important point of the whole, because it constituted a check upon the rest of the procedure.*

Undoubtedly the determination of the muriatic acid in the sublimed sal ammoniac, obtained after the saturation of the liquid of the stomach with potash, is very important; and it constitutes a still more important check, when one denies (as Prout evidently did in his first paper) the presence of any other free acid in the liquid of the stomach. But as we had discovered the presence of several volatile acids, particularly of acetic acid, we could not estimate this check as of the least value. For when the liquid of the stomach, together with muriate of soda (and potash), contains muriate of ammonia and free muriatic and acetic acids, and we neutralize it exactly with potash, evaporate it to dryness and sublime, the acetate of potash and sal ammoniac which it contains mutually decompose each other, and are converted into chloride of potassium and acetate of ammonia. So that in consequence of the presence of acetic acid, less sal ammoniac will sublime than the liquid of the stomach originally contained. Indeed none at all will sublime, if the quantity of acetic acid be sufficiently great. It is probably owing to this cause that Prout found no sal ammoniac in the liquid from the human stomach in two cases; although the complete absence of this salt from such liquids is very unlikely[*]. From these observations it seems probable that the proof of the presence of free muriatic acid,—which Prout has lately endeavoured to deduce from the circumstance, that when the liquid of the stomach after having been neutralized with potash was evaporated to dryness, and the residue exposed to a red heat, this residue did not act as an alkali but constituted a neutral salt,—is not satisfactory. For a liquid of the stomach which contains no free muriatic acid, but muriate of soda, muriate of ammonia, and free acetic acid, must (if the quantity of acetic acid be not too great) after neutralization with potash and ignition contain only chloride of potassium and chloride of sodium.

We are of opinion that Dr. Prout himself in his last statement has admitted that when an organic acid is present, his process is insufficient, and has thus confirmed our own previous statements.

3. *We have misrepresented Prout's opinion respecting the appearance of albumen in the intestines, by making him maintain that no albumen exists in the liquid of the stomach even when the animal takes food containing albumen; but that it shows itself first in the duodenum, in consequence of the union of the liquid of the stomach with the bile and the pancreatic juice.*

After again perusing Prout's former paper[†], we must acknowledge that we have stated his opinion on this subject quite inaccurately. Whether this proceeded from misunderstanding his meaning, or from an inaccurate extract from his paper, we cannot say. We request the reader of our work to obliterate the passage which refers to this misunderstanding on our part.

We trust that these explanations and acknowledgements will obviate the complaints of this celebrated chemist and physician, to whom the chemical part of physiology and pathology lies under so many obligations. We have only to express our high satisfaction at his statement, that his observations on digestion agree with ours, and confirm them in the most important points.

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[*] It is true, Prout determined the quantity of sal ammoniac by another method. From the total quantity of muriatic acid contained in the liquid, he subtracted the portion united to a fixed alkali, and that which existed in a free state. The remainder must represent the portion combined with ammonia. But as the presence of acetic acid would lead him to overrate the quantity of free muriatic acid, the sal ammoniac as thus estimated would be too little or none at all.

[†] Annals of Philosophy (first series), vol. xiii. p. 12.

III. *On*

III. *On the Latitudes and Difference of Longitude of Beachy Head and Dunnose in the Isle of Wight, as laid down in the Trigonometrical Survey of England; and the Length of a Degree perpendicular to the Meridian at the Latitude of Beachy Head.* By J. Ivory, *Esq. M.A. F.R.S.*[*]

THE investigation of the figure of the earth by measurements of the meridian, has given rise to a question of great moment. Although the entire arcs agree very exactly with the elliptical figure, yet on comparing the parts into which the same arc is subdivided, the greatest irregularity is found to prevail. This is so much the case in the arc measured between Dunnose and Clifton, in England, that the length of a degree is found to decrease in advancing from south to north, instead of increasing as the theory requires. To what cause can so great an anomaly be ascribed? In attempting to throw some light on the irregularities of the English arc, I have been led to examine the operations at Beachy Head and Dunnose; and, reserving the discussion of the original question to a future occasion, my present intention is to communicate the observations I have made respecting the operations alluded to, as they materially affect a capital part of the Survey.

If a geodetical line be drawn through the station at Beachy Head, perpendicular to the meridian of Greenwich, the meridional distance of the line from Greenwich is, according to the Survey, 44888 fathoms: and if a plane parallel to the equator be drawn through the same station, this plane will meet the meridian 17 fathoms more to the south[†]; so that the meridional distance between Greenwich and the parallel of latitude passing through Beachy Head, is 44905[‡] fathoms. In order to find the difference of latitude between the two places, the terrestrial arc must be converted into degrees of a great circle of the heavens. In the Survey 60851 fathoms is allowed to a degree, which undoubtedly is too much. For if we divide the terrestrial arc measured by Col. Mudge, by its amplitude, we shall get 60826 fathoms for a degree at the latitude 52° 2′; and even this length must exceed a degree at the more southern point in the middle between Greenwich and Beachy Head. The latitudes of the two places being known nearly, I have employed the formula (C), p. 433, Phil.

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[*] Communicated by the Author.

[†] Survey, vol. i. p. 294.

[‡] This length should be multiplied by 1·00007, in order to bring it to a general standard for the purpose of comparison with other measurements. (Phil. Trans. 1821, p. 93). The same observation applies to all the other lengths that occur. I have omitted to make the correction, which however does not sensibly affect any of the conclusions.

Mag. for June last, and have found for the length of a degree,

At Greenwich 60826 fathoms;
At Dunnose 60815;

and the mean of these, or 60820·5, is the proper rate of conversion in the present instance. The difference of latitude between Greenwich and Beachy Head will now be 44′ 18″; and if the latitude of Greenwich be taken at 51° 28′ 39″, that of Beachy Head will be 50° 44′ 21″.

The distance between the parallels of latitude passing through Beachy Head and Dunnose, as found in the Survey, is 7376·5 fathoms; which being converted into degrees, at the rate of 60815 fath. to one degree, gives 7′ 16″·7 for the difference of latitude of the two stations. The latitude of Dunnose is therefore 50° 37′ 4″·3.

But as the azimuths at the extremities of the geodetical line drawn between the stations are given, we may deduce the latitude of Dunnose from that of Beachy Head by another method. Let B and D denote the azimuths, and λ and λ' the latitudes, of Beachy Head and Dunnose; then, a and $a(1-\varepsilon)$ being the axes of the terrestrial spheroid, the general property belonging to every geodetical line, will give this equation in which the square of ε is neglected,

$$\frac{\cos\lambda' \sin D}{\sqrt{1-2\varepsilon\sin^2\lambda'}} = \frac{\cos\lambda \sin B}{\sqrt{1-2\varepsilon\sin^2\lambda}};$$

from which we easily derive this formula, $\log.\ \cos\lambda' = \log.\left(\frac{\cos\lambda \sin B}{\sin D}\right) + M\varepsilon(\sin^2\lambda - \sin^2\lambda')$, where $M = \cdot 43429$ &c., the modulus of the common logarithms. Now, computing by this rule, we shall find $\lambda' = 50° 37' 5''\cdot 65$. This result is as little different from the former one as can reasonably be expected, considering that the methods of calculation are very different, and likewise proceed upon experimental data quite independent of one another. Both results are confirmed by actual observation, Capt. Kater having found 50° 37′ 5″·27 for the latitude of Dunnose*. We may therefore definitively fix the latitudes of the two stations as below:

Beachy Head...............	50°44′ 21″
Dunnose....................	50 37 5

and it is very improbable that either of these results errs so much as 1″ from the truth.

The two latitudes we have found are so little different from those in the Survey, as to produce no sensible change in the ulterior calculations of the difference of longitude and the

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[*] Phil. Trans. 1819, p. 413.

length of a degree perpendicular to the meridian. According to the Survey, a degree perpendicular to the meridian, at the middle latitude between the two stations, is no less than 61182 fathoms, or about 200 fathoms more than in the spheroid, which has been found to agree so well with all the arcs of the meridian that have been most exactly measured. If therefore we admit that the method of investigation pursued in the Survey is exact, we should have an undeniable proof that the spheroid which represents distances on the meridian so exactly, fails entirely in the case of measurements, the extreme points of which are different in longitude. But a little reflection will show that the theorem laid down in the Survey for finding the difference of longitude is not rigorously exact. In the geometrical demonstration of the theorem it is tacitly assumed, that a geodetical line drawn between two points in different meridians, is contained in one plane. But such a line has a double curvature; and the two tangents which mark its initial and final directions are not both contained in any plane passing through the extreme points of the line. Therefore if the difference of longitude, and the latitudes of two points on the surface of a sphere and a spheroid, be the same, it is not strictly true that the sum of the azimuths in one case is equal to the like sum in the other case. The method is only an approximation; and it cannot be confidently relied on until it is proved that it approximates to the truth without sensible error; which is the more necessary to be done, because the whole investigation turns on very small quantities, a few seconds in the longitude producing a great variation in the length of the perpendicular degree. I have therefore been induced to view the matter in a different light, as in this problem: To find the difference of longitude of two points on the surface of a given spheroid, the latitudes of the points and the length of the chord drawn between them, being known. In solving this problem we may likewise assume that the two points are little different in latitude.

Let a and $a(1-\epsilon)$ represent the axes of the spheroid; λ and λ' the latitudes of the two points; γ the length of the chord between them; and, neglecting the square of ϵ, put

$$p = \frac{\cos\lambda}{\sqrt{1-2\epsilon\sin^2\lambda}}, \qquad u = \frac{\cos\lambda'}{\sqrt{1-2\epsilon\sin^2\lambda'}},$$

$$q = \frac{\sin\lambda\,(1-2\epsilon)}{\sqrt{1-2\epsilon\sin^2\lambda}}, \qquad t = \frac{\sin\lambda'\,(1-2\epsilon)}{\sqrt{1-2\epsilon\sin^2\lambda'}},$$

then ap and au will be the perpendiculars drawn from the two points to the polar axis, and aq and at the perpendiculars to the plane of the equator. Put ω for the angle between the meridians of the two points, or the difference of longitude; then we shall have this equation

$$\frac{\gamma^2}{a^2} = (u-p\cos\omega)^2 + p^2\sin^2\omega + (q-t)^2;$$

and, because $\cos\omega = 1-2\sin^2\frac{\omega}{2}$, the same equation may be thus written:

$$\frac{\gamma^2}{a^2} = (p-u)^2 + (q-t)^2 + 4pu\sin^2\frac{\omega}{2}.$$

Now we have,

$$p-u=(\cos\lambda-\cos\lambda')\,\{1+\varepsilon-\varepsilon(\cos^2\lambda+\cos^2\lambda'+\cos\lambda\cos\lambda')\}$$

$$q-t=(\sin\lambda-\sin\lambda')\,\{1-2\varepsilon+\varepsilon(\sin^2\lambda+\sin^2\lambda'+\sin\lambda\sin\lambda')\}.$$

And if we put $m=\frac{\lambda+\lambda'}{2}$, $n=\frac{\lambda-\lambda'}{2}$; by substituting and neglecting the term multiplied by $\varepsilon\sin^3 n$, because n is a small angle, we shall get,

$$p-u = 2\sin n\sin m\,(1+\varepsilon-3\varepsilon\cos^2 m\cos^2 n)$$

$$q-t = 2\sin n\cos m\,(1-2\varepsilon+3\varepsilon\sin^2 m\cos^2 n);$$

and hence,

$$(p-u)^2+(q-t)^2 = 4\sin^2 n\left(1+2\varepsilon\sin^2 m-4\varepsilon\cos^2 m)\right);$$

or, which is the same thing,

$$(p-u)^2+(q-t)^2 = 4\sin^2\frac{\lambda-\lambda'}{2}\left(1-\varepsilon-3\varepsilon\cos(\lambda+\lambda')\right).$$

Again we have,

$$pu = \frac{\cos\lambda\cos\lambda'}{1-\varepsilon(\sin^2\lambda+\sin^2\lambda')}:$$

but as λ and λ' are nearly equal, we may take $2\sin^2\frac{\lambda+\lambda'}{2} = 1-\cos(\lambda+\lambda')$ for $\sin^2\lambda+\sin^2\lambda'$; consequently,

$$pu = \frac{\cos\lambda\cos\lambda'}{1-\varepsilon+\varepsilon\cos(\lambda+\lambda')}.$$

Lastly, we have $a = p\Delta$; Δ being the length of a degree on the equator of the spheroid, and p the number of degrees in an arc equal to the radius. All the values being substituted, we get,

$$\frac{\gamma^2}{4p^2\Delta^2} = \sin^2\frac{\lambda-\lambda'}{2}\left(1-\varepsilon-3\varepsilon\cos(\lambda+\lambda')\right)+\frac{\cos\lambda\cos\lambda'\sin^2\frac{\omega}{2}}{1-\varepsilon+\varepsilon\cos(\lambda+\lambda')}.$$

From this expression the arc ω may be computed by means of these formulas; viz.

$$\text{Log.}\sin u = \log.\left(\frac{2p\Delta}{\gamma}\sin\frac{\lambda-\lambda'}{2}\right)-\frac{M\varepsilon}{2}-\frac{3M\varepsilon}{2}\cos(\lambda+\lambda')$$

$$\text{Log.} \sin \frac{\omega}{2} = \log.\left(\frac{\gamma \cos u}{2p\Delta}\right) - \tfrac{1}{2} \log. (\cos \lambda \cos \lambda') - \frac{M\varepsilon}{2} + \frac{M\varepsilon}{2} \times \cos(\lambda + \lambda').$$

Applying this method to the case of Beachy Head and Dunnose, we have the latitudes as before determined: the chord between them, or γ, is equal to 339397·6* feet, or 56566·3 fathoms: and, with these data, we get, in the same spheroid as before,

$$\omega = 1^\circ\ 27'\ 5''{\cdot}78,$$

which is 18″ more than according to the Survey.

Now the length of a geodetical line drawn from Beachy Head at right angles to the meridian, and limited by the meridian of Dunnose, is 336119† feet, or 56020 fathoms. Let A be the amplitude of this arc; then we get, by the usual method,

$$\text{Tan } A = \cos \lambda \tan \omega, \quad A = 55'\ 7''{\cdot}57.$$

And if we lengthen the arc in the proportion of the amplitude to 1°, we shall get 60973 fathoms for a degree perpendicular to the meridian at the latitude of Beachy Head, which is 200 fathoms less than according to the Survey. This result may be verified by comparing it with the length of a perpendicular degree at the given latitude. The expression of this degree is,

$$\frac{\Delta}{\sqrt{1 - (2\varepsilon - \varepsilon^2) \sin^2 \lambda}} = \Delta . \left\{ 1 + \varepsilon \sin^2 \lambda + \varepsilon^2 \left(\frac{3}{2} \sin^4 \lambda - \frac{\sin^2 \lambda}{2} \right) \right\}:$$

and, by substituting the numbers, the length comes out 60974, only one fathom more than what has been deduced from the measurements in the Survey.

The difference of longitude we have found is only 1″ more than what Dr. Tiarks has assigned (Phil. Trans. 1824) as resulting from his chronometrical observations. This is no doubt a great confirmation of the theory, but it has not the weight of a direct experiment; because it is deduced from the observed difference of longitude between Dover and Falmouth, by making a proportional allowance, which is a method of proceeding in some degree precarious. As there is no doubt about the accuracy of the observed quantities in the Survey, it might be worth while to compute from the data it contains, by rectifying all the calculations, the difference of longitude between Dover and Falmouth, in order to compare it with the result obtained directly by experiment.

The main purpose of this article is now accomplished. It has been clearly proved that the same spheroid which represents with so much accuracy distances on the meridian, is no less exact when applied to measurements perpendicular to the meridian. At least this is the case in England in the instance we have examined; and a little time will show whether the same conclusion is confirmed or contradicted by the geodetical operations now executing on the continent.

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* Survey, vol. i. p. 295. † Ibid. p. 297.

June 13, 1828. J. IVORY.

IV. *Additional Remarks on the Artificial Production of Cold.* *By* RICHARD WALKER, *Esq., of Oxford.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

PERCEIVING an *erratum* in my communication inserted in the Philosophical Magazine for the present month, which renders my meaning rather obscure, I request you will have the goodness, in your next, to correct it thus:—at page 404, 8th line from the bottom, *for* "freely"—*read* "freshly."

I shall avail myself of this opportunity to present, in a cursory way, a few additional observations.

The drier and finer the mixed powder of sal ammoniac and nitre is, the better; and the pulverization is best effected, in the first instance, with a heated pestle. Glauber's salt in an efflorescent state, or which by long keeping or from access of air has changed to a powder, is unfit for the purpose; in this state producing heat by solution in water. The best way of preparing the frigorific mixture is by previously placing the powdered Glauber's salt, and giving it a level surface, at the bottom of the vessel, and upon that the mixed powder of sal ammoniac and nitre; adding first about half the quantity of water, and immediately after the remaining portion, stirring the whole together each time. The vessel containing the powdered salts, as above stated, may remain thus any convenient time before adding the water. [Care must be taken to stir the evaporating mixture towards the end of the process, and not to urge it too far.] Nitre being a much cheaper article than sal ammoniac, more easily reduced to powder, and producing about 16 degrees of cold by solution in water, may supersede the use of the mixed powder for cooling the water in which wine is placed. This powder, moreover, is useful, occasionally, as an addition to mixtures of ice and salt, to increase the power and accelerate the process.

The proportions of the articles given in my former paper are adapted to the temperature of 50°; at a higher temperature, of course, the water will dissolve a somewhat larger portion of the salts, and the effect will be proportionably greater. Thus the most powerful mixture, given in my table of frigorific mixtures, consisting of phosphate of soda, nitrate of ammonia, and diluted nitric acid, will, when mixed at the temperature of 50°, produce a cold of 21° below 0; and if mixed in due proportions at 100°, it will produce, in an instant, a cold of 20°; viz. a reduction of *eighty degrees*. By means of this mixture, as I have been informed, water has been frozen solid " under the line." I am, Gentlemen,

Your most obedient servant,

Oxford, June 10, 1828. RICHARD WALKER.

V. *Observations on the Geology of the Hyderabad Country*[*].

THE country around Hyderabad is composed entirely of granite, intersected by quartz, which generally runs north and south; and by trap, which has no definite direction.

The hills are generally in ridges. In some instances they are insulated, of a mamillary form, or abrupt and precipitous.

The ridges are covered with detached masses of rock, and frequently (when seen at a little distance) have more the appearance of heaps of loose stones than of solid hills. The mamillary hills are almost always devoid of vegetation, having a smooth surface, with large detached lamellæ lying loosely on their sides, and apparently ready to slide or tumble down on the slightest impulse into the neighbouring valleys.

The insulated hills often present on one or more sides a smooth, perpendicular surface, which makes a very sudden curve at the top, or undulates, and thus contracts the summit of the hill.

Sometimes we find the surface of the granite forming part of an immense curve, and rising very gently and to a small height above the surrounding plain. In other instances it is waved, and presents a great variety of outline.

Huge blocks of granite are every where strewed over the country, and are often piled over each other in the valleys, or on the sides or summits of the hills, giving rise to the most fantastic shapes, and often closely resembling ruined buildings. It is not uncommon to see three or four immense masses of granite placed above each other, with their surfaces nicely adapted, having somewhat the appearance of the ruin of an ancient column, which might be expected to be soon levelled with the ground, by the agency of the weather.

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[*] From the Transactions of the Literary Society of Madras. Part i. page 79.

All the granite of Hyderabad is stratified or lamellar. The strata and lamellæ vary in thickness, from less than an inch to many feet. They have no definite direction or dip; but are generally curved, sometimes to a small extent horizontal, waved, or perpendicular.

The granite on one side of a small hill, close to that of Shapoor, near the Beema, has the appearance (when seen at a little distance) of being columnar; but when it is examined more closely, it is evident that this appearance arises from the following circumstance:—The lamellæ of the granite are perpendicular, and had formerly made a very rapid curve at the top. By the influence of the weather, this curve has been worn down, and has thus allowed the inferior perpendicular part of the lamellæ to separate a little from each other; and accordingly, when seen transversely, they closely resemble columns.

The internal structure of the granite is almost always small granular*. The proportions of its constituent parts vary exceedingly. In many instances the mica is entirely wanting; and when situated near quartz, the granite and quartz are frequently found to pass gradually into one another.

The colour of the granite is sometimes red, in other instances gray, white, or blackish, according to the colour of the felspar, and according as one or other of the constituent parts predominate. Different colours often occur at very small distances in the same stratum, or lamella; and it is not uncommon to meet with strata of different colours resting on one another.

Frequently nodules and small veins of granite, having a very large proportion of mica, and occasionally veins of pure mica, are found in the common granite, from which (in some cases) they easily separate when the rock is broken; but in other instances they are intimately connected with, and gradually pass into the surrounding rock.

The quartz and trap, by which the granite is every where intersected, occur under the form of mountain masses and veins. Sometimes, though more rarely, the trap is found in nodules. The veins vary from a few inches to many miles in length. Their junction with the bounding rock is sometimes distinct, while in other cases they are intermingled at their sides with the neighbouring granite.

The quartz is sometimes intermixed with felspar, which makes it much more perishable than when pure; and accordingly when this is the case, we generally find it wearing down and becoming disintegrated.

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* I will not venture to assert that it is invariably small granular, for my observations have not been sufficiently extensive.

The trap is found under the forms of greenstone and basalt. It is either tabular, massive, or in globular concentric lamellar concretions, with occasionally disseminated crystals of augite. The globular variety is very easily acted on by the weather, in consequence of which it is in many situations completely disintegrated and converted into a black soil. In some places, where this variety of trap occurs in great abundance, it is worn down into small detached globular masses, which are every where strewed over the ground.

Almost all the granite of Hyderabad is quickly disintegrated when exposed to the atmosphere, and assumes a globular or irregular form when decomposing. Every where there are immense accumulations of disintegrated granite, at the bottom of the hills and in the valleys. I have known instances of wells being dug through it to the depth of sixty feet, without penetrating to the original rock. At the surface of the ground it is loose, but at considerable depths it is more or less perfectly consolidated; and the deeper we penetrate into it, the more perfect is its cohesion. It is not uncommon to meet with small quartz veins running through this consolidated debris, in various directions; and in many instances there is an appearance of imperfect stratification.

All the low valleys are covered by a plastic blackish coloured soil, generally known by the name of cotton ground. It varies in depth from a few feet to many fathoms; and when a section of it is examined (which can be done in those places where it is worn down by rivers), it is generally found distinctly arranged in strata, which are sometimes separated by thin layers of sand or gravel. These strata vary in thickness, are sometimes horizontal, in other instances waved, or more or less inclined to the horizon.

I have not had an opportunity of analysing this clay; but that its composition is by no means uniform, may be inferred from the circumstance of its outward appearance varying considerably in different situations. Sometimes it is of a blackish gray colour, and is somewhat friable; while in other cases it has a yellowish or whitish gray colour, and is much more cohesive.

At first sight one would imagine, that the Hyderabad country has at one time been subjected to the agency of some great destroying cause which has fractured and torn asunder the hills, and precipitated their fragments into the neighbouring plains. But upon closer examination, I think we are naturally led to conclude, that the gradual operation of causes which are still in existence, have produced those effects which many would attribute to the operation of very powerful agents. In short, I believe that all these phænomena are the result of the long continued agency of the weather.

It is well known that masses of granite which have been detached from the neighbouring hills, are worn down and disintegrated by the weather; and also, that the lamellæ or strata of granite, which still retain their original situation, when exposed to the atmosphere, split and slide down into the adjoining valleys*. Since then we have ocular proofs of the hills being broken down and disintegrated by the weather, and since these effects are never known to be produced by any other cause, can we hesitate to conclude, that all the accumulations of debris and detached masses of granite have originated in the same manner? Effects are daily produced under our immediate observation, exactly similar to those to be accounted for; and although, at first sight, the magnitude of the effect may appear out of proportion to the cause, yet the latter will be sufficiently adequate, if it be admitted, that it has continued to operate through an immense lapse of ages,—a circumstance which no one can possibly doubt.

It may be argued, that earthquakes are much more powerful than the slow agency of the weather, and more adequate to produce the effects under consideration. Earthquakes are certainly among the most powerful causes with which we are acquainted, in effecting changes on the crust of our globe; yet their effects are very different from those I am attempting to account for. The lamellæ and strata of the Hyderabad granite gradually break up and scale off, exactly in the same manner as we detach successively the layers of an onion. But this appearance is very different from what we should be led to expect, had it been produced by earthquakes; for in that case the ruined appearance of the granite would not have been confined to the surface, but would have extended to the centre of the hills.

One of the most curious and interesting appearances in the geology of the Hyderabad country, is that already mentioned, of large masses of granite resting firmly on one another, in the form of ancient ruins. These are quite different from the masses which have been detached from the neighbouring hills, and afterwards heaped confusedly together; for their surfaces are closely adapted, and four or five masses are often placed firmly on each other, as if by art. Sometimes they occur on the summit of a hill; in other instances, they are found completely insulated in a plain.

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* The same effects are produced upon granite in India, by great degrees of heat, alternating with moisture, as those that are produced upon granite in the Alps of Switzerland, by intense frost succeeded by thaw.

From all the circumstances connected with these masses, I think we must conclude, that at present they continue to occupy their original positions; that they are the slight remains of strata which have been gradually worn down all around them; and that they now stand as monuments of what the depth and nature of these strata formerly were. This conclusion is as legitimate as that the strata on the opposite coasts of England and France were once continuous, deduced from the circumstances of their corresponding in their nature, relative position, and direction. In order to show more clearly on what grounds I rest the above conclusion, I will consider the subject a little more in detail. We often observe a hill composed of successive strata or lamellæ, the most superficial of which are more or less detached and broken up; that round its base are large accumulations of debris and detached fragments; and that on its summit are three or four masses resting firmly on one another, with their surfaces accurately adapted, except perhaps at their edges, where they have been affected by the weather. Now as we have here ocular proofs that a number of strata (which formerly belonged to this hill) have been detached and worn down; and as it would almost amount to an absurdity to suppose that the masses on the summit have been conveyed from a distance, and placed there with their surfaces accurately corresponding, we must conclude that the hill was formerly much higher than at present; and that while a number of its strata have been gradually worn down and swept from its surface, the masses at its summit (which at one time formed part of these strata) have remained steadfast in their original situations, probably from their being more durable, or from their horizontal position. It is very evident how this must happen, when it is recollected how the lamellæ of the granite are broken down and separated from those beneath. They split in various directions, and in this manner form a number of separate masses, which slide down the sides of the hill. Now, it is clear, that the part of the lamellæ on the summit has every chance of remaining stationary, for it rests horizontally; and while all the detached pieces around it slide down into the neighbouring valleys, it will maintain its situation. When the next bed is exposed to the atmosphere, and becomes detached from that beneath, of course the part on the summit immediately under the fragment which remained stationary in the first instance, has every chance of continuing in its situation; and thus in the course of time the appearance above described will be produced. The same explanation is to be given of the origin of those masses which are found insulated in the plains. I imagine that they rest on the summit of what were formerly hills, but which are now completely buried under their own ruins.

The peculiar arrangement and structure of the quartz and trap in the granite of Hyderabad, afford abundant proofs of the correctness of Mr. Jameson's views of the formation of veins, viz. that they are of simultaneous formation with the rock which they traverse. In the Hyderabad country, we find quartz and trap under the forms of veins, nodules, and mountain masses, sometimes perfectly distinct from the surrounding granite, in other instances intermingling with it, and gradually passing into it. With these facts before us, can we doubt that these rocks are of cotemporaneous origin?

I have often been surprised that theorists, in their attempts to explain the various phænomena presented by the crust of our globe, have never employed causes of whose existence we have certain proof, and with whose effects we are well acquainted; but on the other hand have assumed the existence of causes of which we never had experience, and whose effects we never witnessed. Huttonians assume the existence of a central fire*, which they contend to be the cause of the consolidation of the debris of former hills, and consequently of its conversion into new rocks. But that such a supposition is by no means necessary, is evident from the circumstance, that this consolidation often takes place without the assistance of heat. I have already mentioned that the debris of the Hyderabad granite becomes gradually consolidated, merely by pressure; and that

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* The heat of the Huttonians must be an extremely convenient as well as powerful agent, for it can both liquefy and consolidate bodies. At one time it can inject a flood of melted basalt into the superincumbent rocks; at another time it can consolidate sandstone, and other secondary rocks, at the bottom of the ocean. There are two facts which are very hostile to this theory;—first, the greater the pressure, the greater is the obstacle to the fusion of a body; second, the greater the heat, the greater is the opposition to the consolidation of a body. Now, the Huttonian theory requires that the two great agents which it employs, viz. heat and pressure, should act in concert; the heat to liquefy, or (as occasion may require) to consolidate bodies; the pressure to prevent the escape of volatile substances, which might be otherwise dissipated. These two forces, however, must oppose each other; for heat is one of the most powerful agents with which we are acquainted in separating the particles of bodies, while pressure brings them closer to each other. The pressure of the ocean, therefore, although in all probability equal in itself to consolidate the debris of former hills into new rocks, may not be sufficient for that purpose, if opposed by a heat sufficiently powerful to liquefy granite and trap; and a heat that would be equal to the melting of trap at the earth's surface, would be by no means adequate to do so under the pressure of mountains.

the greater the pressure, the more perfect is the consolidation. This is a power with whose effects we are well acquainted. By bringing the particles of bodies closer to each other, pressure becomes a powerful cause of consolidation; and I am convinced that without the assistance of any other agent, it is one of the most general and powerful causes of the changes which happen in the mineral kingdom.

Camp Kulle-dghec, 1st July, 1824.

Rock Specimens from the Vicinity of Hyderabad.

The specimens from No. 1. to 13. are the most common varieties of granite in the Hyderabad country.

1. 2. 3. 4. are from Bowenpilly, several miles to the north of the city of Hyderabad.

5. and 6. are from Shumshabad, about twelve miles west of the city; 5. is from a stratum about half an inch thick, resting on that from which 6. was taken.

The specimens from 7. to 20. inclusive, are from Moula Alley.

Moula Alley hill is a large mass of lamellar granite, of a mamillary form, having a smooth surface, perfectly devoid of vegetation, except on a very few spots, where the disintegrated granite has formed a superficial bed of soil. The lamellæ of the granite in some places scale off, split in various directions, and gradually slide or fall down into the neighbouring valleys, where they continue to break down into still smaller masses, until they become completely disintegrated. The lamellæ vary from a few inches to many feet in thickness.

9. and 10. occur in great abundance in the hill of Moula Alley.

12. Red granite with very little mica. The bed from which this specimen was taken, rests upon No. 9.

13. Granite with the mica in large concretions.

14. White granite without mica:

15. Granite containing a vein of mica.

16. A variety of granite with mica predominating, from a nodule in one of the common kinds of granite.

17. From a nodule in the granite.

18. From a nodule of trap in the granite.

19. From a trap vein about twelve feet thick, with part of the contiguous granite adhering to it.

20. Trap passing gradually into granite.

21. 22. 23. 24. from a vein of quartz in the granite, extending from near the city of Hyderabad, several miles in a northerly direction. This vein is of a very considerable magnitude.

nitude. Being of a more durable nature than the granite, it has been much less affected by the weather; and while the granite in its vicinity has been worn down, and in a great measure levelled, it has remained in the form of a ridge. In several places the quartz is intermixed with felspar, which makes it more liable to be acted on by the weather; and accordingly we find that in these places it is worn down, and the continuity of the ridge is thus interrupted.

25. From a trap vein which extends from the cantonment of Secunderabad in a westerly direction. When it approaches the quartz vein described above, it divides into two or three branches, and penetrates the quartz in several places.

26. Red granite without mica, from the vicinity of the quartz.

27. Quartz and felspar in large concretions, from the vicinity of the quartz.

28. Granite with epidotic veins, associated with the quartz, near the place where it is penetrated by the trap.

29, 30. Granite without mica found near the quartz.

31. Disintegrated granite from a depth of four or five feet from below the soil, beginning to consolidate.

32. From immediately under the soil.

33. Nodular basalt from a trap vein.

34. Nodules of trap found loose on the surface of the ground.

VI. *On the Figure of the Cells of the Honeycomb.* *By* SAMUEL SHARPE, *Esq. F.G.S.*[*]

REAUMUR mentions, in his History of Insects, vol. v. p. 39, that he employed Maraldi to measure a cell of a bee's honeycomb, and Kœnig to calculate what the proportions of the three-sided pyramid at the end should be, in order that the whole cell might be made with the least materials; it being easily shown that the contents of the cell would be the same, whatever the height of the three-sided pyramid, even if = 0, in which case the prism would have a plain end.

Their reports nearly agreed; but the difference still is not to be overlooked, as will be seen in the angles of the parallelograms of the pyramid:

Maraldi's measurement	109° 28′ and	70° 32′
Kœnig's calculation......	109 26	70 34

And neither naturalists nor mathematicians have accounted for the difference.

As, from the nature of the materials, the angles can hardly be measured by reflection from the sides with a reflective goniometer, we have no better means of measuring than Maraldi had, and shall therefore consider his measurement to have been correct.

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[*] Communicated by the Author.

By the fluctional theorem *de maximis et minimis*, the calculated angles appear to be wrong, and should be 109° 28′ 16″, and 70° 31′ 44″; which must be held to agree with the measurement; as no one would pretend to make that more accurate than to the nearest minute, and the difference is much within those limits.

Kœnig's paper was read before the *Academie des Sciences* at Paris in 1739, and the results are mentioned in their Transactions for that year; but as his working is not added, I cannot compare mine with his to see where the error lies.

As it cannot be held unimportant to show that bees build their cells exactly in the form which, at length, by the differential calculus, we find to be best; and as I cannot expect my assertion to be preferred to that of Kœnig,—I add the working at length.

1st. As of those figures which can be brought together without leaving any interval, the hexagon is that which has the greatest number of sides, it is clearly the one which needs the least materials to inclose a given space.

2ndly. If a range of hexagonal cells be met by a range of similar cells, and no space be wasted, each cell must end either with a three-sided pyramid, or with a plane at right angles to the sides (*i. e.* a pyramid whose altitude = 0).

Query. What must be the altitude of the pyramid, in order that a cell of a given prism and given solid contents may be constructed of the smallest quantity of materials?

Let fig. 1. be any such regular three-sided pyramid on a six-sided prism.

Fig. 1.

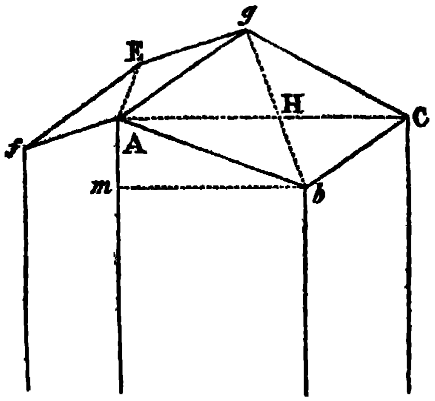


Fig. 2.

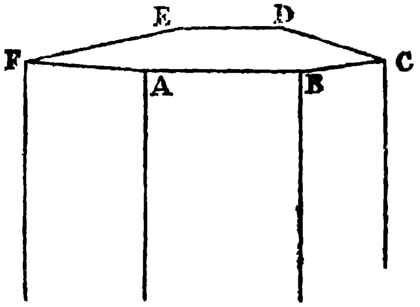
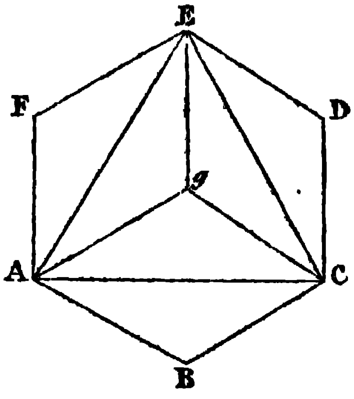


Fig. 2. the same prism with a plain end at right angles to the sides (*i. e.* the altitude of the pyramid = 0).

Fig. 3. the plain end of the above. Join the alternate angles ACE to the centre g, and to each other. Then if the centre g be raised and ACE remain fixed, the other angles (*solid* angles in the prism) BD and F will be lowered, and the solid contents of the cell will remain the same.

Fig. 3.



Again, by comparing fig. 1 and 2. we see that though the pyramid needs more materials than the plain termination, yet the sides of the former prism need less than those of the latter by the six little triangles Amb.

Now let AB ($= mb$) be.......................... a
Am (the altitude of the pyramid) ... x
AH (half of AC) .... d

$$Ab = \sqrt{a^2 + x^2}$$

$$Amb = \frac{ax}{2}$$

H$b = \sqrt{a^2 + x^2 - d^2}$, and the quantity which we wish to be a minimum is

3 (AgCb) − 6 (Amb) $= 6d\sqrt{a^2 + x^2 - d^2} - 3ax$ whose fluxion is $6d\,\overline{a^2 + x^2 - d^2}^{\,-\frac{1}{2}}\, x\dot{x} - 3a\dot{x} = 0$

$$2dx\,\overline{a^2 + x^2 - d^2}^{\,-\frac{1}{2}} = a$$

$$2dx = a\,\overline{a^2 + x^2 - d^2}^{\,\frac{1}{2}}$$

$$4d^2x^2 = a^4 + a^2x^2 - a^2d^2$$

$$x^2 = \frac{a^4 - a^2d^2}{4d^2 - a^2}$$

but $d^2 = a^2 - \qquad = \frac{3a^2}{4}$; therefore $x^2 = \frac{a^4 - \frac{3a^4}{4}}{3a^2 - a^2} = \frac{a^2}{8}$, and

$$x = \frac{\quad}{\sqrt{8}}$$

$$Ab \quad a\sqrt{\frac{9}{8}},$$

and Ab : AH : : radius : cos. HAb $= \frac{a\sqrt{\frac{3}{4}}}{a\sqrt{\frac{9}{8}}} = \sqrt{\frac{2}{3}} =$ cos

35° 15′ 51″·86, and the angle gAb = 70° 31′ 44″ nearly.
AgC = 109 28 16.

VII. *Chemical Examination of the Oxides of Manganese. By* EDWARD TURNER, *M.D. F.R.S. Ed. Professor of Chemistry in the University of London, and Fellow of the Royal College of Physicians of Edinburgh.*[*]

IT was originally my intention, in entering on this inquiry, merely to ascertain the composition of the ores, the mineralogical characters of which have been so ably delineated by Mr. Haidinger in the preceding paper[†]. I had advanced, however, but a short way in the investigation, when my progress was arrested by doubts both as to the manner of conducting the analyses, and as to the mode of calculating their results. In this uncertainty I found it necessary to extend my original plan, with the view of supplying by my own researches what appeared to be not sufficiently established by the labours of other chemists. I have accordingly divided the essay into two parts; attempting in the first division to ascertain the atomic weight of manganese, and the composition of the artificial oxides of that metal; and in the second, applying the facts thus established to illustrate the chemical constitution of the native oxides described by Mr. Haidinger.

PART I.

On the Atomic Weight of Manganese.—Analysis of the Carbonate of Manganese.

A pure carbonate of the protoxide of manganese was prepared in the following manner. The dark brown mass left in the process for procuring oxygen gas from the common peroxide of manganese by heat, was mixed with a sixth of its weight of powdered charcoal, and exposed to a white heat for half an hour. The protoxide thus formed was dissolved by muriatic acid, the solution evaporated to dryness, and the residue kept for some time in a state of fusion at a red heat. The resulting chloride of manganese was re-dissolved by distilled water; and after being filtered, was found to contain no impurity except a little lime, which was separated by the oxalate of potash. The manganese was then precipitated by a solution of the bicarbonate of potash, and the carbonate of manganese was carefully edulcorated and collected on a filter. After removing the upper layer, which had become rather brown by exposure to the air, the white carbonate was kept in a vacuum along with a vessel of sulphuric acid until it became

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[*] From the Transactions of the Royal Society of Edinburgh.

[†] On a future occasion we propose to give Mr. Haidinger's paper here alluded to, and also Dr. Turner's analyses of the ores which he has described.—EDIT.

came quite dry. The salt thus prepared yielded a colourless solution, without any residue, when put into dilute sulphuric acid, and was therefore free from the red oxide of manganese.

Of this carbonate 8·805 grains were heated to redness in a green glass tube, and the water collected in a tube filled with fragments of the chloride of calcium. The quantity of water procured in this way amounted to 0·742 of a grain, equivalent to 8·427 per cent.

The proportion of carbonic acid was estimated by noting the loss of weight which the carbonate of manganese experiences when dissolved in dilute sulphuric acid. This mode of analysis, as commonly performed, is inaccurate; because the liquid retains carbonic acid in solution, while the gas during effervescence carries off with it an appreciable quantity of watery vapour. But when performed with the precautions which I adopted, it yields uniform results, and is susceptible of great precision. A known quantity of the carbonate is placed in a small glass phial fitted with a tight cork, in which two tubes are inserted. One of these tubes descends to near the bottom of the phial and then bends slightly upwards, so as to admit of the acid being gradually introduced without affording an exit to the gas. The other communicates with a tube filled with chloride of calcium, over which all the carbonic acid gas passes before escaping into the air. As soon as the effervescence has ceased, the carbonic acid retained by the solution is driven off by causing it to boil during the space of a few minutes; and the gas is by the same means expelled from the interior of the phial, into which on cooling the atmospheric air is admitted by the tube for introducing the sulphuric acid. The carbonic acid gas remaining with the chloride of calcium is replaced by atmospheric air, which is introduced by inhaling at one extremity of the tube while the other is open. The upper part of the tube for introducing the dilute sulphuric acid, when not required to be open, is of course closed with a cork in order to avoid loss by evaporation.

It was found by means of the preceding process that 20·68 grains of the carbonate, when dissolved in dilute sulphuric acid, lose precisely 7·18 grains, or 34·72 per cent of carbonic acid. It is accordingly composed, in 100 parts, of

Protoxide of manganese	56·853
Carbonic acid	34·720
Water	8·427
	100·000

Regarding 22 as the equivalent of carbonic acid, we have the following proportions:—As 34·72 : 56·853 : : 22 : 36·024.

According to this analysis, 36 may be safely adopted as the combining proportion of the protoxide of manganese; and presuming the elements of this compound to be in the ratio of one equivalent of oxygen to one equivalent of metallic manganese, 28 will be the equivalent of the latter. This result, with respect to the acid and base, corresponds exactly with the analysis of Dr. Thomson, as mentioned in his First Principles of Chemistry, vol. ii. p. 350. It differs considerably from the proportions stated by Dr. Forchhammer. (Annals of Philosophy, N. S. vol. i. p. 54.) According to this chemist, 33·05 parts of carbonic acid combine with 51·755 parts of the protoxide of manganese, a proportion which would fix 34·45 instead of 36 as the equivalent of the protoxide. This estimate is certainly erroneous; and Dr. Forchhammer appears to have fallen into the mistake by supposing that the carbonate of manganese is converted by a red heat into the deutoxide, whereas according to my experiments the red oxide chiefly is then generated.

It appears doubtful whether the water found by analysis in the carbonate, after being dried *in vacuo* with sulphuric acid, is mechanically retained by it or is in a state of chemical union. As the proportion is not atomic, it is probable that the carbonate is really anhydrous. If the ratio were as 58 to 4·5 instead of 5·337, the salt might be regarded as a compound of two equivalents of the carbonate of manganese and one equivalent of water.

Composition of the Sulphate of Manganese.—The most recent analyses of the sulphate of manganese are by Dr. Forchhammer and Dr. Thomson, described in the works already quoted. Dr. Forchhammer precipitated the acid of a known quantity of the neutral sulphate of manganese by the nitrate of baryta, and inferred from the weight of the precipitate, that 100 parts of the sulphate of manganese are composed of 54·378 parts of sulphuric acid and 45·622 of the protoxide. According to this analysis, the atomic weight of the protoxide is 33·56, a number which is surely very far from the truth, and is inconsistent with the equivalent of that oxide derived from Dr. Forchhammer's own analysis of the carbonate.

Dr. Thomson analysed the sulphate of manganese by mixing that salt in atomic proportion with the muriate of baryta, and found, that, after the insoluble precipitate had subsided, no trace of sulphuric acid or baryta could be found in the solution. From this experiment he infers that 36 is the equivalent of the protoxide. I am of opinion that the number assigned by Dr. Thomson is correct, but I am not so certain that the means by which he arrived at this conclusion are altogether free from objection. The principle of his method is unexceptionable, especially if the quantity of the precipitated sulphate be carefully observed at the same time; but it is essential to accuracy that the atomic weight of baryta be perfectly established. Dr. Thomson supplied this element in the inquiry in the following manner. He dissolved 88 parts or one equivalent of sulphate of potash, and 106 parts, or what he considered one equivalent, of the chloride of barium in separate portions of distilled water, and then mixed the solutions together. After the precipitate had subsided, the supernatant liquid was found to contain no trace either of sulphuric acid or baryta. It hence follows, if no error is committed, that 70 is the true equivalent of barium. But in a recent number of Poggendorff's *Annalen der Physik und Chemie* (vol. viii. p. 5), Berzelius denies the accuracy of the experiment. He declares that after mixing together the sulphate of potash and chloride of barium in the proportions mentioned by Dr. Thomson, 2½ per cent of the chloride of barium remained in the residual liquid; and on repeating this experiment for my own information, I certainly found that the whole of the baryta was not precipitated. I wish it to be distinctly understood, however, that I do not confidently rely on the accuracy of my result, having been hitherto unable, from want of leisure, to examine the subject with that care which I deem necessary before attempting to decide a point in dispute between chemists, for whose analytical attainments I entertain such high respect. Dr. Thomson will doubtless feel the necessity of verifying his conclusions without delay; since an error in the atomic weight of barium will at once vitiate an extensive series of his most elaborate analyses. My own observation, however, combined with the remark of Berzelius, has induced me in the mean time to secure my own researches as much as possible from any uncertainty respecting the atomic weight of barium, and I have been therefore induced to ascertain the composition of the sulphate of manganese synthetically rather than by analysis.

Nine grains of pure protoxide of manganese, prepared from the red oxide by means of hydrogen gas, were dissolved in dilute sulphuric acid, the solution was slowly evaporated to perfect dryness in a platinum crucible, and the dry salt exposed for half an hour to a red heat. It then weighed 19·01 grains; and regarding the increase in weight as owing to the acid combined with the protoxide, the resulting sulphate must consist of 9 grains of the protoxide of manganese, and 10·01 grains of sulphuric acid. The atomic weight of the protoxide indicated by this process, is 35·96. The experiment was repeated with 4·855 grains, and the resulting sulphate weighed 10·26 grains, indicating 35·93 as the equivalent of the protoxide of manganese.

As some chemists may doubt the accuracy of this process, I shall attempt to show the grounds on which its merits are to be estimated. Dr. Thomson says it is scarcely possible to expel all the water from the sulphate by means of heat, without at the same time driving off some of its acid. It is indeed very easy to effect the decomposition alluded to by Dr. Thomson; but I found no difficulty, by slow evaporation and raising the fire gradually, to keep the salt at a red heat for an hour or longer without decomposing a particle of it. If the heat should accidentally become so intense as to decompose a little of the salt, the defect is easily remedied by adding a drop or two of acid, and replacing the crucible in the fire.

Dr. Forchhammer has judiciously remarked, that in expelling an excess of sulphuric acid, a portion of the salt is very apt to be carried off mechanically by the acid vapour and lost. This accident has occurred to myself, and always happens when a large quantity of free acid is rapidly expelled. By employing a slight excess of acid, and raising the heat slowly, all loss from this cause may easily be avoided.

The dry salt obtained in my experiments was white, and dissolved readily and completely in distilled water.

Like many other neutral metallic solutions it reddened delicate litmus paper. It was nevertheless quite neutral; for a single drop of a dilute solution of potash occasioned a precipitate which was not in the slightest degree redissolved by agitation.

Analysis of the Chloride of Manganese.—In an excellent paper published in the Philosophical Transactions for the year 1812, Dr. John Davy states the chloride of manganese to be composed of 54 parts of chlorine and 46 of metallic manganese. The atomic weight of manganese calculated from these data is 30·67, a number which is considerably beyond the truth. Dr. Davy prepared the chloride by heating the muriate in a glass tube communicating with the atmosphere by a very small aperture. I have never failed by this method to decompose some of the chloride, a circumstance which complicates the analysis, and probably gave rise to Dr. Davy's error.

According to the analysis of M. Arfwedson (Annals of Philosophy, N. S. vol. vii. p. 274), the elements of the chloride of manganese are in the ratio of 8403 parts of chlorine to 6677 of manganese. This result, in the accuracy of which M. Arfwedson does not place implicit confidence, would fix the equivalent of manganese at 28·61. He prepared the chloride by placing the carbonate of manganese in a spherical cavity blown in a barometer tube, transmitted over it a current of muriatic acid gas, and heated the carbonate by means of a spirit-lamp as soon as the atmospheric air was expelled from the tube. As it is difficult by this, as well as by Dr. Davy's process, to procure a perfectly pure chloride of manganese, I had recourse to the following method. A solution of the muriate of manganese was evaporated to dryness, the heat being carefully regulated so as not to decompose any of the salt, and the dry compound was placed in a spherical cavity in the middle of a barometer tube about six inches long. Muriatic acid gas was then transmitted through the tube, and heat applied by the flame of a spirit-lamp. The chloride entered into perfect fusion at a low red heat, and on cooling yielded a highly crystalline lamellated mass of a beautiful pink colour. Every trace of acid and moisture was expelled by heat; and while the tube was still hot, its extremities were closed by corks, so that the chloride might be weighed without attracting moisture from the air. In the sense above explained it was quite neutral. Of this chloride 12·47 grains were dissolved in distilled water, and formed a colourless solution without any residue. The muriatic acid was thrown down by the nitrate of silver, and yielded 28·42 grains of the fused chloride of silver, equivalent to 7·008 grains of chlorine. Consequently the chloride of manganese consists of

Manganese.............	5·462	28·06
Chlorine.................	7·008	36

It follows from the preceding researches, that 28 is the true atomic weight of metallic manganese, and 36 the equivalent of that oxide of manganese which forms definite compounds with acids, and which I regard as the real protoxide of the metal. It is consequently composed of 28 parts of manganese and 8 parts of oxygen. These numbers agree with the atomic weight of manganese as stated by Dr. Thomson, but not with that given by Berzelius, who fixes it at 28·463. This estimate is made from an analysis of M. Arfwedson, who finds that the deutoxide of manganese is composed of 100 parts of the metal and 42·16 parts of oxygen; but it will appear from the sequel of this paper that the real quantity of oxygen united with 100 parts of manganese to constitute the deutoxide is 42·857, and not 42·16 as Arfwedson supposes.

On the Protoxide of Manganese.—By this term I mean the salifiable base of manganese, the only oxide of the metal which appears to me capable of forming regular salts with acids. I am of opinion that in this compound manganese is in its lowest degree of oxidation. The existence of the sub-oxides described by Berzelius and Dr. John of Berlin, has never been satisfactorily demonstrated; and I have reason to suspect that one or other of them would in some of my experiments have been generated, did there exist any tendency to their formation.

The protoxide may be formed, as was shown by M. Berthier in the 20th volume of the *Annales de Chimie et de Physique*, by exposing the peroxide, deutoxide, or red oxide of manganese to the combined agency of charcoal and a white heat; and Dr. Forchhammer has in the Annals of Philosophy described an elegant method of preparing it by means of hydrogen gas at a red heat. Arfwedson has likewise had recourse to this method, and I have employed it very extensively during the course of the present investigation. The mode of performing the experiment is as follows. The material for yielding the protoxide was either the red oxide, deutoxide, or peroxide of manganese; and, occasionally, the carbonate was used. When it was wished to employ a red heat only, the material was placed in a small tray of platinum foil, which was introduced into a tube of green glass, through which the hydrogen gas was transmitted. The heat was applied by means of a pan of burning charcoal. To prevent the tube from bending while softened by the heat, two or three pieces of tobacco-pipe were tied to it longitudinally by means of iron-wire. But when it was wished to prepare the oxide at a very high temperature, the material was put into a small tube of porcelain, and then introduced into a gun-barrel which was exposed to a full white heat in a common wind-furnace. A supply of hydrogen gas was procured in the usual manner from zinc and dilute sulphuric acid; but before coming in contact with the oxide of manganese, it was purified by being transmitted through a strong solution of potash, and then dried by the chloride of calcium. At the close of the process, the protoxide was of course preserved in an atmosphere of hydrogen gas until it was quite cold.

The abstraction of oxygen commences at a temperature below that of redness; and when the peroxide is employed, it becomes red hot by the caloric evolved during the formation of water, considerably before the tray which supports it is rendered luminous by the heat of the fire. It appears nevertheless from all my experiments that a strong heat is requisite in order to convert all the red oxide into the protoxide. When the process is conducted at a low red heat, I uniformly found that on putting the product into dilute sulphuric acid, which instantly dissolved all the protoxide, a portion of the red oxide came into view. This affords a sure criterion of the operation being complete; for the pure protoxide dissolves without residue in dilute sulphuric acid, and yields with it a perfectly colourless solution. There seems to be no risk of decomposing the protoxide by the employment of a high temperature. I have exposed the recently prepared protoxide a second time to the action of hydrogen gas and a long continued bright red heat without the weight being changed in the slightest degree; and after exposure to the same gas and a full white heat for an hour, it dissolves in dilute sulphuric acid without the slightest effervescence.

The protoxide of manganese is described by Forchhammer as being of a beautiful light-green, and by Arfwedson as of a pistachio-green colour. I have seen specimens with a tint very near the pistachio-green, but these always contained an admixture of red oxide. The colour of the pure protoxide is very near the mountain-green.

With respect to the action of air, my observations differ from those of Forchhammer, who found that recently prepared protoxide attracted oxygen from the atmosphere before he could weigh it. The protoxide procured in my experiments is far more permanent. I exposed fifteen grains of recently prepared protoxide to the free action of the air during the space of nineteen days, when it was found to have undergone no change either in appearance or weight. If, therefore, it does attract oxygen at all from the air, the operation must proceed very tardily. It absorbs oxygen very slowly even at a temperature of 400° F.; for 7·269 grains of the protoxide, after an hour's exposure to that degree of heat, did not gain in weight more than 0·021 of a grain. At a temperature of 600° F. it absorbs oxygen much more rapidly; and at a low red heat it loses its green tint, and becomes almost black in an instant. I have repeated this process frequently, but in no case did the protoxide take fire, as occurred in the experiments of Forchhammer and Arfwedson. I entirely agree with M. Arfwedson, however, in the statement, that the protoxide is converted, by simultaneous exposure to heat and air, into the red oxide. This is the uniform result at whatever temperature the oxidation is effected.

I have already mentioned my opinion, that, of the oxides of manganese, the protoxide is the only one which forms definite compounds with acids. It unites readily with this class of bodies, without effervescence, producing with them the same salt which is formed when the same acids act on the carbonate of manganese. When it comes in contact with concentrated sulphuric acid, an intense heat is instantly evolved; and the same phænomenon is produced, though in a less degree, by strong muriatic acid. This oxide is likewise the base of the salts which are formed when sulphuric or muriatic acid is heated with the peroxide, deutoxide, or red oxide of manganese. As the accuracy of this statement, as respects sulphuric acid, has been denied by an acute chemist and good observer, I have been induced to examine the question with considerable care. I mentioned in my Elements of Chemistry, in explaining the process for procuring oxygen gas by means of sulphuric acid and the black oxide of manganese, that the peroxide loses a whole proportion of oxygen, and is converted into the protoxide, which unites with the acid, forming a sulphate of the protoxide of manganese. The gentleman who has done me the honour to review that work in the Annals of Philosophy, I apprehend Mr. Richard Phillips, has made the following remark on the preceding passage. "This statement is at variance with both Dr. Thomson's and also with the results of our experiments; for we find that 44 or one atom of peroxide of manganese yield 4·2 of oxygen, which is so much nearer 4 than 8, that there is no question but that the deutoxide, and not the protoxide is obtained by the action of sulphuric acid; that this is the case is further proved by the deep red colour of the solution of the sulphate, and by its losing that colour, as stated by Dr. Thomson, when mixed with sulphurous or nitrous acid."

To decide this point between the reviewer and myself, it is only necessary to heat the peroxide of manganese with concentrated sulphuric acid, so as to form a solution highly charged with the oxide of manganese, and decant off the solution while hot from the undecomposed peroxide. The liquid on cooling deposits a perfectly white salt, which possesses every property of the protosulphate of manganese. If the acid, which retains an amethyst-tint even when cold, be again heated, the red colour speedily disappears; because the red oxide, which is dissolved in small quantity by the sulphuric acid, is then also converted into the protoxide with the evolution of oxygen gas. The red colour disappears gradually even without the aid of heat; for the solution will be found after a few days to be almost and sometimes quite colourless, while a minute quantity of red oxide has subsided to the bottom. On applying a very gentle heat, the red oxide is redissolved, and the acid acquires a lively amethyst-red colour. It is easy, by operating in this way, to obtain satisfactory proof, that a minute portion of red oxide suffices to communicate a rich colour to a considerable quantity of sulphuric acid. The acid may be made to retain its red colour, either by diluting it with water, or by keeping it in contact with undissolved oxide.

On the Red Oxide.—I have followed the usage of most chemists in applying the term *red oxide* to that compound which Arfwedson has described under the name of *Oxidum manganoso-manganicum* (Annals of Philosophy, N. S. vol. vii. p. 267), and which is uniformly produced when the nitrate, peroxide, or deutoxide of manganese is exposed to a white heat. In my early experiments on this oxide, I entertained considerable doubt as to the uniformity of its composition. This opinion originated in the remark, that, on exposing the peroxide of manganese to a white heat, the quantity of oxygen lost by different portions of it, though agreeing perfectly in some experiments, differed widely in others; and that, on one occasion, I procured the green oxide almost in a state of purity. I subsequently discovered, however, that the disagreement in the results was occasioned by the want of a free current of air within the furnace. In some of the experiments the draft was unguardedly cut off, and consequently an atmosphere of carbonic oxide gas, collecting around the heated manganese, reduced it more or less nearly to the state of protoxide. On avoiding this source of fallacy, the results were no longer discordant; and I am now quite satisfied that the red oxide formed at a white heat and with free exposure to atmospheric air, is uniform in its composition. The accuracy of this inference is established by the occurrence of the red oxide in nature, as will appear in the sequel of the present communication.

The red oxide, when formed at a white heat and rubbed in a mortar to the same degree of fineness, is always of a brownish-red colour when cold, and nearly black while warm. The powder of the native red oxide has a reddish-brown tint, and the colour of the red oxide prepared by exposing the precipitated protoxide or the carbonate to a moderate red heat, has most commonly an admixture of yellow, something like rhubarb, though of a deeper hue; but both of these acquire the red colour when heated to whiteness.

The red oxide manifests little tendency to pass into a higher degree of oxidation by abstracting oxygen from the atmosphere, even by the aid of heat. Thus a portion of the red oxide, preserved for an hour at a low red heat, and freely exposed to the air at the same time, did not acquire any appreciable addition to its weight. The protoxide of manganese precipitated from the sulphate by an excess of pure potash, collected on a filter and washed, fully exposed to the air in its moist state for twenty-four hours, and then heated in an open vessel to a moderate red heat, which was insufficient to decompose the deutoxide, lost only 0·218 per cent by subsequent exposure to a white heat. The quantity of deutoxide present, therefore, must have been very minute. The anhydrous protoxide, as already mentioned, always yields the pure red oxide when heated to redness in the open air. The carbonate, also, in similar circumstances, is converted into a red oxide containing but a very small proportion of the deutoxide. It will appear from these experiments that it is unsafe in analyses to heat the precipitated protoxide or carbonate to redness, and consider the product as the deutoxide; a practice which is calculated to lead analytical chemists into considerable errors, and indeed has actually done so. If it is wished to procure the deutoxide, the precipitate should be moistened with nitric acid, and then exposed to heat.

I have endeavoured to ascertain the composition of the red oxide by several methods. The first is by the combined agency of heat and hydrogen gas. In the first experiments 100 parts of pure red oxide, in being thus converted into the protoxide, lost 6·802 and 6·817 parts of oxygen; but as the resulting green oxide, when put into dilute sulphuric acid, was found to contain a little red oxide, the loss in oxygen must be rather below the truth. To avoid this error I exposed 44·256 grains of red oxide to hydrogen gas and a white heat for the space of one hour, when the loss amounted to 3·153 grains on 7·125 per cent.

Judging by the increase in weight which the protoxide acquires when heated in the open air, 100 parts of the red oxide consist of 93·05 parts of protoxide and 6·95 of oxygen. According to a similar experiment made by Arfwedson, the red oxide is composed of 93·153 protoxide and 6·847 parts of oxygen.

In an analysis already described, the carbonate of manganese was found to contain 56·853 per cent of the protoxide of manganese. When 100 parts of the same carbonate are exposed to air and a white heat, 61·18 parts of red oxide are obtained. From these data it may easily be calculated that the red oxide consists of 92·927 parts of protoxide, and 7·073 of oxygen.

As a mean of the numbers afforded by these three methods, it follows that the red oxide is composed of 92·951 parts of the green oxide and 7·049 of oxygen, or of 72·291 parts of metallic manganese and 27·709 of oxygen. According to M. Berthier*, who reduced the red oxide to the metallic state by means of charcoal and a long continued intense heat, the oxygen is only 26·6 per cent. But this estimate, as M. Berthier himself suspects, certainly renders the quantity of oxygen too small; for though, guided by theoretical views, I am disposed to consider my own number not rigidly exact, yet from the care with which the experiments were made, I am satisfied their result cannot be far from the truth.

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* *Ann. de Chimie et de Physique*, tom. xx.

From this proportion of manganese and oxygen, we may consider the red oxide a compound either of 80 parts or two equivalents of the deutoxide and 36 or one equivalent of the protoxide, as M. Arfwedson supposes, or of 44 parts or one equivalent of the peroxide and 72 or two equivalents of the protoxide of manganese. If, on either of these suppositions, the composition of the red oxide in 100 parts be calculated, it will be found to consist of 93·104 parts of the protoxide and 6·896 of oxygen, or of 72·414 parts of metallic manganese and 27·586 of oxygen. These numbers approximate closely to those furnished by my experiments, and may serve perhaps to correct them.

The red oxide of manganese, when agitated with strong sulphuric acid, is dissolved in minute quantity, without appreciable disengagement of oxygen gas, and the solution is promoted by a slight increase of temperature. If the resulting liquid be separated from undissolved oxide, and exposed to heat, its amethyst-red tint quickly disappears, and the protosulphate of manganese is generated. When the red oxide is briskly heated with sulphuric acid, the protosulphate is formed, and oxygen gas evolved with effervescence.

On boiling the red oxide with an excess of very dilute sulphuric acid (in the proportion, for example, of two measured drachms of strong acid to five ounces of water), a colourless solution of the protosulphate is obtained; while a portion of peroxide is left, the quantity of which corresponds to the atomic view just given; that is, 116 parts of the red oxide yield 44 parts of the peroxide of manganese.

When the red oxide is mixed with strong muriatic acid, a portion of it is almost instantly dissolved, and communicates a deep red colour to the liquid. But the solution is not permanent. The odour of chlorine is perceptible from the beginning, even at a temperature of zero of Fahrenheit; the disengagement of that gas continues slowly, though without distinct effervescence, until in a few days the solution, if separated from undissolved oxide, becomes quite colourless. The red oxide dissolves in hot muriatic acid with effervescence, owing to the evolution of chlorine.

On the Deutoxide.—This oxide is prepared by exposing the nitrate or peroxide of manganese for a considerable time to a rather low red heat. I have found great difficulty in procuring it artificially in a pure state. After exposing the peroxide for an hour or longer to a moderate red heat, the residue frequently contains too much oxygen for constituting the deutoxide; and on augmenting the temperature slightly, the loss in oxygen is very apt to become excessive. The result is so much influenced by slight differences of temperature, that I do not feel confident in inferring the existence of the deutoxide from such researches. That there is such a compound, however, is demonstrated by its occurring in two different states in the mineral kingdom. My experiments as to its composition, as will afterwards appear, agree with the statement of Berzelius, Arfwedson, and Thomson. It is intermediate between the protoxide and peroxide, consisting of 28 parts or one equivalent of manganese, and 12 parts or one equivalent and a half of oxygen; or rather, to be consistent with the atomic theory, of two equivalents of the former to three of the latter. Its elements, it is obvious, are in such proportion, that it may be regarded as a compound of 44 parts or one equivalent of the peroxide, and 36 parts or one equivalent of the protoxide of manganese; and into these it may be resolved by being boiled in dilute sulphuric acid.

The colour of the deutoxide of manganese varies with the source from which it is derived. That which is procured by heat from the native peroxide or the hydrated deutoxide, has a brown tint; but when prepared from the nitrate of manganese it is almost as black as the peroxide itself, and the native deutoxide is of the same colour.

On heating a mixture of the deutoxide of manganese and concentrated sulphuric acid, oxygen gas is evolved with effervescence, and the protosulphate is generated. In the cold the acid acts upon it slowly, and acquires an amethyst-red colour; but this effect does not take place so readily as with the red oxide. The solution is attended with the disengagement of a little oxygen, a circumstance from which it may be inferred that a portion of deutoxide is resolved into oxygen and the red oxide, and that the latter, on being dissolved, is the cause of the red colour. Arfwedson represents the deutoxide as yielding a deep grass-green coloured solution with sulphuric acid; but I have never been able to observe this phænomenon.

Strong muriatic acid acts upon the deutoxide in the same manner as on the red oxide of manganese, excepting that the acid acquires the deep red tint more rapidly with the latter than when the former is employed. It is hence probable that the red colour is really communicated by the red oxide.

Peroxide of Manganese.—To procure a pure peroxide of manganese, a solution of the protonitrate was evaporated to dryness, and the heat continued until the whole of the salt was converted into a uniform black mass. It was then reduced to fine powder, carefully washed with distilled water, and dried by exposure for several hours to a temperature of 600° F. On heating a portion of this peroxide to redness in a glass tube, a little moisture was expelled, which reddened litmus paper powerfully. Consequently the peroxide still retained a little nitric or nitrous acid, which I found it impossible to expel entirely, except by the employment of a temperature bordering on a commencing red heat. The peroxide, after exposure to that degree of heat, was quite free from acid, but still retained a trace of moisture. On exposure to a white heat it lost only 10·82 per cent of oxygen, whereas had the peroxide been pure, it should have yielded 12·122 per cent. It appears therefore that the heat required to expel the last portions of the nitric acid, decomposes some of the oxide itself; and this circumstance induced me not to rely on the analysis of the artificial peroxide of manganese.

From my examination of the native peroxide of manganese, I conclude with all other chemists who have of late years studied the oxides of manganese, that it contains twice as much oxygen as the protoxide. It is accordingly composed of 28 parts or one equivalent of manganese, and 16 parts or two equivalents of oxygen; and in being converted by a white heat into the red oxide, it should yield 12·122 per cent of oxygen gas.

Sulphuric acid acts very feebly on the peroxide of manganese. At first I could observe no action at all; but on employing a considerable quantity of the oxide, and agitating the mixture frequently, the acid after an interval of two or three days acquired an amethyst-red tint, a minute quantity of oxygen gas being at the same time disengaged. The nature of the change which is produced when sulphuric acid is heated with the peroxide of manganese, has already been discussed.

Muriatic acid, as is well known, acts upon the peroxide of manganese at common temperatures, chlorine gas being disengaged with effervescence. If heat and an excess of acid be employed, a colourless muriate of the protoxide is procured; but in the cold, or if the oxide be in excess, in addition to the protomuriate, a deep red coloured solution is formed, similar to that already mentioned in the description of the red oxide.

[To be continued.]

VIII. *Ex-*

VIII. *Experiments on the Pressure of the Sea, at considerable Depths.* By Jacob Green, *M.D. Professor of Chemistry in Jefferson Medical College, Philadelphia, United States, North America.*[*]

AMONG the various expedients resorted to for the purpose of relieving the tedium and monotony of a sea-voyage, no one is more common during a calm, than to attach to a long line (the log) an empty bottle, well corked, and then to sink it many fathoms in the sea. In all such experiments it is well known, that the bottles upon being drawn up are either full or are partially filled with water. The manner in which the water gets into the bottle is in some instances perfectly obvious, but in others very perplexing, if not wholly inexplicable. Sometimes the cork, however well secured and sealed, is driven into the bottle, and when drawn up the vessel is of course found filled with water; and in such cases, what is a little surprising, the cork is often found occupying its original position in the neck of the vessel, being forced there no doubt by the expansion of the dense sea-water on being drawn near the surface. This seems to be proved by the cork often being in an *inverted* position. In the above experiment, and in some others to be mentioned presently, the bottle appears to be filled instantly; as the person who lowers the bottle down often feels a sudden increase of weight, somewhat similar to the sensation produced when a fish takes the hook on a dipsey line.

Sometimes the above experiment is varied by filling a vessel with fresh water, which on examination is found to be replaced by salt water; the cork remaining apparently undisturbed.

Sometimes when the previously empty bottle is only half-full of water, this when poured into a tumbler effervesces like water highly charged with carbonic acid gas. This is readily explained: for when the bottle descends it is full of air, and when the water enters, it will of course absorb the air; especially when the dense water itself expands as it is drawn towards the surface.

Sometimes the experiment is performed by first corking the bottle *tight*, and then tying over the cork a number of layers of linen dipped in a warm mixture of tar and wax; in fact, every device seems to have been tried to prevent the entrance of the water by the cork. In many of these cases, when the bottle is drawn up from a depth of 200 or 300 fathoms, it is found filled or nearly filled with water, the cork sound, and in its first situation, and the wax and tar unbroken. Two experiments are mentioned, in which vessels with air-tight glass stoppers were used. In one case the bottle was broken, and in the other some drops of water were found in it.

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[*] Communicated by the Author.

' How does the water find its way into the bottles? There are two opinions: One is, that it passes through the cork and all its coverings, in consequence of the vast pressure of superincumbent water, in the same manner as blocks of wood are penetrated by mercury in the pneumatic experiment of the mercurial shower. The other and less popular opinion is, that the water is forced through the pores of the glass*.

The following experiment, which I made on the 7th of May 1828, in latitude 48 + longitude 24° 34′, will perhaps throw some light on this subject.—Mr. Charles Dixey, the obliging and intelligent master of the packet-ship Algonquin, had a boat rowed off from the ship for me, to the distance of about half a mile, when the sea was almost perfectly calm. A hollow glass globe hermetically sealed, which I had previously prepared in Philadelphia, was then fastened to a line, and sunk, with a heavy mass of lead, to the depth of 230 fathoms, or 1380 feet. On the same line, and 30 fathoms above the glass globe, was fastened a small bottle with an air-tight glass stopper; 50 fathoms above this, a stout glass bottle with a long neck was tied; a good cork was previously driven into the mouth of this bottle, which was then sealed over with pitch, and a piece of linen dipped in melted pitch was placed over this; and when cool, another piece of linen treated in the same way was fastened over the first. Twenty fathoms above this bottle, another was attached to the line, much stouter, and corked and sealed like the first, except that it had but one covering of pitched sail-cloth. Thirty fathoms above this was a small thin bottle filled with fresh water closely corked; and 20 fathoms from this last there was a thin empty bottle corked tight and sealed, a sail-needle being passed through-and-through the cork, so as to project on either side of the neck.

Upon drawing in the line, thus furnished with its vessels, and which appeared to have sunk in a perpendicular direction, the following was the result:

The empty bottle with the sail-needle through the cork, and which came up the first, was about half full of water, and the cork and sealing as perfect as when it first entered the sea.

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* See Perkins on Pressure, Phil. Mag. vol. lvii. p. 54. J. Deuchar's Remarks on the same, Ibid. vol. lviii. p. 201. Campbell's Travels, 1st series, p. 335. Silliman's Journal, vol. xiv. p. 194. Deuchar's Mem. in the Trans. of the Wernerian Soc. 1821 —2—3.

The cork of the second bottle, which had been previously filled with *fresh* water, was loosened and a little raised, and the water was *brackish.*

The third bottle, which was sealed and covered with a single piece of sail-cloth, came up empty, and in all respects as it descended.

The fourth bottle, with a long neck, and the cork of which was secured with two layers of linen, was crushed to pieces, all except that part of the neck round which the line was tied; the neck of the bottle both above and below the place where the line was fastened had disappeared, and the intermediate portion remained embraced by the line. This I thought a little remarkable; and perhaps may be explained by supposing that the bottle was first filled by the superincumbent pressure with dense sea-water, which expanded on being drawn up near the surface. Had the vessel been broken by external pressure, that part surrounded with the line ought to have been crushed with the rest.

The fifth bottle, which had been made for the purpose of containing French perfumery or æther, and which was therefore furnished with a long close glass stopper, came up about one-fourth filled with water.

The hollow glass globe, hermetically sealed, which was the last and had been sunk the deepest of all, was found perfectly empty, not having suffered the smallest change. It is therefore concluded, that at the depth of 230 fathoms the water enters glass vessels through the stoppers and coverings which surround them, and not through the pores of the glass. What the effect of a pressure of 400 fathoms or more will have on the glass globe above mentioned, Captain Dixey has engaged to ascertain for me on his return to America, if opportunity shall offer.

IX. *Notices respecting New Books.*

A Geological Memoir on a Part of Western Sussex; with some Observations upon Chalk-Basins, the Weald-Denudation, and Outliers-by-protrusion. By P. I. Martin. London, 1828; 4to; pp. 100, and Synoptical Table; coloured Plates iii, and a Geological Map.

ON the occasion of reviewing Mr. Mantell's admirable "Illustrations of the Geology of Sussex," in the Philosophical Magazine for December last, we expressed a hope that the example of that gentleman, in carefully and minutely examining the strata and organic remains occurring in his own vicinity in Sussex, would be extensively and zealously followed, and that every important assemblage of strata in our island, might have its respective local inquirer. Since the publication of that review, the same means of augmenting our knowledge of the mineral structure of our country, and of promoting the advancement of geology in general, have been urgently recommended from the Chair of the Geological Society; in an Anniversary Address, as replete with comprehensive and correct ideas on the present state and requirements of the science, as it is with attempered views of geological theory, and the good-feeling to which the social pursuit of useful knowledge can never fail to give rise*. But we did not expect so soon to have the pleasure of directing our readers' attention to another highly interesting work of local geology, from which it appears, that, in one instance at least, our hopes of the benefits that might accrue from the plan of inquiry we ventured to suggest, have been fully justified. We observed, on the occasion alluded-to, that the "local inquirer," "bringing the general ascertained facts of the science to bear upon the peculiar phænomena of his own district, might obtain results reciprocally illustrating those general facts, with the same success that has attended the active labours of Mr. Mantell." Now this has been precisely the case with the researches of Mr. Martin, detailed in the work before us. For, having applied his general knowledge of the structure of the earth to the particular examination of a portion of the "Weald-denudation," he has become acquainted with facts, to a certain extent peculiarly observable in this district, which have led him to a *theory*—not, be it observed, an *hypothesis*,—of derangement and denudation, and of the origin of Chalk-basins, which will probably contribute materially to alter the views hitherto prevalent among geologists, of the history of the superior and supermedial strata in general. Nor is this result the less valuable, because ideas of a similar theory had been previously promulgated, without the author's knowledge, by two such accurate observers as Mr. Poulett Scrope and Professor Buckland. And there is yet another point of view in which the present Memoir becomes interesting; for since the research which led to its composition was made many months before either the delivery of Dr. Fitton's address to the Geological Society referred to above, or the publication of our own review of Mr. Mantell's "Illustrations," we may regard it as a proof that the importance of specific local investigations has already been appreciated and acted upon by geologists; and that we may confidently expect, at no distant period, memoirs of equal utility on other districts and formations.

Mr. Martin, willing, we apprehend, to enlist all classes of intellectual inquirers into the service of geology, prefaces his Memoir with an "Advertisement to the general reader." In this he briefly describes the Weald-denudation, illustrating his account by a profile of that remarkable tract, and also adverts to the interest of the stupendous convulsions which must have been concerned in producing the phænomena it exhibits; quoting in conclusion the subjoined apposite observations by the Father of inductive science: "Men use commonly to take a prospect of nature, as from a high turret; and to view her afar off; and are too much taken up with generalities. ' Whereas, if they would resolve to descend and approach nearer to particulars, and more exactly and considerately look into things themselves, there might be made a more true and profitable discovery and comprehension."

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* See Phil. Mag. and Annals, N. S. vol. iii. p. 299.

The Advertisement is succeeded by fourteen pages of "Introductory Remarks." It is here stated that the substance of the following Memoir was read before the Geological Society in March 1827; (See Proceedings of the Geological Society, No. 2; or Phil. Mag. N. S. vol. i. p. 388.) and that it then attracted some attention, among other causes, from its bringing again into discussion the arrangement and nomenclature of the beds immediately below the chalk. This subject is succinctly discussed in these Remarks, and, as it appears to us, in a luminous and satisfactory manner; the "upper shale" of the "Survey of the Yorkshire Coast" being regarded as identical with gault; and the whole suite of sands and clays between the chalk and the Weald-sands, &c. grouped together, in accordance with the observations of Messrs. Sedgwick and De la Beche, under the appellation of *glauconite.* For the upper member of this series, embracing every bed between the chalk and the gault, the name of *malm* is substituted, in place of the terms *firestone* and *upper green-sand,* successively employed by Dr. Fitton, and *malm-rock,* adopted by Mr. Murchison*.

The weald-clay and the iron- or Hastings-sand are regarded as deposits of common character and single origin,—as constituting one formation; and since Hastings-sand or iron-sand would be as little designative of the whole thing signified as Weald-clay, and to avoid the inconvenience of the periphrasis of Weald-sands and-clays, Mr. Martin proposes, as any compound from Weald must have a Saxon termination, to call the whole formation the *Wealden.* In the choice of this appellation, he observes, he has been "guided by a wish to do as little violence as possible to inveterate habit, as well as to adhere to the useful practice of deriving it from the locality in which the type is best exemplified."

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* We are not aware that either of the designations here proposed can be objected to; but at the same time we are unwilling to relinquish the term firestone; for, although it be very true, as Mr. Martin observes, that "any stone of a particular combination of lime, argil, and silex, may be a firestone," (*i. e.* a stone that will sustain a high temperature without alteration, and may thence be applied to such purposes in the arts as require a stone of this quality,) yet the term having been exclusively applied by English geologists to a species of stone occurring in the upper member of the glauconitic series, and derivatively to this subordinate formation itself, whatever its mineral nature, is not liable to misconstruction; and some circumstances in its history which Mr. M. has not alluded to, render us very desirous that its use should be retained by geologists. The beds of this substance in the neighbourhood of Godstone, in Surrey, and other localities, were extensively quarried by our ancestors, for the erection of many of their ecclesiastical and other edifices, as well as for the purposes of sculpture; examples of which may be seen in parts of Westminster Abbey, in the repaired southeast angle of the keep of Rochester Castle, &c.: and thence the name, *as applied*

The Introduction concludes with some remarks on the nature and difficulties of the research which led to the composition of the Memoir, and on the theory of the Weald which that research induced the author to form. It is here stated that down to the time of the memoir's being read before the Geological Society, "the writer was entirely unacquainted with the existence of any attempt to explain the act of denudation by any other agency than that of watery flood." His own speculations upon the structure of the country "had produced a conviction of some other agency besides that of water having been called into action, and that that agency had at the same moment of convulsion formed what are called the Chalk-basins of London and Hampshire, and in that act broken up what is now the cavity of the Weald." By an extension of his reading, however, he has found that Mr. Scrope, in his work on Volcanoes, published in 1825, promulgated a sketch of this theory of the Weald; and that, at about the same time, Prof. Buckland, in a paper read before the Geological Society in 1825, and published in the following year, gave a conjectural explanation of the same kind. If by these circumstances, Mr. Martin observes, "he has been deprived of the satisfaction of believing himself the first to promulgate this discovery, he has the pleasure of finding his opinions backed by high authority; and is emboldened to carry on the research to a satisfactory exposition of many of the facts which bear upon this theory, and are strong presumption, if they do not amount to a perfect proof, of its truth.".

The Introduction is succeeded by the "District Survey," illustrated by a map extracted from the Ordnance Survey, coloured geologically, and comprising that part of Sussex which extends from the Adur, between Steyning and West Grinstead, on the east; and the little stream (called for convenience the Lod,) which runs into the Rother by Lodsworth and Half-way-bridge, between the chalk-downs at Graffham and the foot of Blackdown, on the west. The chalk, malm, galt, Shanklin-sands, wealden, diluvium and alluvium of this district, are successively described, more or less at length, according to the novelty of the information to be imparted; but in all cases the descriptions are illustrated by tables of the mineral

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applied to this stone, has become familiar to antiquarian and topographical writers on the South of England in general, and also to architects. Thus this stone is emphatically denominated "Firestone" by Sir Christopher Wren, as may be seen in the '*Parentalia*.' We allude to these circumstances the more readily, as we perceive, from various remarks in his Memoir, that Mr. Martin is fully aware of the importance to the increase and unity of knowledge, of preserving in scientific disquisitions those popular appellations which have been from time immemorial bestowed on the objects of research; and of adopting in science the few *well-understood* denominations of the arts.

mineral and organic contents of each formation respectively. The two groups of the Shanklin-sands are described in a very discriminative and particular manner; and an extract of a letter from Dr. Fitton is annexed, stating his confirmed opinion that the tract of country in Sussex, consisting of these sands, is geólogically identical with that of the vicinity of Folkstone in Kent.

The "Wealden" includes "the Weald-clay, Hastings-sand, iron-sand, and Tilgate-beds of various geological writers."

In this section of the work, after describing the transition from the green-sand to the weald-clay, and stating that the escarpment of the former is every where prolonged by a considerable slope of the adjacent clay, Mr. Martin describes the beds assimilating to the Hastings-sand, which he has discovered in the Weald, in the following terms:

"Besides this slope, there is from the bottom of the green-sand a breadth of from a quarter to half a mile of clay, giving together an average depth, perhaps of 150 or 200 feet. At a moderate depth from the surface, this clay is generally blue, running into layers of hard blue shale, often impressed with the Cypris faba; and containing nodular concretions in concentric layers of ferruginous clay, sometimes containing calcareous matter. These argillo-calcareous Septaria are sometimes coated with a ferruginous concrete, containing casts of shells of the genera Cyclas, Cyrena, and Paludina, with the Cypris faba. At the top of this stratum, also, at its junction with the green-sand, in one locality, the author has discovered large bones of vertebrated animals* inclosed in a ferruginous concrete of sand and clay, but too imperfect to be appropriated.

"At the bottom of this first layer of the weald-clay, appears the first bed of sand, of which the thickness is uncertain. Sections have been made in it to the depth of twenty or thirty feet, and it is probably as much more. It is a brownish-yellow micaceous sand, abounding in white argillaceous veins, and with a coarse iron-rag†, sometimes containing casts of Cyclades, the teeth and scales of fishes, and vegetable impressions ‡, and in every respect assimilating to what has been hitherto considered the true Hastings-sand. The course of this bed of sand has been particularly attended to, and it has been traced through all the line of country here attempted to be described. From Mulsey (in the line of section of the Arundel and Bognor road), tracing it westward, it may be found at

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* "At Henfield between the village and the turnpike-gate, on the Horsham road, associated with Vivipara."

† "The sand is sometimes sufficiently indurated to form a building stone. And the iron-rag frequently becomes a conglomerate of sand and clay, with angular, and sometimes slightly rolled fragments of chert and sandstone. It has been extensively quarried in this part of the country, anciently, for smelting."

‡ "A specimen of Endogenites erosa from this course of sand, was found near Mulsey, and is now in the possession of Mr. Sowerby. It measures about ten inches in length, and six in circumference, and has lost a few inches at its apex. At the base, it is broader than that figured in Mantell's Tilgate Forest. The petrifying matter is silex."

Bignor-farm, under Bedham Hill, Buckfold, Parkhurst, Lickfold, and within half a mile of Blackdown, near Lurgershall, where there is a fine bank of it exposed at Northhurst-farm. Eastward it runs by Willets and Spear Hill, and crosses the Worthing road, at Wincaves; from whence its course is a good deal disturbed by the disruption of the Vale of Greenhurst; but it is found at Jessups, and again a little north of Henfield*."

The first course of the well-known concrete of fossil shells of Helix or Vivipara, called Sussex marble, is followed by another succession of clays, of much less depth than either of the former; and to this follows a second bed of sand more micaceous and of a deeper brown, with less admixture of clay, and carrying Horsham grit. Next succeeds the thickest and finest bed of Sussex marble: there are then beds of blue and red clay, and a third course of fawn-coloured micaceous sand.

" Immediately below this course of sand, a thick bed of red clay succeeds, and then the calcareous grit, sand-stone, and clays of what has generally been considered the 'forest range' of the weald.

" By this sketch of the contents of this part of the weald, it will be seen that there are two distinct beds of sand above the Sussex marble, the highest of which has all the character of the 'Hastings-sand,' and the second carries Horsham grit.

" There is therefore no line of demarcation to distinguish the weald from the Hastings-sands and -clays, except a slight elevation of the line of country, a more stony structure, and greater sterility."

To the " District Survey" succeeds the " Theory of Derangement and Denudation," which is certainly the most important part of the work. But we find it difficult to convey an adequate idea of this theory by extracts. Those which we subjoin, however, will impart some idea of it.

After stating that the Weald-denudation is understood to be bounded on each side by what are called the chalk-basins of London and Hampshire, the author observes, that, although the contents of these basins have been carefully examined and described, no satisfactory explanation of the mechanism or mode of formation of the basins themselves has yet been given.—He then proceeds to give his own theory, and partly in the following manner:

" *The strata which compose these basins, then, previously in a horizontal position, suffered disruption; and in the act of basining (whether by the elevation of the sides, or the subsidence of the central parts, is not now material), all their parts were deeply and extensively fissured, in an order correspondent with that act;—producing, with the help of diluvian action, a system of longitudinal and transverse valleys answering to the double inclination (the dip and lateral bearings, or obliquity of the plane) of their fractured masses, and a consequent removal of the broken materials brought within the range of the denuding force.*

" The effect of raising from the horizontal position, or in any other way stretching, a ponderous and frangible body, is to produce a division of its parts in such order and direction as its varying strength and tenacity dictates; the fractured parts taking their places, according to their magnitude or gravity, or the disposition of those which support them. This irregular fracture, alternate elevation and subsidence, and settling of parts thus disturbed, are well exemplified in the familiar operation of the heaving of the spade in digging. If the earth be tenacious and the action steady, it tears with such a divergence of the principal rents as will be here described; and the more friable parts are seen dropping in in such a way, and in such proportion, as the moving power dictates, and their structure allows. If another illustration were necessary, it might be found in what we observe in the elevation and cracking of the flour which covers the fermenting nucleus in a baker's trough.

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* " The irregularity of the denudation between Warminghurst and Henfield, seems to have left outliers of this sand, as well as of the upper bed of weald-clay, out of the usual course in that line of country."

"Where opportunities occur for tracing these appearances in the weald, they are found to be in perfect accordance with this theory. These opportunities present themselves frequently in the clay districts; but they are more distinctly traceable in the stony, in the dip and variable lateral bearings of the several masses. It may be safely said, that undulation, at the time of deposit, had very little share in these phænomena, or in the construction of the hills and valleys of these districts. Every variation in dip or lateral bearing has its commencement in a fracture, or if the displacement be moderate, such a contortion as would be produced by the gravitation of bodies like these, moving under great superincumbent and lateral pressure.

"The general dip of the chalk-hills is southward, but the lateral bearings of the several masses are variable*, and the inclination also different in different localities. Next to these succeed the more broken strata of the green-sand; and lastly, the smaller lacerations of the wealden formation, the widely spreading transverse fissures of which concur to produce the effect of longitudinal ones over all the convexity of the 'forest ridge.' But it is not to be understood, that there is any difference in the general disposition of all these parts. The convexity extends from the bottom of the English Channel to the bottom of what is called the London basin. This convexity may be likened to a dome, and the loss of a part of the crown of the dome is the weald vacuity. In other words, the commencement of each basin is in the anticlinal line of the weald valley. From this point, all the strata begin to slope. Both basins, therefore, may be said to be entire in a part of the wealden, although they have lost a part of their rims in the chalk and the glauconite.

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* "In ordinary geological language, there is not sufficient precision used in these terms; and lateral bearings are confounded with the dip, which should express the backward and downward inclination only. Thus, of the strata in question, sometimes we are told that they dip S.E., sometimes S.W. The fact is, that they all *dip* to the south, generally speaking, but the obliquity of the plane, or *lateral bearing* of the different masses, is sometimes east and sometimes west."

"The principal transverse fissures, some of which were destined to become river-channels, have a remarkable correspondence on each side of the valley, particularly in the chalk, and in several instances are directly opposed to each other; which could not have happened without a simultaneous action and common consent and continuity of parts. The direction of a rent would be ruled by the density and tenacity of the different parts of the stratum; occasionally deviating from the straight line, it might be lost in one part, and taken up and carried on by another, giving less resistance. The coincidence is therefore the more remarkable, and proves not only the continuity of the chalk strata at the moment of convulsion, but also their uniform density and strength. Of the valleys thus opposed to each other by the continuity of the greater transverse fissures, the most remarkable are,—the defiles of the Arun in the South, and the Wey in the North Downs; the vale of Leatherhead, or the Mole, and that of Findon, or the Worthing road; of the Adur and Smitham-bottom, or the pass of Merstham and Croydon. The Ouse is also opposed to the Darent, and the Cuckmere to the Medway.

"But it must not be supposed that this simple transverse fissure is the only appearance of displacement exhibited by the chalk strata. They have suffered in common with the other rocky strata, that adjustment of parts which might be expected from the nature of the material on which they rest. If the fissile character of the stony strata determine their division in straight and broad lines, it would also be the character of the wealden formation to be torn and contorted in a manner widely different. For a stratum of clay of great thickness, carrying stone barely sufficient to give it stability, would tear rather than split in the act of displacement; and such a divergence of the fissures as might be expected in so tenacious a mass, can be readily traced in every part of the weald surface. This divergence and laceration has therefore modified the disposition of the stony strata still superincumbent upon the clay; and the subsidence, elevation, and contortion consequent thereon, are everywhere visible in the dip and variable bearings of all their masses. In appreciating the evidence of these acts of laceration and fracture, it must not be forgotten also, that strata of various structure far below these under review, have suffered the same disruption; and by the variable nature of their fracturings have operated to modify, and in some cases to obscure, a great part of the direct testimony of the order here described. The pressure of superincumbent strata of unknown thickness and kind must also be taken into account; and it will not be thought wonderful that, diluvian action and the operation of more modern causes apart, there should be so little direct evidence of fissure upon the surface.

"The divergence or distribution of the transverse fissures is to be traced in the upper stony strata, as well as in the minuter divisions of the clay districts. If a fissure of this sort be followed from its termination in any of the hilly counties, it will be found to present, first a little coomb, into which the stone inclines on both sides, and at first carrying no water. By-and-bye springs burst forth, (the natural consequence of this convergence of layers of stone,) and it becomes a valley with a rivulet; both the valley and the rivulet enlarge by the accession of other coombs and rivulets, the effects of the branches of one greater river fissure; and the eye of the observer may, from any considerable eminence, embrace the courses of these valleys, as they converge towards some of the river outlets*."

The theory is satisfactorily illustrated from the phænomena observable in the valley of the river Arun, and in the vale of Greenhurst. A few pages of concluding observations present some acute criticism on the reasoning of the French naturalists on the æra of the formation of chalk-basins, &c.: and here occurs a remark on the geological site of the Thames Tunnel, which, as this analysis has occupied more space than was originally intended, we shall offer a reply to in our next Number.

The last section of the memoir, "On Outliers-by-protrusion," we give entire; and also, as a summary of the facts relating to the geological structure of the weald, we copy Mr. Martin's Synoptical Table.—Our opinion of the merits of the work is sufficiently evinced by our remarks in the commencement of this article, and the full account we have given of its contents, without the necessity of expatiating further on the subject.

"*On Outliers by Protrusion.*—In reviewing the outskirts of the district made the object of the 'Survey' of the foregoing Essay, the writer's notice was drawn to a remarkable chalk-eminence upon the coast, between Worthing and Little Hampton, called *High-Down.*

"High-Down stands considerably in advance of the general line of chalk-hills, and is insulated by a breadth, between them, of two miles of plastic clay, covered by the woods of Patching, Clapham, and Castle-Goring. It has, therefore, the character of an outlier; and upon a closer inspection, was found to present an escarpment to the sea, from which it is about two miles distant, and to have a northerly dip,—in opposition to the general inclination of the South Downs. The observation of this phænomenon immediately suggested the thought, that the well-known outlier of *Ports-Down*, in Hampshire, would be found to be in the same predicament. And as there is nothing to be observed of High-Down, which will not apply to the larger outlier above-mentioned, a description of the latter will suffice†.

"Ports-Down is a long narrow ridge of chalk, running east and west, which, rising three or four hundred feet above the level of the sea, overlooks the Island of Portsea, and the several islets and inlets of its celebrated harbour. To these, and to the south, it presents an abrupt escarpment; on the other side, it slopes rapidly northward, and is quickly lost, under the plastic clay and sands of the 'Forest of Bere;' of which there is about five or six miles, between it and the main body of the chalk of the South Downs, with which it forms a trough or basin. In advance, and at more than double the distance, are the chalk-hills of the Isle of Wight. It therefore distinctly belongs to, and is an emanation from, the north side of the Hampshire basin.

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* "It will not be supposed that the author means that this should apply to the courses of all rivers, and in all their parts. He is now talking of the Arun and the Adur, and not of the greater rivers of the globe. But the principle is capable of extension to the tributary streams of these also; and all perennial rivers must have their sources in disruption,—for disruption and displacement are the essence of springs and fountains."

† "The reader may take the Ordnance Survey, or any other good map which marks the high grounds, for a guide."

"At the eastern extremity, the chalk rises out of the plastic clay at Bedhampton, and, taking a direct course westerly, gradually attains its greatest elevation between Cosham and Portchester; it then gradually declines, and, after a course of about seven miles, sinks again under the plastic clay, at Fareham. Here it ceases to be an outlier, but the line of elevation can be traced further westward, and the chalk is sometimes quarried through the overlying strata, beyond Fareham, and comes to the surface again, at Titchfield and Funtley; beyond which, this research has not been carried.

"At the eastern extremity of the ridge, where it emerges, at Bedhampton, it dips gently northward, with a lateral bearing to the *east*. In the centre the inclination is much greater. There is no good section where it appears to be the greatest, but where the hill is intersected by the Portsmouth road, it amounts to twenty-five or thirty degrees, and at the highest, is perhaps nearly forty. Westward from thence, it is again diminished, and in the chalk-pits near Fareham, is found not only to have lost all its northward dip, but also to have returned to the general southward declination of the Hampshire Downs, dipping five degrees in that direction, with a lateral bearing *westward*. In inclination, therefore, it traverses an arc of forty-five degrees, in the space of about four miles. To answer to this contortion, a course of fissures may be traced, and these are particularly well displayed in the chalk-pit, close to Fareham, where, as the outlier sinks under the plastic clay, they have not been exposed to obliteration by denuding causes. Through a gap begun by some of these fissures, or a larger disruption of the whole mass, the rivulet passes, which takes its course transversely to the chalk, into the Fareham inlet; for west of it, the general bearing of the ground, by Upland-House, and on towards Funtley, is indicative of a resumption of the northerly dip.

"A moment's consideration will show, that such an outlier as this is very differently circumstanced from the more common one, or that which is entirely detached from the main body; separated by the removal of the intervening parts, and therefore properly the *outlier of erosion*. This, although it is an outlier in reference to the surrounding parts upon the surface, is still connected with, or incumbent upon, or in juxtaposition with, the kindred stratum below; and may therefore be called an *outlier-by-protrusion*.

[*Concluded on* p. 50.]

SYNOPTICAL TABLE.

FORMATIONS.	SUBDIVISIONS.	NATURAL CHARACTERS.	LOCALITIES.
A. CHALK.	1 Upper Chalk.	Soft, with flint nodules.	South Downs.
	2 Lower Chalk.	Hard, without flint.	
	1 Malm.	Blue-chalk-marl above, graduating into indurated, argillaceous limestone, or malm-rock; with subordinate beds of *green-sand*.	Sutton, Bury, Amberley, Washington, Steyning—the high grounds immediately under the chalk-hills.
		Containing Ammonites, Pectines, Inocerami, &c.	
		Soil, moist and fertile.	
	2 Galt.	Upper part,—harsh and shaley brown clay, mottled blueish;—	Bottom of Sutton and Bury hollow-ways, Washington, Rowdell, Fulking, &c.
		Lower beds,—blue and black shale, and stiff clay;	Bignor Park, Bury Common, New Woods, Hardham.
		sometimes containing ferruginous concrete;—and marl, as at	Wiggonholt*.
		Ammonites,—other fossils said to have been found in wells.	
		Soil, good for pasture ground, and for the growth of oak and elm.	
B. GLAUCONITE.	*Ferruginous Sand:*	Coarse red-sand, with layers of ironstone, compact and cellular.	Burton, Redhill near Bignor, Sandgate, Sullington, Pulborough, Fittleworth, Henfield.
		White, yellow, brown, and black sand with quartz grains, and occasionally green particles;—with	Graffham Common, Cold-Waltham, West-Chiltington Common, Sullington, Wiston-Parsonage, &c.
		Veins of ochreous clay, and subordinate beds of black shale and clay.	
		Terebratulæ and other fossils abundant at.........	Parham, Sparright Lodge, Pulborough, West Chiltington Common.
		Soil, for the most part arid and barren.	
	3 Shanklin Sands. *Lower Green-sand.*	Sand, full of green particles, alternating with courses and large concretional masses of blue calcareous grit.	West-Chiltington, Thakeham, Henfield, Ashurst, Pulborough, Bognor (in Fittleworth parish), Tillington, &c.
		Green sand-rock or Pulborough stone.	
		The same, siliceous, and passing into chert.	Pulborough Quarries, Pitt's Hill, Bognor Quarries, &c.
		Containing Ammonites, Pectines, Myæ, Pholadomyæ, &c.	Bedham Hill, Brinkshole, Petworth.
		Soil, moist and fertile.	

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* Since the Memoir was sent to press, the writer has found marl in the galt at Wiggonholt.

FORMATIONS.	SUBDIVISIONS.	NATURAL CHARACTERS.	LOCALITIES.
C. WEALDEN.		Stiff clay, brown at the surface, blue and shaley below; containing concretional iron-stone with *bones of vertebrated animals*, casts of Paludinæ, Cyclades, bones of Fish, Cypris faba, &c.	Broadmere Common, Henfield, Wappingthorn Hill, Ashington, Greenhurst;—Range of country below the escarpment of the glauconite.
	1st Course of Wealden-Sand.	Yellow and fawn-coloured sand, with beds of red clay, and iron-rag-conglomerate of sandstone and chert. Endogenites, and other vegetable remains.	Mulsey, Spear Hill, Wincaves, Buckfold, Parkhurst, Northhurst, near Lurgershall.
	1st Course of Sussex-Marble.	Clay, with interrupted course of Sussex-marble*. Sometimes iron-sand, blue-marl, and selenite. Bones, Vivipara, Cypris faba, &c.	Pococks, Hungar Hill, Battlehurst, Marshalls, &c.
	2d Course of Wealden-Sand.	Micaceous fawn-coloured sand, with beds of red clay and concretional masses of calcareous grit, and flag-stones of the same; occasionally a friable sandstone, with quartz pebbles.	Andrew's Hill, Butterstalks, Knep Castle, Lowfold Sand-Coppice Eberknoll, &c.
	2d Course of Sussex-Marble.	Blue and brown clay, containing a course of Sussex-marble, more interrupted than the former—with Vivipara, Cyprides, unnamed bivalve (perhaps Unio?) &c.	Slaughter Farm, Kingsfold, Steers Common by Kirdford, Plaistow.
	3d Course of Wealden-Sand.	Micaceous fawn-coloured sand, alternating with beds of red clay, friable sandstone, and calcareous grit. *Bones of vertebrated animals*, &c. Soil, a clay-country of great uniformity of aspect, and of moderate fertility only, best for oak-timber.	Parbrook Hill, Billingshurst, Sprenks and Brick-kilns in Shipley, Wisborough Green, and Kirdford.

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* Sussex-marble is a well-known shell-limestone (chiefly of the genus Vivipara), supposed to be fluviatile.

"The relations of the two specimens of Ports-Down and High-Down are exceedingly simple, and their history extremely well displayed. The opposing dip, contortion, and form of both eminences, are satisfactory proof of their protrusion from the parent stratum below;—whether effected by a separate and distinct propelling impulse, or simply *by arrest, by the interposition of some opposing substance, during the subsidence of the main body*, it is not now material to inquire. The mass thus detached and elevated, a part of which forms Ports-Down, must have been covered, as some portion of it still is, by the overlying strata, and is an outlier only where its highest parts have been exposed to the denuding forces.

"No line of country exists, indicative of any connexion between Ports-Down and High-Down, which are twenty-four miles asunder.

"It belongs to the general history of chalk-basins, and would pass the limits prescribed to this research, to endeavour to show which of the actions above referred to, have produced these outliers. Either way, they tend powerfully to illustrate the theoretical parts of the foregoing Essay, and to prove the disruption and displacement of the chalk,—the act of basining,—posterior to the deposit of the higher strata.

"The Isle of Thanet is probably an outlier of this description, and the writer is informed that Windsor stands upon chalk, which must therefore be also an outlier-by-protrusion, and perhaps so also the chalk islands in the Paris basin.

"It will be easily seen, that it is not necessary that such outliers should have a dip and an inclination opposite to the main body. In the elevation of the former, or the subsidence of the latter, they may both preserve the same parallelism, or nearly similar relations to the horizontal line. It is enough, if it be proved, that they are out of the direct line with each other below; and that the detached portions have been exposed to the flood of denudation upon the surface, and are outliers where they have been elevated within the range of the denuding force." [B.]

Elements of Chemistry. By ANDREW FYFE, M D. F.R.S.E. &c. 1827.

IN the Philosophical Magazine for November last we offered some observations on Dr. Fyfe's "Manual of Chemistry," without any anticipation that it would so soon fall to our lot to notice two octavo volumes on the same subject by the same author. We presume that the success which has attended Dr. Fyfe's first work, has stimulated him to fresh exertion; and we have with no slight attention examined nearly the whole of the work now under consideration, in the hope of being able to give a more favourable opinion of it, than that to which we conceived the Manual was entitled.

We shall not occupy either our own time or that of the reader with a minute examination of the arrangement adopted in the present work: it is materially altered from that of the Manual; and we do not perceive that the change is accompanied with the improvement, for which there was so ample room. The Elements commence with the subject of Heat, while in the Manual, and in our opinion with better judgement, Attraction was first treated of. Dr. Fyfe prefers the term *caloric* to that of *heat*, observing that "by heat we are to understand the sensation produced by a warm body; by caloric, the active cause of this sensation." Having made this distinction between cause and effect, our author proceeds to state, that "different bodies have different quantities [of caloric], and on this depends their temperature.' This is an error of very considerable importance; and if one mistake could be compensated for by another, Dr. F. has done all that can be required of him, by asserting that "our sensations give us no indication whatever" of temperature; *ergo*, our sensations indicate no difference between a cold bath and a warm one.

When treating of Pyrometers, Dr. F. has given a description of Wedgwood's instrument, and on many occasions he refers to it as a standard for determining the fusing-point of bodies; and yet he admits the fact, ascertained we believe by Sir James Hall, that this pyrometer is liable to one great objection, viz. that "if the clay be exposed to a moderate heat for a long time, it will contract nearly as much as if heated intensely." Being aware of this circumstance, all notice of the instrument ought surely to have been omitted, for it is worse than useless to quote that as authority which we do not believe to be true.

Under the head of Cohesive Attraction, some attention is naturally bestowed by our author upon the subject of Crystallization. But the little which he has said respecting it is so inaccurate and incomplete, that it would have been better altogether omitted. No mention whatever is made of a goniometer of any sort; and in treating of Salts in the various parts of the work, their crystalline forms are very erroneously described. Thus with respect to carbonate of soda, it is stated that it crystallizes in octohedrons composed of two four-sided pyramids joined by their bases; and Alum is correctly described as possessing the octohedral form. We may therefore conclude that in Dr. Fyfe's opinion there is no difference between the crystals of these very different salts. The fact however is, that, although the *apparent* form of carbonate of soda is an octohedron, yet it has a rhombic base and it is elongated, whereas the octohedron of alum is a regular one. We have said the *apparent* form is that of an octohedron, but this is not the real form of the crystal; Mr. Brooke has shown that it is an oblique rhombic prism. Various other salts are very incorrectly described, but we have not room for further observations on this subject.

We are told by Dr. Fyfe, when treating of Acidifying and Alkalifying principles, that the merit of the discovery of oxygen is due to Priestley, Lavoisier, and Scheele, the priority of it, only, being assigned to Priestley. It is however quite clear from Dr. Priestley's statement, (Doctrine of Phlogiston Established, p. 88. 1800.) that Lavoisier knew nothing whatever of the existence of oxygen, until Dr. Priestley mentioned it to him during a visit to Paris. On this subject there is also another error, which we should not have expected in the work of a medico-chemical writer. Dr. Fyfe says that Dr. Priestley procured oxygen gas from red precipitate, whereas he particularly mentions that he obtained it by employing *mercurius calcinatus per se*: this circumstance he on one occasion especially notices, observing that no nitric acid had been used in preparing it, and consequently that the oxygen could not be derived from the decomposition of nitric acid, a portion of which is well known sometimes to remain in red precipitate.

No directions are given for preparing oxygen gas in the section which treats of its properties. We are merely told that recourse is to be had to the decomposition of its compounds when we wish to prepare it; and in defiance of all propriety, and neglect of all convenience, we are directed to *see* Manganese, Red Oxide of Lead, Red Oxide of Mercury, and Chlorate of Potassa,—all of which are treated of in the second volume, while the properties of oxygen are described in the first.

With respect to Hydrogen Gas no advice is given to the young experimenter to allow it to escape for a short time previously to inflaming it, or to cover the vessel from which the gas is evolving, in order to prevent the ill effects which might arise from an explosion; and as to the means by which hydrogen gas is obtained, the omissions are, if possible, more glaring than those noticed as to the preparation of oxygen gas. Not only are there no directions given for this purpose, but we are not even told that they are to be found in a subsequent part of the work. In one experiment with this gas mention is indeed made of "a mixture which will furnish hydrogen;" but as to its composition, no more is said than if it were a profound secret and so intended to remain. It is indeed true that some hints are given as to the preparation of hydrogen gas when Water is treated of, but they are by no means so ample as they ought to have been.

In the observations prefixed to the section on Acids, Dr. F. remarks, "though Davy considered chlorine and iodine as acidifying principles, yet others maintained that hydrogen is the principle of acidity in those [acids] not containing oxygen. From the arrangement I have adopted, it will be perceived, that if we are really to attach a principle of acidity to certain bodies, it should be given to hydrogen, because we find that sulphur, chlorine and iodine, unite with oxygen, and form one set of acids, and with hydrogen to generate another." Now without pretending to feel the force of this reasoning, which we do not understand, there are sufficient facts stated in the quotation to induce us to conclude that no acidifying principle whatever exists, especially since oxygen and hydrogen both enter into the composition of several alkalies. Indeed, to admit of the existence either of an acidifying or an alkalifying principle, is in our estimation no more required than to allow of a principle of form or of colour.

We have neither time nor inclination to present the reader with all the observations which have occurred to us while perusing the section on the Metals. There are, however, some statements respecting a few of them which call for remark, and more especially as to iron and its compounds. Rust of iron is stated by Dr. Fyfe to be a carbonate: but this is not the case; for no solid compound of carbonic acid and peroxide of iron can be formed, and yet similar assertions occur twice in subsequent parts of this section. "The only compounds of any interest of iron and the simple acidifiable bodies are those with carbon, sulphur and cyanogen :" this last, however, we need hardly state, is a compound of two simple acidifiable bodies. The protosulphuret of iron is rather a scarce substance ; and yet Dr. F. states that it is employed in the preparation of the sulphate of iron by exposure to air and moisture :—now it is the persulphuret, which is a very plentiful mineral, that is used for this purpose.

Respecting the *liquor ferri alkalini* of the London Pharmacopœia Dr. Fyfe has committed several mistakes. In the first place " It is" *not* " much used ;" secondly, the solution of carbonate of potash must *not* be added to the solution of pernitrate of iron, but the latter to the former. The following remarks show indeed that Dr. F. is equally ignorant of the nature of the process, and of the product obtained by it. "In the first part of the process, a pernitrate of iron, but with a large excess of acid, is formed ; and in the second, this excess is merely saturated by the alcaline solution, a part of the iron being precipitated in the state of a carbonate, but which is instantly dissolved by the nitric acid ; so that the product is a mixture of nitrates of iron and of potass." Now as the acid solution is added to the alkaline one, instead of the reverse as here stated, the precipitate formed is *not* dissolved by the nitric acid, but by the carbonate of potash ; so that the product is *not* a mixture of nitrates of iron and potash, but a solution of carbonate of potash holding peroxide of iron in combination, and mixed with nitrate of potash. That the solution contains no nitrate of iron might almost have been learned from the name, absurd as it is, which the London College have given to this preparation. It is sufficiently ridiculous to speak of alkaline iron ; but it would have been worse than this to have bestowed a name denoting alkalinity, where none was in existence.

The *sulphas ferri exsiccatus* of the Edinburgh Pharmacopœia is *not* deprived of the whole of the water of crystallization ; it retains one atom of water. According to Dr. Fyfe, colcothar is used " for making razor-strops :" we supposed they had been made of wood and leather, and merely covered with colcothar. When the carbonated alkalies are employed to decompose sulphate of iron, the protocarbonate is *not* converted into percarbonate by absorbing oxygen, no such compound exists ;—it becomes a mixture of protocarbonate and peroxide of iron. " Permuriate [of iron] with the maximum oxid" *cannot* " be prepared by dissolving that oxide or the carbonate in the acid :" it must be " that oxide," for the carbonate contains protoxide : by the bye [illegible] *permuriate with the maximum oxide* is so particularly mentioned, may [illegible] inquire whether there is any permuriate *without* it ? In the London Pharmacopœia it is *not* the red oxide of iron which is used for preparing the *tinctura ferri muriatis*, but the *ferri subcarbonas*, which is a mixture of protocarbonate and peroxide : the Edinburgh process for the same preparation does *not* at first yield a protomuriate, for the scales of iron employed are a mixture, perhaps a compound, of protoxide and peroxide of iron.—There are several other statements respecting the compounds of iron which we are precluded from noticing merely for want of room, for we have still a few observations to make with regard to some other metallic compounds.

Copper, according to Dr. Fyfe, is not an abundant production, and yet the county of Cornwall alone produces nearly ten thousand tons annually. Sulphate of copper according to our author is "always" procured by exposing the natural ore containing sulphur and copper to the air. We venture to assert that this salt is never now so prepared, nor do we believe that it ever was. Is Dr. Fyfe aware of the fact, that with few exceptions, the ore of copper is a double sulphuret of that metal and iron ? And if a salt were formed from it by the action of air and moisture, it would undoubtedly be the double sulphate of copper and iron, which is a well known compound.

Dr. Fyfe has, we think, entirely mistaken the nature of the *pulvis antimonialis*. He says that "the oxide of antimony and phosphate of lime enter into union and form a triple phosphate of antimony and lime." Now it appears to us that there is not the slightest evidence of combination existing between the oxide and the phosphate; indeed of all substances in nature, phosphate of lime, from its extreme inertness, is one of the least likely to combine with a metallic oxide. Nor can we assent to the assertion that *pulvis antimonialis* is one of the best of the antimonial preparations : on the contrary, there is the strongest evidence to prove that it is generally inert; and when it possesses power, it is impossible to determine the degree of its activity by any ordinary means.

Dr. Fyfe is not more careful in representing the opinions of others than in detailing his own; thus he says, "according to Phillips (Ann. of Phil. N. S. iv.), when properly manufactured it (*pulvis antimonialis*) should be composed of phosphate of lime and protoxide; whereas it frequently contains the peroxide, and therefore differing from James's Powder, which from the analysis of Pearson and Phillips is composed of

Phosphate of lime........	43
Protoxide of antimony....	57.

He has found, also, that instead of the oxide and the phosphate being in combination, they are frequently merely mixed, and must therefore be totally inert as a medicine."

Now as we readily acquit Dr. Fyfe of all intentional misrepresentation, we have no alternative but to suppose that he never read the paper to which he above alludes, and from which he appears to quote. On referring to Dr. Pearson's analysis, it will be observed that not a word is stated with respect to the state of the oxidizement of antimony; and for a very sufficient reason—nothing at that time [illegible] more than thirty years since,) was known on the subject; and Phillips is so far from attributing the inertness of *pulvis antimonialis* to the ingredients being merely mixed and not combined, that he considers them always mixed and never combined; and he distinctly mentions that he found the antimony in James's Powder to be peroxide, not protoxide as asserted by Dr. Fyfe.

It is quite needless to add more instances of Dr. Fyfe's want of precision with regard to his statements. We may observe that the wood-cuts which he has introduced "are few and far between;" but their quality is such, that whether intended for use or ornament, we cannot lament their scarcity.

Unless Dr. Fyfe writes for medical pupils only, he should be careful to avoid such terms as *aqua potassæ* and *aqua ammoniæ*, which common readers might find it difficult to comprehend.—We cannot omit to notice the extreme facility with which Dr. Fyfe alters the names of the various authors whom he quotes:—thus we have Herschell for Herschel, Allan for Allen, Sommerville for Somerville, Philip for Phillips, Mayou for Mayow, Creighton for Crichton, Chevreuil for Chevreul, Liebeg and Leibeg for Liebig, Daniells for Daniell, Arrago for Arago, Dobereigner and Doboreigner for Döbereiner, and many other similar mistakes. We observe also that Cryophorus is three times written Creophyrus.

We mark these merely as indications of that want of care and correctness which pervades every part of the work; and all we can say in its favour, if indeed that be any thing, is to admit that it is not unworthy of the author of the Manual.

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 8.—A COMMUNICATION was read to the Society, containing some "Particulars of the Earthquake felt in the Netherlands, and in some of the Frontier Towns of France, on the 23d of February last." Extracted from a letter to Captain Sabine, from Professor Quetelet, Director of the Royal Observatory at Brussels.

The number of earthquakes which are on record as having been experienced in the Netherlands, for many centuries past, does not exceed six or eight; and none of them have been productive of disastrous effects. Within a space of ten years, during the last century, three only took place, one of which happened in 1755, immediately after the great earthquake at Lisbon; and the last was in 1760. The one which has lately occurred was particularly felt along the banks of the Meuse; and its greatest violence was felt in the towns of Liège, Tongres, Tirelemont, and Huy: many of the walls and buildings of which suffered considerable injury; but, happily, no lives were lost. In the adjacent towns of Maestricht, Namur, Louvain, and Brussels, strong shocks were also experienced; but their violence diminished in proportion to the distance from the former, or principal, seat of concussion. They appear also to have been sensibly felt at Bonn, Dusseldorf, and Dordrecht, on one side, and at Flushing, Middleburg, and Dunkirk, on the other; although they were not perceptible at many of the intermediate towns. Slight shocks were also experienced at several of the frontier towns of France, as Avesnes, Commercy, and Longuyon; as also at the coal-mines near Liège, at the depth of from fifty to sixty toises; in which latter case they were accompanied by a hollow sound, resembling that of a heavily laden waggon. The direction in which the shocks were propagated appears to have been from east to west.

For some time before the earthquake the weather had been fine; but it became cloudy on the evening which preceded it, and continued so for several subsequent days. At Brussels the barometer had fallen during the three preceding days from 29·421 inches to 29·044; on the night before the earthquake it had risen to 29·126; and a few moments after the event, it stood at 29·233. It continued afterwards to rise; and on the 27th it had reached 30·166. At Liège, however, the barometer remained very low after the earthquake.

The shocks lasted about eight or ten seconds.

There have been experienced, since the 23d of February, slighter shocks; and these also were preceded by a great depression of the barometer.

"Another communication was also read, giving "an Account of some Particulars concerning an Earthquake experienced at Bogota, and in the Cordillera between Bogota and Popayan, on the 16th of November 1827, and the following days." Contained in a letter from Colonel Patrick Campbell, Secretary of Legation, to James Bandinel, Esq. of the Foreign Office. Communicated by Captain Sabine.

The earthquake is described by the narrator as occurring suddenly, at half-past six o'clock in the evening, whilst he was at dinner. It was announced by a loud rumbling noise; the whole house shook with violence; the decanters and glasses on the table being thrown down. The family ran for shelter under the door-way of the principal floor, which they had no sooner reached than they witnessed the fall of the towers of the cathedral opposite to them, with a dreadful crash. The whole tremor lasted about a minute. The first shock consisted of a long, undulating motion; the next was quick and violent; and the party found it difficult to preserve their balance, and were affected as if from sea-sickness. The damage sustained by the town of Bogota is immense, and has been estimated at about two millions of dollars, independently of the destruction of the cathedral, which had been completed about nine years ago, and the building of which cost 800,000 dollars. The government palace, and almost all the public offices and barracks, have either been rendered useless, or severely shattered. Of the churches, only those of the Capuchins, Carmelites, and the chapel of the convent "de la Ensenanza," can be said to have escaped without injury. Few of the houses above one story high are habitable, and even many of the low houses have been thrown down. The whole of the upper part of the Barrio del Rosorio, consisting of buildings of this latter description, now presents nothing but a heap of ruins. Many habitations which had withstood the first shocks, have given way under those which followed, although incomparably less violent. The injury to dwellings has been remarkably unequal in different parts of the town,—some streets having only partially suffered, while others are totally destroyed. Amidst this widely spreading destruction, it is fortunate that the loss of lives has been very inconsiderable, being, in the city of Bogota, limited to only five or six persons.

It appears that the earthquake was not felt much to the north of Bogota; but to the south the devastation has been most extensive. Throughout the whole of the plain of Bogota, as far as the towns of Purificacion and Neiva, there remains no church or public edifice of importance that has not been either overthrown or materially damaged. In the towns of Purificacion and Ibogué, the shock was so powerful as to throw down many houses constructed of cane, with thatched roofs. In Neiva, not only were all the public buildings destroyed by the earthquake, but torrents of rain conspired to increase the havoc. Even straw-huts were levelled with the ground; and the roofs of some of them taking fire, added to the horror of the scene, and to the extent of the calamity. Great part of the plain of Neiva was inundated: this was productive of considerable loss of lives, particularly on the banks of the Magdalena, the current of which was at first considerably lessened; but a great flood succeeded, and swept down vast quantities of mud and other substances, emitting a strongly sulphureous vapour, and attended with a general destruction of the fish.

These and other facts render it probable that some volcanic eruption took place in Tolima, an old volcano of Tocaima, from the mouth of which it is reported, that, of late, dense columns of smoke have been seen to arise, and more remarkably so on the day of the earthquake; as also from the ridge of mountains of Santa Anna in Maraquita, and the Paramo of Ruiz, which is a part of the same Cordillera, and contiguous to that of Tolima.

Popayan, which is 200 geographical miles S.S.W. of Bogota, has also suffered much from the same earthquake; many houses having fallen in consequence of the violent shocks that continued to succeed each other every six hours down to the evening of the 18th, which is the date of the latest intelligence from that place. The torrents of rain with which they were accompanied, have proved a great aggravation to the misery they have created. At Patea, still further to the S.S.W. the devastation has been still greater; some of the largest trees having been thrown down by the concussions. It is hence inferred, that eruptions have taken place at the same period in the volcano of Pasto; and the wide crevices which have appeared in the road of Guanacas, leave no doubt that the whole of the Cordillera has sustained a powerful shock.

In the plains of Bogota considerable crevices have also opened, and the river Tunza has already begun to flow through those which have appeared near Costa. In other parts of the Cordillera, although the earth has continued in motion for a quarter of an hour without intermission, the movement has been nearly insensible, and observable only by means of the compass or the pendulum.

May 15. A paper was read, entitled "A Comparison of the Changes of Magnetic Intensity in the Dipping and Horizontal Needles throughout the day, at Truernberg Bay, in Spitzbergen."—By Captain Henry Foster, R.N. F.R.S.

The observations made by the author at Port Bowen, in 1825, on the diurnal changes of magnetic intensity taking place in the dipping and horizontal needles, appeared to indicate a rotatory motion of the polarizing axis of the earth, depending on the relative position of the sun, as the cause of these changes. By Captain Foster's remaining at Spitzbergen during the late northern voyage of discovery, a favourable opportunity was afforded him of prosecuting this inquiry. Instead of making the observations with a single needle, variously suspended, as had been done at Port Bowen, two were employed; the one adjusted as a dipping needle, and the other suspended horizontally. The relation between the simultaneous intensities of the two needles could thus be ascertained, and inferences deduced relative to the question, whether a diurnal variation in the dip existed as one of the causes of the observed phænomena; or whether, the dip remaining constant, they were occasioned by a change in the intensity.

The dipping needle used, was one belonging to the Board of Longitude, and made by Dollond:—both this and the horizontal needle were made in the form of parallelopipedons, each 6 inches long, 0·4 broad, and 0·05 thick. The experiments were continued from the 30th of July to the 9th of August, and were so arranged, that in the course of two days an observation was made every hour in the four-and-twenty; that is, part of them in one day, and another part in the other day.

The observations on the horizontal needle were made in the following manner. After being freely suspended by a silk thread divested of torsion, the needle was turned somewhat more than 40° out of the magnetic meridian, and the oscillations counted only when the arc of vibration had decreased to 40°. The times of performing ten oscillations were then noted successively, until two hundred were completed: the terminal arc, and the temperature of the instrument, were also registered. The oscillations of the dipping needle were taken as follows:—one hundred with the face of the instrument east, previous to those of the horizontal needle being observed; and another hundred after the latter, with the face west,—a process which gives the mean time of observation nearly the same for both needles. Two tables are given: the first containing a register of the observations; and the second, the mean proportional intensities at every hour, in each needle, deduced from the respective times of the performance of one hundred oscillations. From a comparison of the changes occurring in the two needles, it appears, that at the time when an increase took place in the intensity of the dipping needle, that of the horizontal needle underwent a corresponding diminution, and *vice versâ*. On comparing these results with the hypothesis of a rotation of the general polarizing axis of the earth about its mean position as a centre, and employing for this investigation the formulæ given by Mr. Barlow in his Essay on Magnetic Attractions, it is found, that the radius of this circle of rotation is very nearly eight minutes. The magnitude of this radius, however, will be considerably influenced by the sun's declination.

The change of intensity of the dipping needle, in as far as it is owing to a variation of the dip, would only be in the proportion of 3726 to 3732; whereas, its actual amount is found to be one eighty-third part of the whole. This, therefore, seems to imply changes in the general magnetic intensity of the earth; but the author, limiting his present inquiry to the variations in the dip, concludes that the times of the day when these changes are the greatest and the least, are such as indicate a constant inflection of the magnetic pole towards the sun during the diurnal rotation, and to point to the sun as the primary agent in the production of these changes.

May 15.—A paper was also read, entitled, "Experiments relative to the Effect of Temperature on the Refractive Index and Dispersive Power of Expansible Fluids, and on the Influence of these Changes in a Telescope with a Fluid Lens." By Peter Barlow, F.R.S.

In a paper lately read to the Society, the author stated that he had not detected any change in the focal length of the telescope by changes of temperature; but he has since ascertained that, in order to obtain the brightest and most perfect image, the distance of the object-glass requires a minute adjustment, amounting to 0·134 of an inch, corresponding to an elevation of temperature from 57° to 84°, or a depression from 57° to 31°.

In order to introduce greater clearness and precision, the author proceeds to define certain terms which he finds it necessary to employ. By the *length of the telescope*, he would be understood to mean the distance between the object-glass and the focus; by the *fluid focus*, that between the fluid lens and the focus; and by the *focal power* of the telescope, he means the focal length of a telescope of the usual construction, which gives the same convergency to the rays, or produces an image of the same size.

As it is difficult to determine the refractive index of the fluid under different circumstances, from which their effect on the focal power of the telescope might be computed, Mr. Barlow endeavoured to ascertain by direct observations the effect of changes of temperature on the power of the telescope, and thence computes the corresponding change in the refractive index of the fluid. The result is the amount of adjustment already stated. The correction for angular measurements was the 60th part of a second in every minute for every degree of thermometric change; a quantity which, he observes, is too small to deserve notice, except in cases of extreme delicacy. The dispersions at 31° and at 84° are in the ratio of 3067 to 3084. The change in the refractive index between 32° and 212°, supposing it to increase uniformly, would be about one tenth of the whole, a proportion which is very nearly the same as the actual expansion of the fluid. Hence the author considers it as probable that in this, and all other expansible fluids, the index of refraction varies directly as the density: on the other hand, it would appear that the dispersive ratio remains at all temperatures constantly the same.

May 22d.—A letter was read from Thomas Andrew Knight, Esq., addressed to the President, containing "An Account of some Circumstances relating to the Economy of Bees."

In a former paper the author stated his having observed that, several days previous to the settling of a swarm of bees in the cavity of a hollow tree adapted to their reception, a considerable number of those insects were incessantly employed in examining the state of the tree, and particularly of every dead knot above the cavity which appeared likely to admit water. He has since had an opportunity of noticing, that the bees who performed this task of inspection, instead of being the same individuals, as he had formerly imagined, were, in fact, a continual succession of different bees: the whole number in the course of three days being such as to warrant the inference, that not a single labouring bee ever emigrates in a swarm without having seen its proposed future habitation. He finds that the same remark applies not only to the permanent place of settlement, but also to the place where the bees rest temporarily, soon after swarming, in order to collect their numbers.

The swarms which were the subjects of Mr. Knight's experiments showed a remarkable disposition to unite under the same queen. On one occasion, a swarm which had arisen from one of his hives settled upon a bush, at a distance of about twenty-five yards; but instead of collecting together into a compact mass, as they usually do, they remained thinly dispersed for nearly half an hour, after which, as if tired of waiting, they singly, and one after the other, and not in obedience to any signal, arose and returned home. The next morning a swarm issued from a neighbouring hive, and proceeded to the same bush upon which the other bees had settled on the preceding day, collecting themselves into a mass, as they usually do when their queen is present. In a few minutes afterwards a very large assemblage of bees rushed from the hive from which the former swarm had issued, and proceeded directly to the one which had just settled, and instantly united with them.—The author is led from these and other facts to conclude, that such unions of swarms are generally, if not always, the result of previous concert and arrangement.

He next proceeds to mention some circumstances which induce him to believe that sex is not given to the eggs of birds, or to the spawn of fishes or insects, at any very early period of their growth. Female ducks, kept apart from any male bird till the period of laying eggs approached, when a musk drake was put into company with them, produced a numerous offspring, six out of seven of which proved to be males.

The mule-fishes found in many rivers where the common trout abounds, and where a solitary salmon is present, are uniformly of the male sex: hence the spawn must have been without sex at the time it was deposited by the female.

Mr. Knight states that he has also met with analogous circumstances in the vegetable world, respecting the sexes of the blossoms of monoecious plants. When the heat is excessive, compared with the quantity of light which the plant receives, only male flowers appear: but if the light be in excess, female flowers alone are produced.

At this meeting His Royal Highness the Duke of Sussex was elected a Fellow of the Royal Society.

June 5th.—A paper was read, entitled "Description of a Sounding-Board in Attercliffe Church, near Sheffield."—By the Rev. John Blackburn, minister of Attercliffe.

The church of Attercliffe had long been remarkable for the difficulty and the indistinctness with which the voice from the pulpit was heard: these defects have been completely remedied by the erection of a concave sounding-board, having the form resulting from half a revolution of one branch of a parabola on its axis. It is made of pine-wood; its axis is inclined forwards to the plane of the floor at an angle of about 10 or 15°; it is elevated, so that the speaker's mouth may be in the focus; and a small curvilinear portion is removed on each side from beneath, so that the view of the preacher from the side galleries may not be intercepted. A curtain is suspended from the lower edge, for about eighteen inches on each side. The effect of this sounding-board has been to increase the volume of the sound to nearly five times what it was before; so that the voice is now audible, with perfect distinctness, even in the remotest part of the church; and more especially in those places, however distant they may be, which are situated in the prolongation of the axis of the paraboloid. But the side galleries are also benefited, probably from the increase of the secondary vibrations excited in a lateral direction. Several experiments are related illustrative of these effects; among which the most striking was one in which a person placed so as to have one ear in the focus of the paraboloid, and the other towards a person speaking from the remote end of the church, heard the voice in a direction the reverse of that from which it really proceeded. The superior distinctness of sounds proceeding from the focus, is accounted for by their all arriving at the same moment of time, at a plane perpendicular to the axis, after reflection from the surface of the paraboloid; which is a consequence of the equality of the paths which they have described.

LINNÆAN SOCIETY.

June 17.—A paper was read, entitled, "Description of a species of *Tringa* killed in Cambridgeshire, new to England and Europe," by Wm. Yarrell, Esq. F.L.S.

The author describes a singularly marked *Tringa*, which was shot in Cambridgeshire in the month of September 1826.

This bird is rendered more than usually interesting from the circumstance that it is not only new to this country, but is acknowledged by the best practical ornithologists of the day, to be also entirely new to Europe. It is described by Monsieur Vieillot, under the name of *Tringa rufescens*, as having been found in Louisiana; and a single specimen deposited in the Paris Museum has furnished the only records known.

A description of the plumage, and the measurement of various parts, are given in detail; and the paper concludes with a list of the more recent additions to British ornithology, accompanied by references to the various authorities from whom the first notices of these addenda have emanated.

ASTRONOMICAL SOCIETY.

May 9.—A paper was read, entitled "Approximate Places of Double Stars in the Southern Hemisphere, for 1827, as observed at Paramatta, N. S. Wales. By Mr. James Dunlop."

After the departure of Sir T. M. Brisbane from the Colony of New South Wales, the author, finding himself in the possession of reflecting telescopes capable of adding considerably to our knowledge of the nebulæ and double stars of that portion, resolved to remain, for the purpose of making a general survey, of the heavens, from the south pole to 30° of south declination. The dark nights in the absence of the moon were devoted to observations of the nebulæ, and the moonlight to those of double stars, of which however only a part could be subjected to exact micrometrical measurement. The apparatus employed for this purpose consisted of a 46-inch achromatic telescope, equatorially mounted, and furnished with two micrometers;—one a parallel-line micrometer, the author's own workmanship; the other, a double-image micrometer, on Amici's principle. Those which could not be micrometrically measured, had their positions and distances noted by estimation while passing the field of the 9-feet reflector, with which they were discovered in the sweeps for nebulæ, and their places are given as determined in the sweeps.

The author prefaces his catalogue with the details of the micrometrical measures of about 30 principal Southern double stars, the most remarkable of which are *α Crucis* and *α Centauri*, the former bearing a great resemblance, both in the magnitudes and the mutual distance of its individuals, to *Castor;* the latter being a star of the first magnitude, accompanied by one of the fourth, at about 20″ distance,—a remarkable combination, such as does not occur in our hemisphere.

A Catalogue of 254 double stars arranged in order of right ascension follows, in which the right ascension to seconds of time, and declination to the nearer minute of space,—the position, quadrant, distance, the differences of right ascension and declination when observed, and the magnitudes, are set down in separate columns. They comprise double stars of all classes and of every variety. One very remarkable is the star 1 k *Argus*, Æ 8^h 4^m, declin. $-42° 7'$, which consists of individuals of the sixth and eighth magnitudes, the large star being blue, and the small one dusky red. This affords almost the only instance known of a combination of two considerably bright stars differing decidedly in magnitudes, where a marked excess of the less refrangible rays enters into the composition of the light of the smaller star, and of the more refrangible into that of the larger. Among the double stars is set down also one of the seventh magnitude, right ascension 1^h 19^m 43^s, declin. $-33° 31'$, of that singular deep red purple colour of which examples are not wanting in our own hemisphere.

An extract of a letter was read from Professor Harding, of Göttingen, to Dr. Tiarks, in which he alludes to a phænomenon which had recently been observed by several astronomers on the continent, relative to an inequality of the dark space between the body of Saturn and its ring. This appearance was first noticed by M. Schwabe on December 21, 1827, and has since been confirmed by several persons to whom M. Harding had communicated the circumstance. It seems that the space on the eastern side of the planet appears larger than the space on the western side. M. Harding was at first inclined to treat the whole as an optical deception, till the fact was confirmed by others, when he was induced to attempt an explanation of the phænomenon. He endeavoured to account for it by the present position of Saturn; but the result of his calculation proved that that cause would not increase the space (in March) more than $\frac{1}{88}$; a quantity probably too small to become perceptible to the eye. He indeed imagined that the appearance might be caused by the shadow of the body, which at present falls much beyond the south-eastern part of the ring, and which might render it impossible to perceive the equality of the two spaces. But this, he says, is disproved by the observation of M. Schwabe, who saw the same phænomenon on the 31st of December, three weeks before the opposition, when the shadow was on the western side, and could be but faintly discerned. M. Harding is unable to explain it as an optical deception, and yet cannot consider it in any other light at present. Actual measurement, he says, can alone decide the question. He has already written to M. Struve to take some measures with his powerful telescope, and he requests that this communication may, with the same view, be forwarded to Messrs. Herschel and South, who have the best means, in this country, of determining this singular phænomenon.

Mr. South then read a note, which he had annexed to the above communication, stating that in compliance with M. Harding's wishes, Mr. Herschel and himself had directed their attention to Saturn, but that they did not detect any inequality in the two spaces above alluded to, by means of micrometers attached to his 5-feet equatorial. The mean of 35 measures, taken on April 26, April 29, and May 8, gave the preceding (or western) space 3″·532; and the following (or eastern) space 3″·607. At the same time he remarks that the mean of 20 measures taken on April 26 (viz. 10 by Mr. Herschel and 10 by himself) gave the spaces precisely the same; each being 3″·472. Mr. Herschel's measures gave the preceding (or western) space 3″·612; and the following (or eastern) space 3″·442; whilst his own gave the former 3″·331, and the latter 3″·502. Mr. South adds, however, that Mr. Herschel, after a careful examination, thought that, beyond all doubt, the following (or eastern) space appeared the larger: and it is a remarkable fact, that of seven persons who were present in Mr. South's observatory shortly afterwards, and who successively viewed Saturn through his 5-feet equatorial, six of them gave it as their opinion that the apparent right (or eastern) space was the larger: whilst the other observer declared he could not distinguish any difference. The situation, however, of Saturn was so low, as to render most of these observations far from satisfactory.

M. Harding also alludes in his letter to the reappearance of the variable star in the constellation *Serpens*, mentioned in No. 5. of the Society's monthly notices. (See Phil. Mag. N. S. vol. ii p. 226.) He says, it is now again become visible, and has already attained the 8th or 9th magnitude. Its position for the beginning of this year is

$$Æ = 15^h\ 46^m\ 45^s \quad \text{Decl.} = +\ 15°\ 39'\ 30''$$

and he invites astronomers to watch this star during the period of its changes.

A communication was then read from Mr. Rumker of the observatory at Paramatta in New South Wales, giving an account of his observations for determining the absolute length of the pendulum vibrating seconds there, according to Borda's method. The apparatus, with which these experiments were made, was constructed by Fortin, of Paris, and taken out to the colony by Sir Thomas Brisbane. There are some slight alterations from the apparatus described by M. Biot, which are pointed out by Mr. Rumker: and he also alludes to a new method of observing the coincidences. In Borda's method, the coincidence is determined by the intersection of the wire of the pendulum of experiment with a cross marked on the bob of the pendulum of the clock. In lieu of this cross, Mr. Rumker placed a small graduated arc, and the determination of the coincidence resolves itself into observing the moment when the wire describes its minimum amplitude on the arc. Mr. Rumker likewise adopts a new mode of determining the correction for the arc of vibration. He finds that in large arcs (such as 8 or 9 degrees, to which his arcs sometimes extend) the decrease is not in a geometrical progression, when the times are in arithmetical progression. He has therefore formed a table of the actual decrease of the arcs as observed by himself, at equal intervals of five minutes each; and given the corresponding corrections for each interval. In the course of his reductions he notices some errors in the formula given by M. Biot for finding the centre of oscillation of a pendulum constructed according to the method of Borda. The mean of 41 series of experiments gives the length of the pendulum, vibrating seconds at Paramatta, *in vacuo*, at the freezing point, and at the level of the sea, equal to 992·412801 millimetres, or 39·071618 English inches.

PROCEEDINGS AT THE FRIDAY-EVENING MEETINGS OF THE ROYAL INSTITUTION OF GREAT BRITAIN.

May 23.—Mr. Brockedon on a new method of projecting shot. —This method belongs to Mr. Sievier. It consists in making the shot with a cylindrical chamber, so as to pass freely on to a maundril or bar fixed on trunnions, a powder-chamber being formed at the bottom of the cylindrical cavity in the shot. The powder is inflamed by means of a touch-hole in the shot, in the usual way. A charge of powder thus used is found to produce effects very much surpassing that occasioned when a shot of equal weight is thrown from a cannon; and this is accounted for by supposing that the force of recoil, which in a cannon is so great as to throw it a considerable distance backwards, is added in the new form of shot to the usual quantity of projectile force. The experiments made with shot weighing up to twenty-five pounds, were successful both as to force and direction; and the extraordinary advantage gained as to lightness in the apparatus necessary to throw the shot, was proved by one man taking all that was necessary to Primrose-hill, the place of experiment.

Some fine fulgurites or lightning sand-tubes were placed on the library tables.

May 30.—Mr. Curtis gave a lecture on the structure and physiology of the Ear in man and animals, illustrated by drawings and anatomical preparations.

June 6.—Mr. Burnet gave an illustrated account of the experiments recently made by himself and Mr. Mayo, on the irritability of the sensitive plant, and extended his observations to the irritability and supposed nervous structure of plants in general. Their experiments accord with those of Dutrochet, as far as the two series run parallel. When a sensitive plant has been made to droop, Mr. Burnet finds that if the part in which the moving power resides is blackened so as to absorb the light of the sun, the restoration of the plant to its natural state is very much longer before it takes place. He also found that at the moment the expansion at the foot of the leaflets or other parts were touched to produce the motion of the plant, it changed colour.

June 13.—A full account of the recent and present state of the Thames Tunnel was given by Mr. Faraday, illustrated by Mr. Brunel's drawings and models. The peculiar nature of the ground in which the tunnel lies, the occurrence of springs in the soil, the extraordinary manner in which they affect it during the rise and fall of the tide, were stated and explained; and then the present state of the tunnel, now perfectly free from water, and the intentions of the engineer with regard to its future progress, were described.

These evening meetings then closed for the season.

XI. *Intelligence and Miscellaneous Articles.*

DIRECT METHOD OF ASCERTAINING THE VELOCITY OF CANNON-BALLS.

LIEUT.-GEN. HELVIG, in the Prussian service, has invented a direct and certain method of measuring the time which a cannon ball or bullet takes to pass through a certain space. His process consists in disengaging by means of the ball or bullet the detent of a third's watch (*une détente de montre à tierce*) at the moment when the ball or bullet quits the mouth of the piece, and to stop the same watch by means of the ball or bullet at the instant when it reaches the mark. The numerous experiments which he has made, present already the most interesting results. He has communicated this notice in order to establish his right to the invention, but intends shortly to publish a full detail of his experiments upon the subject.—*Bulletin des Sciences Militaires*, p. 119.

CHRYSOLITE IN THE CAVITIES OF OBSIDIAN.

Professor Gustavus Rose of Berlin has found in the cavities of obsidian, in the Jacal Rock near Real-del-Monte in Mexico, little crystals, greenish, and reddish-yellow, and transparent, which belong to the species of prismatic chrysolite.—Poggendorff's *Annalen*, vol. x. p. 323.

METEOR OF A GREEN COLOUR.

[Communicated by Mr. B. D. Silliman, in a letter dated New York, March 1, 1828.]

On the night of the 11th of February, between 11 and 12 o'clock, as I was crossing the East River, between this city and Long Island, I observed a beautiful meteor which was visible for about the space of two seconds. Its course was from a point perhaps 5° below the zenith, toward the horizon in a N.E. direction. It described an arc of perhaps 20°, when it apparently exploded, but without any report that I could hear. Its colour was a singularly pure grass green, of a light shade; the trail which it left was of the same colour, and so were the scintillations which accompanied its apparent explosion. The latter were distinct, like those accompanying the bursting of a rocket, but by no means so numerous.—Two gentlemen who were in the boat with me at the time, also saw it.—*Silliman's Journal.*

BITTER OF ALOES.—CARBAZOTIC ACID.

It is well known that the peculiar substance produced by the action of nitric acid upon aloes, combines with bases, and forms salts which detonate by heat; this substance is the aloetic acid of M. Braconnot. Mr. Liebig formerly made some experiments on this substance, (*Ann. de Chim.* Mai 1827,) but they were not satisfactory. He has lately renewed his experiments, and finds that the detonating principle is carbazotic acid.

The bitter of aloes is plentifully obtained by the action of nitric acid of sp. gr. 1·25. With potash it forms a purple salt, which is but slightly soluble, which precipitates the salts of barytes, lead, and peroxide of iron in flocks of a deep purple colour; the protonitrate of mercury is precipitated of a light red. In order to decompose the salt of potash, it was decomposed by acetate of lead; and, contrary to all expectation, the weight of the precipitate was less than that of the potash-salt employed. The washings were of a yellow colour, and deposited crystals of the same. These crystals, treated with heat and sulphate of potash, yielded carbazotate of potash, from which the carbazotic acid was separated.

When aloes are heated with nitric acid of 1·432 as long as vapours of nitrous acid are disengaged, and on afterwards mixing the liquor with a little water to separate the bitter, there may be obtained by saturating the liquor with potash and evaporation, a large quantity of carbazotate of potash in fine crystals. The bitter of aloes is consequently a compound of a peculiar substance, possessing the properties of the resins, and carbazotic acid.

Wool, morphia, narcotine, and myrrh, yielded no carbazotic acid when treated with nitric acid.—*Annales de Chimie*, Fev. 1828.

HEAT DEVELOPED DURING COMBUSTION.

M. Despretz finds that when equal quantities of oxygen are used for the combustion of the following substances, the annexed proportions of heat are developed :

Hydrogen	2578°
Charcoal	2967
Iron	5325

Phosphorus, zinc, and tin give nearly the same quantities as iron. It appears then that of all bodies, hydrogen develops the least heat for the same proportion of oxygen gas absorbed; the metals disengage the most. It is remarkable that carbon, which does not alter the volume of the gas, evolves a quantity of heat which is equal to three-fifths of that given out by iron and the metals in general.—*Ibid.*

ON THE SUGAR OF LIQUORICE-ROOT.

Döbereiner and Robiquet have long since given processes for the purification of this substance; the latter precipitates it by vinegar. Berzelius separates it in the following manner. The liquorice-root is to be sliced and infused in boiling water; when cold the infusion is to be filtered, and sulphuric acid added to it gradually, as long as precipitation occurs. This precipitate is a compound of the acid with the saccharine matter. It is first to be washed with acidulated cold water, and then with pure water, till it ceases to be rendered acid: the precipitate is afterwards to be digested with alcohol which separates the vegetable albumen and dissolves the compound of sugar and sulphuric acid; there is then to be gradually added to the solution carbonate of potash or soda in fine powder, and when it ceases to be acid, it is to be decanted and evaporated. It is proper to leave a very slight excess of acid in the solution, and for this purpose it is convenient to set aside a portion to be afterwards added to the saturated solution, until it is rendered weakly acid. The liquor is then to remain in order that the sulphate of potash may separate, and afterwards it is to be evaporated.

The saccharine matter is obtained in the form of a yellow transparent mass, which breaks into a coarse powder resembling amber—when heated in the air it swells up, inflames, and burns with a bright flame, but with smoke. When in powder it burns like the lycopodium or powdered resin. It suffers no change by exposure to the air. The aqueous solution is precipitated by all acids, and the more perfectly as the solution is more concentrated, and especially if excess of acid be used. The washed precipitates have no sour taste, but on the contrary a pure saccharine flavour which is developed in a short time. The precipitates are soluble in boiling water, and on cooling a yellow transparent jelly is formed if the solution be concentrated. Alcohol also dissolves them, and they burn without leaving any residuum.

The saccharine matter of liquorice combines also readily with bases; it is on this account very difficult to separate it from acids, without its retaining a portion of the bases employed for that purpose. The compounds with the alkalies dissolve readily in water, but with difficulty in alcohol; when they are perfectly saturated they contain no trace of carbonic acid, even when bases combined with carbonic acid have been employed, and their taste is purely saccharine, without mixture of alkalinity. The compounds formed with barytes and lime are soluble and are not precipitated by carbonic acid; this saccharine matter forms insoluble compounds with the metallic acids,—when poured into a solution of acetate of lead, a precipitate is formed, which when decomposed by sulphuretted hydrogen, forms a black liquid, in which the sulphuret of lead remains suspended; if it were not for this, it would be a good method of obtaining pure saccharine matter: the same substance is obtainable from the inspissated liquorice juice; but it is black and cannot be decolorized. It unites not only with acids and bases like the yellow saccharine matter, but also with salts, such as the sulphates of barytes, lime and potash. It precipitates many metallic salts.—*Ibid.*

SOLUTION IN SULPHURIC ACID WITHOUT OXIDIZEMENT.

Vogel of Bayreuth, whilst examining anhydrous sulphuric acid, found that sulphur by being put into contact with it, imparted to it a fine blue colour, which passes to green or brown by the addition of a greater quantity of sulphur. Water precipitates sulphur from these combinations, and heat decomposes them. It appeared probable that the sulphur was simply held in solution by the sulphuric acid, and M. Magnus mentions several analogous cases, which leave no doubt on the subject.

Müller of Reichenstein discovered long since that powdered tellurium when sprinkled with concentrated sulphuric acid, was dissolved and became a perfectly transparent fluid of a fine crimson-red colour without observing any evolution of gas, or smell of sulphurous acid. On the addition of a proper quantity of water the tellurium is precipitated in the state of a deep blackish-brown metallic powder. This solution may be kept for a long time in a close vessel, without any alteration; but if it attract moisture from the air, it gradually changes into sulphate of oxide of tellurium, and continually exhales the odour of sulphurous acid. This change is readily effected with the assistance of heat. Selenium is also dissolved by sulphuric acid, the solution is of a very fine green colour, and a few drops of water precipitate the selenium of a red colour.

Tellurium and selenium act like sulphur with sulphuric acid; except that sulphur requires for its solution that the acid should be anhydrous. These three bodies are oxidized when the sulphuric acid attracts moisture gradually, and exhale an odour of sulphurous acid; but if the water be added quickly, they are then precipitated. Lastly, the three solutions are coloured—that of the sulphur being blue, green or brown, the tellurium crimson-red, and the selenium green.

According to Bussy, iodine is also soluble in anhydrous sulphuric acid, and gives it a blueish green colour.

It follows from these facts that sulphuric acid has the property not only of dissolving compound bodies without oxidizing them, as Berzelius has shown with respect to the metallic cyanurets, but it dissolves some simple bodies, such as sulphur and selenium, for the oxides of which it has no affinity, and also tellurium, with the oxide of which it forms a crystallizable compound.—*Ibid.*

VEGETABLE ALBUMEN AND GELATINE.

Beccaria discovered, as is well known, a peculiar glutinous principle in wheat, which is obtained by working the flour in water, and which he called gluten. Taddei has given an account of two new peculiar principles which he supposes he has found in gluten, and which he has named *gliadine* and *zymome*. The other kinds of grain yield no principle similar to the gluten of Beccaria. But Einhof, in his remarkable analysis of rye, barley and pease, has shown that these seeds contain a substance analogous to the gluten of wheat, but which dissolves in water during the manipulation. Having had occasion to make some experiments on the gluten of Beccaria, I found that Taddei had only given two new names to the known and common principles of plants, particularly the seeds of the *gramineae*.

If the gluten of Beccaria be boiled with alcohol, as long as this fluid grows turbid on cooling, a considerable portion of the mass is separated; if water be added to this spirituous solution, and the mixture be distilled, the watery fluid remaining in the retort deposits on cooling a coherent glutinous matter, perfectly resembling gluten. This is vegetable gelatin, the gluten, of the same nature as the matter separated, according to Einhof's method, from rye and barley. The matter insoluble in alcohol, whilst moist is semitransparent, and so much like animal albumen, that it is impossible to distinguish by its appearance only, that it is vegetable albumen, or, as Wahlenberg calls it, with good reason, the *white* of grain. Caustic alkali, when the solution is weak and cold, dissolves vegetable albumen, and leaves the filaments of starch which it has retained. The following are the principal properties of vegetable albumen. This matter, obtained after the evaporation of the alcohol from the remaining liquor, is of a yellowish gray colour, adhesive, glutinous, and very elastic; it has no taste, but it has a peculiar smell. In a dry atmosphere it becomes shining on the surface, and gradually dries into a mass of a deep yellow colour, and is perfectly transparent, resembling dry animal matter. It dissolves in alcohol, and the solution is of a pale yellow colour, and remains after the evaporation of the spirit, in the form of transparent yellow varnish. When vegetable gelatine is treated with cold alcohol, a milky fluid is obtained, and a viscid white matter remains. This matter is not vegetable gelatine; it is dissolved by boiling, but the liquor becomes milky on cooling. If the vegetable gelatine be dissolved with heat in weak spirit of wine, it precipitates on cooling, retaining its glutinous property; it dissolves in vinegar, leaving a white viscid matter, which the acid does not dissolve even when boiling, but which partly passes through the filter. When precipitated from its solution in vinegar by an alkali, it retains its glutinous state. With the mineral acids it forms a glutinous compound, insoluble in water, until the excess of acid is removed, and it is then as perfectly precipitated from this solution as from that in vinegar, when more acid is added. The phosphoric acid is however an exception, for it does not precipitate the acid solutions. Vegetable gelatine also combines with the caustic alkalies, and when the gelatine is in excess, a solution is obtained, which is so perfectly neutral that no alkaline taste remains. It gives by evaporation a transparent mass, which is again soluble in water, which leaves undissolved the greater part of the viscid principle. Ammonia and lime-water precipitate vegetable gelatine from solution in acids, and redissolve it; but if it be aggregated these alkalies do not dissolve it, or at least the solution is slowly effected. With the earths and the metallic oxides, vegetable albumen forms insoluble compounds; the alkaline carbonates precipitate vegetable albumen from solution in the caustic alkalies or in the acids. The precipitate is a compound of the gelatine with the alkali, which without the liquid is not gelatinous. The persulphate of iron does not precipitate vegetable gelatine from solution in vinegar. On the contrary, it is precipitated from its acid solutions by the ferrocyanate of potash, in a hard, white, semitransparent mass, which is deposited on the sides of the vessels. It is also precipitated from solution, either in acid or alkali, by the permuriate of mercury and tincture of galls. The gelatine, in the solid state, is *tanned* in the two solutions, exactly like animal gelatine. The viscid principle, which has been several times mentioned, has not had its properties examined. The best method of separating it is to treat vegetable gelatine with concentrated vinegar, and when the mass is thoroughly penetrated, to mix it in the cold with weak alcohol, which dissolves the acetate of gelatine, and the undissolved matter is also to be washed with cold weak spirit. It dries into a colourless transparent body, which yields ammonia by distillation. It swells in alcohol and becomes viscid; when heated in it, solution takes place, but it is precipitated on cooling.

Vegetable albumen, when dissolved to saturation in weak alkaline solutions, possesses in so great a degree the properties of white of egg, that, as is well known, it has been mistaken for it. Its solution in potash, when the latter is not in excess, has no alkaline taste whatever. It coagulates slightly by ebullition, but it is generally retained by the alkali; it combines with acids. The solution when perfectly saturated is soluble in water, but an excess of acid precipitates it; vinegar and phosphoric acid, however, are exceptions to this, for they may be added in large quantity, without occasioning precipitation. Before treatment with potash, vegetable albumen when boiled in alcohol dissolves sparingly in vinegar or phosphoric acid; but when boiled with these acids, it forms a transparent jelly, which is colourless and bulky. With permuriate of mercury, tincture of galls, and ferrocyanate of potash, it acts like animal albumen.

The French chemists have considered the azotized principle contained in emulsive seeds as analogous to cheese in milk. Soubeiran has shown that this principle in almonds, similar to that which has been described, possesses the properties of white of egg, but not those of cheese; and Payen and Henry, who had considered the results obtained by Soubeiran as opposed to theirs, are convinced by new trials, that this principle cannot be considered as *caseum*, but that it ought to be called *albumino-caseous*. I add, that according to its properties, it is rarely identical with vegetable albumen.—*Ibid.*

BERZELIUS.

ANHYDROUS CRYSTALS OF SULPHATE OF SODA.

In the Number for April of the Royal Institution Journal, Mr. Faraday makes the following observations with respect to this salt:—If a drop of a solution of sulphate of soda be placed upon a glass plate and allowed to evaporate spontaneously, it will leave crystals which may be distinguished by their form and alternate efflorescence as being the salt in question. Most of the potash and soda salts may be distinguished as to their base by such an experiment. They are easily converted into sulphates by a drop or two of sulphuric acid and ignition; and then being dissolved and tried as above, will yield crystals which may be known by their forms, and more especially by their efflorescence if of soda, and their unchangeable state if of potash. This test is, however, in some circumstances liable to uncertainty, arising from a curious cause. If the drop of solution on the glass be allowed to evaporate at common temperatures, then the efflorescence takes place, and the distinction is so far perfect; but if the glass plate with the drop upon it be placed upon a warm part of a sand-bath or hot iron-plate, or in any other situation of a certain temperature considerably beneath the boiling point of the solution, the crystals which are left upon evaporation of the fluid are smaller in quantity, more similar in appearance to sulphate of soda, and finally *do not* effloresce. Upon examining the cause of this difference I found they were *anhydrous*, consequently incapable of efflorescing, and indeed, exactly of the same nature as the crystals obtained by Dr. Thomson from certain hot saturated leys.—*Ann. Phil. N. S.* xx. 201.

Hence it would appear, that a mere difference in the temperature at which a solution of sulphate of soda is evaporated, will cause the formation of hydrated or anhydrous crystals at pleasure, and that whether the quantity of the solution be large or small. This indeed might have been expected from what takes place when hydrated crystals of sulphate of soda are carefully melted; a portion dissolves, and a portion separates,—the latter in an anhydrous state. (*Quarterly Journal*, xix. p. 153.) I find that, if it were desirable, crystallized anhydrous sulphate of soda might easily be prepared for the market; though, as the pure salt is now but little used, it is not likely this condensed form will be required. Whenever a salt of soda is to be distinguished from one of potash in the manner above described, this effect of temperature must be carefully guarded against.

CASEOUS OXIDE, AND CASEIC ACID.

The results obtained by Proust relative to the substance produced by the fermentation of cheese, have been described and examined by M. Henri

M. Henri Braconnot. The substance which Proust distinguished as caseous oxide, he shows to have no claim to such a title, and proposes to call it *aposepedine*, as being produced by putrefaction. It also appears to be produced in certain diseases. The properties which Proust has assigned to *caseic acid*, belong, according to M. Braconnot, to various contaminating substances, none of which have any title to be considered as a particular acid. The substances present are free acetic acid; aposepedine; animal matter, soluble in water and insoluble in alcohol (ozmazome); animal matter, soluble in both water and alcohol; a yellow, acrid, fluid oil; a brown resin; acetate and muriate of potash, and traces of acetate of ammonia.

On examining the fatty matter of cheese, Braconnot found it to consist of margarate of lime with margaric and oleic acids; the butter having undergone the same kind of change during the fermentation of cheese, as that produced when it is saponified by the action of alkalies or other bodies.—*Ann. de Chim.* xxxvi. p. 159.

RIB OF A WHALE FOUND IN THE DILUVIUM OF BRIGHTON CLIFFS.

A short time since some labourers employed in collecting flints from the beach near Kemp Town (a new suburb erecting to the east of Brighton), observed the extremity of a large bone projecting from the base of the cliff. They immediately broke off a portion of it, but the remainder was fortunately so impacted in the rock that they were unable to remove it without more labour than they were willing to bestow. Intelligence of the discovery having reached Mr. Mantell of Castle Place, Lewes, he visited the spot, and assisted by the labourers, succeeded in making an excavation to the extent of three or four yards in the cliff, and completely exposed the bone without injuring it in the slightest degree; but unfortunately in attempting to remove it subsequently, it fell to pieces*. This fragment of bone (for it evidently was but a small portion of the original) measured nine feet in length, the piece destroyed by the workmen was estimated at about three feet, so that the specimen when first discovered must have been twelve feet long; from its slight degree of curvature it could not have been less than thirty feet when entire. The circumference of the largest extremity was thirty-four inches, and the bone gradually diminished in size, terminating obtusely. The surface was almost flat on the inner side of the curvature, and convex on the outer, corresponding in this respect with the ribs of the common whale. From a mere fragment of bone, however gigantic, it is of course impossible to decide positively as to the animal to which it belonged; yet as this example was too enormous to have belonged to any terrestrial animal, and not only in form but also in structure bore a close analogy to the rib of a whale, it may with but little hesitation be considered as the sternal portion of a rib of that animal. According to

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* A fragment five feet long was, however, removed to Mr. Mantell's museum.

Mr. Mantell's description of the cliffs at Brighton, (Geology of Sussex, p. 277,) they consist of

1. *Calcareous bed*, composed of the ruin of chalk strata with clay, &c., fifty to sixty feet thick.
2. Bed of shingle or pebbles, five to eight feet.
3. Sand, three to four feet, with boulders of granite, porphyry, slate, &c.
4. Upper chalk, forming the sea-shore.

The bone was imbedded in the sand No. 3, lying beneath the shingle bed and upon the chalk. Vast quantities of the teeth of the horse, and a few of a species of ox, and of the elephant (*E. primigenius*), have lately been discovered in the calcareous bed.

INEQUALITY OF THE DARK SPACE BETWEEN THE BODY OF SATURN AND ITS RING.

Do the observations of Sir W. Herschel on an apparent irregularity in the figure of Saturn, (recorded in the Philosophical Transactions for 1808, p. 160,) throw any light upon the recent observations of MM. Schwabe and Harding, and of Messrs. Herschel and South, on the apparent *inequality* of the dark space between the body of this planet and its ring, as noticed in No. 12 of the Monthly Notices of the Astronomical Society, which has just been circulated?

INQUIRER.

NATIVE IRON ? SLIGHTLY ARSENIURETTED.

The substance described below, was brought to me two or three weeks since, by Mr. Philo Baldwin*, who stated that it was from Bedford county, Pennsylvania, in which county we believe Mr. B. lives.

Perceiving that it was a singular modification of iron, and different from any thing I was acquainted with,—it was, at my request, submitted by Mr. S. to chemical examination.

My impressions are, that it is a new variety of native iron, and that it differs from that substance only by containing a little arsenic, with a little plumbago. Measures will be taken to obtain a greater supply, as it is stated to be abundant, and will at least form an interesting addition to our cabinets.

Chemical examination.—The fragment weighed, I should judge, two or three ounces; and although it had sustained considerable injury, it evidently formed a distinct crystal. By observing a symmetrical modification which this crystal had undergone, in the truncation of two of its alternate obtuse solid angles, I was able easily to ascertain, that it belonged to the class of rhombic prisms, but whether the prism was right or oblique, I could not determine. The natural planes were not sufficiently even, to allow of the determination of their angles with perfect accuracy: neither were the results, from numerous cleavage-planes, uniform enough for this purpose; although in the latter case the reflective goniometer was used with the utmost convenience. The inclination of the primary planes may be regarded as an approximation to 121° and 59°, and those of the secondary (intersecting the base parallel to its greater diagonal) to the primary 146°. With the cleavage-crystals the following angles were obtained, 120°, 121°, and 122°; a diversity very remarkable, as the cleavages appeared to the eye quite perfect, and the planes highly uniform.

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* Mr. Baldwin went to Newtown, Connecticut, where he formerly resided, and was to return in a week to learn the nature of the mineral, but has not yet called, which prevents me from stating the exact locality.—*B. S.*

The cleavage parallel to the lateral planes is effected without much difficulty, whilst no terminal one is visible; it breaking in that direction with great difficulty, and presenting an uneven and sub-hackly fracture. The external planes of the crystals before being broken, were dull and nearly black, owing to a thin coating of brown oxide of iron, but fresh cleavages presented a fine metallic lustre, and a colour between silver-white and steel-gray. It breaks with the greatest difficulty, and small masses often flatten under the blow of the hammer, like pure iron. Its hardness is almost that of ordinary steel. Specific gravity, in distilled water at 60° F., 7·337. It is highly magnetic, with polarity so distinct as to take up iron filings. Before the blowpipe it melts.

Fragments of the size of a pea, brought within the exterior flame of the compound blowpipe, emitted a very slight vapour, in which the well-known odour of arsenic was detected: and immediately on coming within the inner cone of flame, they burnt with intense energy, and with a most brilliant light, throwing out a profusion of scintillations, after the manner of pure iron, or more like a burning watch-spring. No odour of sulphur was perceived in these trials. In order, however, to make myself sure of the absence of sulphur, I resorted to the following experiment. A portion of the metal was dissolved in dilute nitric acid: the solution was supersaturated with potash and boiled in the alkaline liquor; the precipitate was separated, and the supernatant fluid neutralized by nitric acid, to which was afterwards added nitrate of lead; the precipitate was separated, and found to be perfectly soluble in dilute nitric acid, thus evincing the absence of sulphate of lead, which must have formed part of the precipitate, provided sulphur had existed in the mineral under examination.

After having examined it in the usual modes, for silver, gold, and other metals, and not discovering any to be present*, I dissolved fifty grains in nitric acid, with a view to ascertain merely the proportion of iron present. After the solution appeared to be effected, I observed a number of little black flakes floating in the liquid, which resisted the action of the acid. These being separated by the filter, were examined and found to be plumbago, which, under somewhat similar circumstances, though less disguised and more abundant, was found in the native iron of Canaan. They weighed 0·2 grs. and from other trials, appear to exist in the mineral pretty constantly in this proportion. The nitric solution was precipitated by ammonia, and the residuum after drying indicated 48·7 grs. of metallic iron.

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* After the iron had all been removed from the nitric solution by ammonia, and the fluid boiled, hydro-sulphuret of ammonia gave no cloudiness, thus evincing the absence of nickel.

I afterwards repeated my examination with greater care in the following manner. Twenty-five grs. were dissolved in dilute nitric acid. This solution was boiled for some time with an excess of soda, and deposited 35 grs. of the peroxide of iron. The supernatant liquor with the washings of the precipitate being evaporated and neutralized by nitric acid, was decomposed by nitrate of lead, and afforded a precipitate weighing 1·5 gr. which upon burning charcoal gave the smell of arsenic, and was entirely soluble in nitric acid, and therefore consisted wholly of arseniate of lead. The result of my trial, then, would be as follows, after deducting the weight of the plumbago: for 24·9 of the mineral,

Iron	24·263
Arsenic	·389
	24·652
Loss	·248
	24·9

Which gives per hundred of the mineral, free from the plumbago,

Iron	97·44
Arsenic	1·56
	99
Loss	1
	100

This therefore cannot but be regarded as a singular substance, especially as it affords us an instance of the remarkable effect produced by a small proportion of arsenic in disguising the natural properties of iron. Whether it coincides with the species described by Mohs under the name of axotomous arsenical pyrites, (to which opinion I am rather inclined, from its crystalline character and specific gravity,) or whether it constitutes a distinct species in mineralogy, I will not at present venture to assert. When an additional supply of this substance shall be furnished us for examination, and the means of comparing it with some genuine specimens of the above-mentioned species shall occur, it will be very easy to decide upon this point.

Yale College, March 4th, 1828. CHARLES U. SHEPARD.

ARSENIATE OF COBALT.

Arseniate of cobalt has been lately discovered at the lead mine of Tyne Bottom, about three miles south of the town of Alston in Cumberland, by Mr. H. L. Pattinson, Assay Master for the Commissioners and Governors of Greenwich Hospital, in the Manor of Alston Moor. It occurs in the form of a rose-coloured efflorescence, investing hepatic and common pyrites; and specimens in great plenty are to be picked up on the old mining heaps. The veins at Tyne Bottom bear nearly east and west, and are worked in a limestone stratum called the Tyne Bottom Limestone in Mr. Westgarth Forster's section of the strata which occur in that district. They were formerly very productive of lead-ore; and beautiful specimens of transparent and finely crystallized carbonate of lime were obtained, but for a few years past the quantity of ore yielded has not been considerable.

SOLAR SPOTS, &c.

On the 27th of May, thirty-two maculæ or black spots, in groups, were observed on the sun's disc; the largest with its umbra exceeded by admeasurement the circular extent of the earth, and was situated near the central part of the arc which formed the lower right-hand quadrant. The nucleus of this spot, or the opening in the sun's atmosphere (a rational hypothesis of the late Dr. Herschel), was in *the shape of a man's hat*, and the well-defined speckled umbra nearly so, with the exception of the angular parts. Seven of the largest spots were in a line near the sun's centre, and four near the upper limb; most of the others were interspersed about the largest, which went off the visible part of the disc by means of the sun's motion on its axis in the night of the 29th.

The apparent angular distance of the planet Venus from the sun's centre at the time of its greatest eastern elongation on the 19th, was 45° 28′ 30″, when its appearance was like the moon at her last quarter with an inverting telescope, or at her first quarter without an inversion. This planet, which is the most radiant in the solar system, and which now casts a faint shadow in the evening after twilight, may be seen with the naked eye in the open day in clear weather during the next four weeks.

LIST OF NEW PATENTS.

To W. Marshall, of Fountain Grove, Huddersfield, for improvements in machinery for cutting, or shearing, cropping and finishing cloth, &c.—Dated the 26th of April 1828.—2 months allowed to enrol specification.

To T. Breiderback, of Birmingham, for a machine or improved mode for forming tubes or rods, &c.—26th of April.—4 months.

To J. Griffen, of Withy Moor Works, near Dudley, for an improvement in the manufacturing of scythe backs, chaff-knife backs, and hay-knife backs.—26th of April.—6 months.

To J. J. Watt, of Stracey-street, Stepney, for his discovery of the application of a certain chemical agent by which animal poison may be destroyed and the disease consequent thereon effectually prevented.—26th of April.—6 months.

To C. C. Bompas, of the Inner Temple, Esq., for his improvements in the propelling of locomotive carriages and machines, and boats and other vessels.—29th of April.—6 months.

To T. Hillman, of Mill-wall, Poplar, for improvements in the construction and fastening of masts.—1st of May.—6 months.

To J. Brownill, of Sheffield, for his improved method of transferring vessels from a higher to a lower level, or from a lower to a higher level on canals, and also for the more conveniently raising or lowering of weights, carriages, or goods, on rail-roads, &c.—1st of May.—6 months.

To J. Palmer, of Globe-road, Mile-End, for improvements in the moulds, machinery or apparatus, for making paper.—6th of May—6 months.

To T. Adams, of Oldbury, Salop, for improvements on trusses, or apparatus for the relief or cure of rupture.—6th of May.—6 months.

To F. Westby, of Leicester, for his apparatus to be used for the purpose of whetting or sharpening the edges of the blades of knives, &c.—6th of May.—2 months.

To Rear Admiral Brooking, of Plymouth, for a turning or shipping fid for securing and releasing the upper masts of ships and vessels.—6th of May.—6 months.

To M. Fulwood, of Stratford, Essex, for a cement, mastic or composition, denominated German Cement.—6th of May.—2 months.

To J. B. Macneil, of Foleshill, Coventry, for improvements in preparing and applying materials for constructing or rendering more durable roads, which materials are applicable to other purposes.—6th of May.—6 months.

To T. Jackson, of Red-Lion-street, Holborn, for a new metal stud to be applied to boots, shoes, and other like articles of manufacture.—13th of May.—6 months.

To J. Ford, of Wandsworth-road, Vauxhall, for improvements in machinery for clearing, opening, scribbling, carding, combing, slubbing and spinning wool, and for carding, roving, or shivering and spinning cotton, short-stapled flax, hemp and silk, either separately or combined, and for spinning or twisting long-stapled flax, hemp, silk, mohair, &c. and either separately or combined.—13th of May.—6 mo.

To T. Bonsar Crompton, of Tamworth, in Lancashire, and E. Taylor, of Marsden in Yorkshire, for improvements in the process of paper-making which relates to the cutting.—13th of May.—2 months.

To C. Chubb, of St. Paul's Churchyard, London, for improvements in the construction of door-latches.—17th of May.—6 months.

To T. W. and J. Powell, of Bristol, for improvements in the process of forming moulds for refining sugar, and in the application of materials hitherto unused in making the said moulds.—17th of May.—2 months.

To T. Aspinwall, of Bishopsgate Churchyard, London, Esq., for an improved method of casting printing types by means of a mechanical process. Communicated from abroad. 22nd of May.—6 months.

To S. Hall, of Basford, Nottinghamshire, for an apparatus for generating steam and various gases to produce motive power, and for other useful purposes.—31st of May.—6 months.

To J. Moffat, of King's Arms-yard, Coleman-street, London, for an improvement in apparatus for stoppering and securing chain cables;

bles; also for weighing anchors attached to such chain or other cables, either with or without a messenger.—3rd of June.—6 months.

To D. Jobbins, of Uley, Gloucestershire, for an improved method, by certain machinery applicable to stocks or fulling-machines, of milling and scowering woollen cloths, &c.—3rd June.—2 months.

To Baron Charles Wettersted, of Commercial-place, Commercial-road, for a liquid or composition for water-proofing and strengthening leather.—4th of June.—6 months.

To R. Wilty, of Hauley, Staffordshire, for improvements in apparatus for making coal-gas.—10th of June.—6 months.

To E. G. Atherley, of York-place, Portman-square, for an apparatus for a method of generating power.—12th of June.—6 months.

To W. Strachan, of Avon Eitha, Ruabon, Denbighshire, for an improvement in the making of alum.—12th of June.—4 months.

To J. Bartlett, of Chard, Somersetshire, for a new method of preparing flax-thread or yarn for use in the manufacture of boots, shoes, sadlery; and of sail and of other cloths and bagging.—16th of June. —2 months.

To G. J. Young, of Newcastle-upon-Tyne, for a machine whereby an additional and improved purchase or power will be given in working ships, windlasses, and capstans.—21st of June.—6 months.

METEOROLOGICAL OBSERVATIONS FOR MAY 1828.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·32 May 1. Wind NE.—Min. 29·36 May 24. Wind S.
Range of the index 0·96.
Mean barometrical pressure for the month .... 29·826
Spaces described by the rising and falling of the mercury............ 4·120
Greatest variation in 24 hours 0·440.—Number of changes 18.
Therm. Max. 76° May 16. Wind S.—Min. 42° May 8. Wind N.E.
Range 34°.—Mean temp. of exter. air 58°·76. For 31 days with ⊙ in ♉ 56·42
Max. var. in 24 hours 24°·00—Mean temp. of spring water at 8 A.M. 51°·29

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 11th............... 84°
Greatest dryness of the air in the afternoon of the 15th 40
Range of the index.. 44
Mean at 2 P.M. 53°·0—Mean at 8 A.M. 57°·0—Mean at 8 P.M. 64·9
—— of three observations each day at 8, 2, and 8 o'clock......... 58·3
Evaporation for the month 3·95 inches.
Rain near ground 2·29 inches.
Prevailing wind, S.E.

Summary of the Weather.

A clear sky, 5; fine, with various modifications of clouds, 12½; an overcast sky without rain, 8; rain, 5½.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus
23	20	27	0	27	24	21

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
1	5	4	7	2½	5½	3	3	31

General Observations.—The state of the weather this month has been changeable, except in the second week, and showery, with several frosty mornings, and intervals of hot sunshine; but upon the whole it has been a fine growing month for the corn and vegetation, with mild nights in general. The hoar frosts in the mornings of the 6th, 7th, 9th and 10th, and the cold blighting winds on several subsequent days, have much injured the late bloom of the wall and other fruit trees, and caused a great part of the fruit that was set to fall off. The vines will not be so prolific this year as they have been for two or three years past. The grass fields in this neighbourhood have been much improved by the recent showers, and from their fine appearance a good crop may be expected: the grass in several fields is already cut.

The chaffers were first observed here in the evening of the 6th, and have been unusually numerous, having been seen on the wing every fine day since. On the 14th, a quarter of an hour before sunset, a large meteor was observed in the N.W. at an altitude of about 40 degrees. Its light was vivid, and its descent rapid and nearly perpendicular; the disjoined parts continued luminous several seconds of time after its explosion. Its appearance so early was remarkable, as meteors are very seldom seen till after the evening twilight.

In the night of the 15th there was thunder, and sheet-lightning the following night for several hours, after a very warm day: thunder and lightning also occurred in the evening of the 23rd.

The mean temperature of the external air this month is more than three degrees higher than the mean of May for the last twelve years.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are one lunar and two solar halos, three meteors, thunder and lightning twice; and six gales of wind, or days on which they have prevailed; namely, one from the North-east, three from the South-east, one from the South, and one from the West.

REMARKS.

London.—May 1. Very fine. 2. Fine: slight rain at night. 3. Drizzly: Cloudy. 4. Fine: rain at night. 5. Cloudy. 6. Sultry: with thunder. 7. Fine: drizzly at night. 8. Fine. 9. Showery. 10. Fine. 11. Cloudy. 12—15. Very fine. 16. Sultry: much lightning at night. 17—20. Very fine. 21. Cloudy: with showers. 22. Cloudy morning: fine. 23. Very fine. 24. Heavy rain in morning: showery. 25. Fine. 26. Showery. 27. Fine morning: showery. 28. Fine. 29. Showery. 30. Very fine. 31. Fine.

Boston.—May 1, 2. Fine. 3. Cloudy. 4. Cloudy: rain P.M. 5. Rain. 6. Fine. 7. Cloudy. 8, 9. Fine. 10, 11. Cloudy. 12—15. Fine. 16. Cloudy. 17—19. Fine. 20. Cloudy. 21. Fine. 22, 23. Cloudy. 24. Rain. 25, 26. Cloudy. 27, 28. Fine. 29, Rain. 30, 31. Fine.

Penzance.— May 1. Clear. 2. Fair. 3. Fair: a shower. 4. Clear: showers. 5—10. Clear. 11. Fair: clear. 12, 13. Clear. 14. Clear: fair. 15. Clear: rain. 16. Fair: rain at night. 17. Rain: clear. 18. Clear: fair. 19. Rain. 20. Cloudy: rain. 21. Rain: showers. 22, 23. Fair: showers. 24. Rain: blowing strong. 25. Fair. 26. Rain: showers. 27, 28. Fair: showers. 29. Clear. 30. Clear: fair. 31. Rain: clear.—Rain-gauge ground level.

Meteorological Observations made at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY *at Penzance, Dr.* BURNEY *at Gosport, and Mr.* VEALL *at Boston.*

Days of Month, 1828.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.									
May 1	30·340	30·297	30·25	30·20	30·32	30·25	29·75	66	38	60	50	66	47	54	SE.	SE.	NE.	calm	...	...	...	...	...
2	30·263	30·142	30·02	30·00	30·19	30·07	29·74	62	47	60	49	64	52	54·5	E.	SE.	E.	E.	...	·05	...	0·045	...
3	30·024	29·997	30·02	30·00	29·94	29·92	29·60	61	49	60	50	60	52	54	SE.	NW.	SE.	E.	0·35	·03	...	·040	...
4	29·791	29·754	29·94	29·93	29·76	29·74	29·33	64	40	55	49	62	47	55·5	S.	NW.	NW.	calm	...	·13	0·060	...	...
5	29·740	29·702	29·90	29·86	29·70	29·68	29·20	61	35	55	46	60	45	51	N.	NW.	NW.	calm	...	·02	0·080	·010	0·30
☾ 6	29·848	29·742	29·80	29·80	29·76	29·70	29·22	63	36	56	46	66	43	56	SW.	NW.	SW.	calm	·40	...	...	...	·26
7	29·914	29·847	29·84	29·80	29·84	29·80	29·32	64	46	58	44	63	48	50	N.	N.	NE.	NW.	...	·05	...	·020	...
8	29·990	29·912	30·00	29·90	29·90	29·82	29·44	61	35	57	48	61	42	53	NW.	N.	NE.	NE.	...	...	...	...	...
9	30·168	30·075	30·25	30·20	30·12	30·03	29·57	60	36	58	46	65	44	54	W.	N.	SW.	NW.	·40	·04	...	...	...
10	30·191	30·132	30·25	30·24	30·17	30·10	29·60	64	48	56	48	68	48	53·5	W.	NW.	W.	W.	...	...	...	...	...
11	30·110	30·084	30·25	30·24	30·08	30·06	29·50	65	41	62	51	68	48	58	N.	NW.	W.	W.	...	...	...	...	...
12	30·302	30·239	30·26	30·26	30·25	30·20	29·66	67	38	64	50	63	46	57	NE.	NE.	SE.	NW.	·40	...	...	...	...
● 13	30·318	30·225	30·28	30·25	30·28	30·18	29·78	72	42	66	48	59	47	57·5	NE.	NE.	SE.	W.	...	...	...	...	...
14	30·203	30·138	30·18	30·10	30·14	30·04	29·65	71	44	65	49	64	52	61	E.	SE.	E.	NW.	...	...	...	...	...
15	30·072	29·996	29·96	29·94	29·99	29·92	29·61	69	54	66	55	74	58	57	E.	SW.	SE.	E.	·65	...	...	...	...
16	29·901	29·856	29·96	29·94	29·83	29·77	29·45	71	52	65	55	76	58	56·5	E.	NW.	N.	E.	...	...	0·665	·050	...
17	29·860	29·814	29·75	29·75	29·77	29·74	29·40	70	45	61	54	69	56	60	E.	NW.	SE.	E.	...	...	0·560	·170	...
18	29·847	29·823	29·75	29·75	29·72	29·72	29·36	66	42	56	51	71	50	59	NE.	SE.	NE.	NE.	·35	...	...	...	...
19	29·836	29·818	29·75	29·75	29·76	29·76	29·34	67	42	59	52	65	49	62	NE.	SE.	E.	NE.	...	...	0·400	...	...
20	29·786	29·715	29·73	29·60	29·70	29·64	29·32	65	47	65	52	67	55	55·5	E.	SE.	E.	NE.	...	·03	0·100	·210	...
☽ 21	29·584	29·569	29·50	29·48	29·54	29·51	29·20	65	49	64	53	69	55	58	E.	S.	SE.	NE.	·35	·03	0·125	·040	...
22	29·595	29·587	29·46	29·45	29·51	29·51	29·13	61	48	66	54	69	54	57	E.	SE.	SE.	NE.	...	...	0·050	·160	...
23	29·602	29·592	29·50	29·35	29·56	29·53	29·11	66	52	62	55	71	56	56	E.	SE.	SE.	NE.	...	...	0·205	·140	...
24	29·520	29·440	29·35	29·15	29·43	29·36	28·93	67	53	58	53	67	54	56·5	S.	SW.	S.	NE.	·30	·34	0·425	·260	·31
25	29·844	29·697	29·65	29·65	29·81	29·72	29·04	66	47	62	50	68	54	60·5	SW.	SW.	SW.	W.	...	...	0·340	·080	·23
26	29·735	29·624	29·65	29·60	29·67	29·57	29·24	70	47	63	54	64	53	59	S.	SW.	SE.	E.	...	·13	0·075	·090	·04
27	29·594	29·545	29·40	29·35	29·54	29·47	29·00	68	57	60	50	65	52	63	S.	SW.	S.	calm	·25	·34	0·305	·840	...
28	29·673	29·608	29·55	29·55	29·63	29·55	28·90	67	54	60	48	65	55	62·5	S.	SW.	SW.	calm	...	...	0·155	·090	·18
○ 29	29·762	29·708	29·76	29·63	29·73	29·64	29·10	67	50	63	53	73	52	62	SW.	NW.	SW.	W.	...	.12	0·115	...	·06
30	29·957	29·920	29·94	29·94	29·95	29·90	29·25	75	52	65	53	72	54	62	S.	NW.	SW.	SW.	...	·09	...	·040	·26
31	29·933	29·860	29·96	29·90	29·92	29·84	29·23	69	50	62	54	70	53	60	SW.	NW.	W.	W.	·50	...	0·105	·005	·04
Aver. :	29·913	29·853	30 28	29·15	30·32	29·36	29·35	66	45	66	44	76	42	57·2					3·95	1·40	3·765	2·290	1·68

THE

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ANNALS OF PHILOSOPHY.

[NEW SERIES.]

AUGUST 1828.

XII. *On Websterite found in the Plastic Clay of Auteuil, near Paris. By* M. ALEXANDRE BRONGNIART, *Member of the Royal Academy of Sciences, and Professor of Mineralogy at the Jardin du Roi, Paris.**

THE occurrence of the same geognostic circumstances, in districts considered to be of the same formation though situated very remotely from each other, exhibiting even in the beds least developed, a repetition of the most minute particulars,—presents a phænomenon that cannot but attract the attention of naturalists; and appears to point out, that causes, simple, but powerful and general, have concurred in producing the several strata deposited in each successive epoch.

These reflections have followed from a discovery I have made in the environs of Paris, of a substance which, in itself, is comparatively of little importance: but it is precisely because the beds in which it is found are so feebly and irregularly developed as scarcely to be entitled to rank as a formation, and because this substance, occurring in small nodules, is possessed of characters not very important when taken separately, that we are struck with its appearance in so many places situated widely apart.

We are not surprised at finding granite of similar composition in Europe, Asia, and America: but it is more singular to observe Websterite always in the same formation in Germany, England, in several parts of France, and even at the gates of Paris.

The mineral substance which forms the subject of the present communication, is the subsulphate of alumina, which was found first at Halle in Saxony, and which had been long known by the erroneous appellation of *native alumina;* afterwards by that of *aluminite* (already appropriated to aluminous schist), and which I have in another place* named *Websterite*, dedicating it to Mr. Webster of London †, who first discovered this substance at Newhaven, near Brighton.

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* Extracted from the "*Annales des Sciences Naturelles*," for March 1828.

The history of this mineral, independently of the geological circumstances connected with it, is very remarkable‡. It was at first, and for a long time, taken for pure alumina; though, as it was difficult to imagine how such a substance could exist in a state of perfect purity in the midst of very recent beds, its origin was rather attributed to some of the processes of a manufacture which formerly existed in the spot where it was found. In fact, its aspect, and its form (that of nodules about the size of nuts), its position so near the surface of the earth, and its being the only example which had till then occurred of this substance; lastly, the complete ignorance in which we were of the date of the stratum in which it had been met with,—all contributed to excite the idea that it was merely a product of art accidentally buried in the loose soil.

Afterwards its properties were more carefully examined: first Mr. Schreiber observed its crystalline structure, which is not visible but by the microscope §; then MM. Simon, of Berlin, and Bucholz detected the presence of sulphuric acid. M. Chenevix supposed that it was a sulphate of alumina having the base in excess; and finally, M. Stromeyer proved that it was a subsulphate of alumina in definite proportions, containing 47 per cent of water, or a combination of one atom of alumina, one atom of sulphuric acid, and 9 atoms of water. $Al.S + 9Aq.$

Mr. Webster having discovered a similar mineral at Newhaven ||, M. Stromeyer determined its composition to be the same as that found at Halle: afterwards M. de Basterot meeting with a white

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* See the Supplement to Vol. iii. of the *Dictionnaire des Sciences Naturelle*, article *Argile Native*.

† Late Secretary and Curator of the Museum of the Geological Society of London, and now Professional Geologist and Lecturer on Geology.

‡ This history was given much in detail by Keferstein (*Leonh. tasch.*, 10th year 1816, p. 33); and M. Bonnard has inserted an abridgement of it in the *Annales des Mines*, 1821, tom. vi. p. 588.

§ I have verified this observation, and have described it in my *Traité élémentaire de Minéralogie*, published in 1807, t. i. p. 515; M. Keferstein repeated it, and rendered the results more interesting by comparing them with the microscopic appearance of other earthy substances.

|| In the enumeration of facts respecting the history of this mineral substance, it may be proper to observe, that it was in 1812 that Mr. Webster found it at Newhaven: shortly afterwards it was determined by Dr. Wollaston and Mr. Tennant to be a subsulphate of alumina; and the latter gentleman

a white earthy substance at Bernon, near to Epernay, in the lignite of that canton, M. Lassaigne analysed it, and found it to be also a subsulphate of alumina, but differing somewhat in the proportion of its constituents, owing probably to some impurities; for it is proper to state that Websterite appears always as a white friable earth, and that it is difficult to detach it entirely from the clayey matrix which surrounds it.

But it is a singularity in this mineral, which I have constantly observed in the three examples just mentioned, that it is composed of an infinity of minute acicular crystals, so small that they cannot be seen without a microscope with a magnifying power of at least 400 times: then the crystals are very distinct. M. Schreiber had remarked them in the Websterite of Halle: I found them in that of Newhaven and Epernay; and by means of the fine microscope of Amici, I have been enabled to determine that they consist of six-sided compressed prisms terminated by two culminating facets, consequently having a form incompatible with that of alum.

The three examples of Websterite found in places very distant from each other, possess therefore the two classes of characters which essentially constitute mineral species, particular composition and form. Let us now examine their geological situation; and this will not be a useless repetition, since we may thus avoid describing the same circumstances in detail, when speaking of the Websterite of Auteuil.

The Websterite of Newhaven has shown clearly the geognostic position of this mineral: it is there in nodules of from one inch to two or three inches in diameter, imbedded in an ochrey clay mixed with gypsum, which is placed upon the chalk, and which penetrates in irregular veins the superior and disintegrated part of the rock. That of Bernon, near Epernay, found by M. Basterot, occurs also in veins and nodules, in the plastic clay above the chalk, accompanied by lignite and gypsum.

If, after having acquired these ideas respecting the position of Websterite in two points more than 100 leagues asunder, we should extend our observation to Halle in Saxony, 200 leagues further, we shall there find, instead a supposed ordinary alluvial soil, the plastic clay with its gypsum, its lignites, its amber, and its Websterite disseminated in nodules through the for-

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gentleman inserted a notice respecting it in the *Journal de Physique*, for September 1814. Previous to that time all the knowledge respecting it in England, seemed to be confined to the notice of the substance found at Halle, and which was described in our elementary books as native alumine. —Edit.

mation. At Morl also, not far from the last-mentioned spot, we meet with a similar substance, determined by M. Stromeyer to contain the same principles as that of Halle.

We have next to consider a new variety of this mineral, which is met with at Auteuil, and which is properly the subject of this memoir.

The chalk which supports all the superior sedimentary strata in the basin of Paris is seen uncovered at Meudon, but does not appear on the right bank of the Seine; it is however very near the surface under the hills called Point-du-Jour; and also near Auteuil it carries the plastic clay, which covers it in many points. Indeed, this clay is worked at the foot of the village of Auteuil, in a place called la Glacière, and is used for making bricks and other purposes. It was in this spot, or very near it, that M. Becquerel found in the plastic clay, lignite, pyrites, sulphate of strontian, phosphate of lime, and even a little blende: there also are procured the large well-defined and clear crystals of gypsum, so much prized by amateurs of fine minerals. This locality, in the part where I have examined it, exhibits, immediately under the vegetable soil, a plastic clay yellowish and very sandy, having little tenacity, traversed by veins of yellowish clay still more sandy, ochrey, and divided into many small portions. Beneath, is a bluecish plastic clay that is more tenacious, and contains a good deal of pyrites and gypsum. Still lower, is a bed of sand, or rather yellow ferruginous coarse gravel; and below that another bed of clay. In this upper yellowish clay we find, and rather in nodules than in veins, some parts that are whitish and friable, composed of a multitude of small roundish grains, closely connected together, yet admitting a greyish clay in the interstices. When these little masses are cut across, they present the aspect of an oolite consisting of white grains in a greyish paste or cement. This is Websterite. Each grain, when closely examined, appears to be a little spheroid of a structure indistinctly radiated. When crushed, the powder is very brilliant and soft to the touch; and when examined by the microscope, exhibits wedge-shaped masses composed of prisms, but so ill-determined, that it is necessary to be aware of their crystalline structure to recognize them.

It is from these characters that I conjectured this white oolitic substance to be Websterite; but the complete chemical analysis made by M. Dumas leaves no doubt as to its nature. It does not effervesce with nitric acid, which proves that the argillaceous interposed part is not marl, but plastic clay. When heated in a glass tube, water rises at first; but when the tube becomes red hot, sulphurous acid is disengaged in great abundance. Treated by the blowpipe with nitrate of cobalt, it assumes the fine blue colour which denotes alumina.

It dissolves almost entirely in caustic potash; and this solution affords by nitric acid a precipitate which is redissolved by an excess of acid. This last solution is precipitated by ammonia and by barytes.

These experiments sufficiently prove the presence of water, alumina, and sulphuric acid, and also the absence of silica.

The complete analysis by M. J. Dumas gives

Sulphuric acid	23
Alumina	30
Water	47

and this result agrees in the constituent principles and their proportions with those of the Websterite of Halle and Newhaven.

Thus, we perceive, as I have stated in the commencement of this notice, that this friable substance, which has more the appearance of an adventitious earthy mixture than of a mineral species, presents in its composition, an identity of principles, together with a precision in their proportions, rarely found in crystallized minerals, which indicate, by their solidity and limpidity, species completely limited. We see it also placed in geological positions and circumstances of which the constancy is no less striking. There is, however, between the Websterite of Autueil, and that of other localities, a slight difference of structure, which may serve to establish a variety in this species. It has the oolitic structure; and we may therefore distinguish it by the name of *Oolitic Websterite of Auteuil.*

XIII. *On the Phænomena of Volcanoes. By Sir* Humphry Davy, *Bart. F.R.S.*[*]

When in the years 1807 and 1808 I discovered that the alkalies and the earths were composed of inflammable matter united to oxygen, a number of inquiries suggested themselves with respect to various parts of chemical science, some of which were capable of being immediately assisted by experiment, and others required for their solution a long series of observations, and circumstances obtained only with difficulty. Of the last kind were the inferences concerning the geological appearances connected with these discoveries.

The metals of the alkalies, and those of such of the earths as I had decomposed, were found to be highly combustible, and altered by air and water even at the usual temperatures of the atmosphere; it was not possible, consequently, that they should be found at the surface of the globe, but probable that they might exist in the interior: and allowing this hypothesis, it became easy to account for volcanic fires, by exposure of the metals of earths and alkalies to air and water; and to explain, not only the formation of lavas, but likewise that of basalts and many other crystalline rocks, from the slow cooling of the products of combustion or oxidation of the newly-discovered substances.

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[*] From the Philosophical Transactions, for 1828. Part I.

I developed this opinion in a paper on the decomposition of the earths, published in 1808; and since 1812 I have endeavoured to gain evidence respecting it by examining volcanic phænomena of ancient and recent occurrence in various parts of Europe.

In this communication I shall have the honour of laying before the Royal Society some results of my inquiries. If they do not solve the problem respecting the cause of volcanic fires, they will, I trust, be found to offer some elucidations of the subject, and may serve as the foundation of future labours.

The active volcano on which I have made my observations is Vesuvius; and there probably does not exist another so admirably fitted for the purpose: its vicinity to a great city; the facility with which it may be ascended in every season of the year; and the nature of its activity,—all offer peculiar advantages to the philosophic inquirer.

I had made several observations on Vesuvius in the springs of 1814 and 1815, which I shall refer to on a future occasion in these pages; but it was in December 1819, and January and February 1820, that the volcano offered the most favourable opportunity for investigation. On my arrival at Naples, December 4, I found that there had been a small eruption a few days before, and that a stream of lava was flowing with considerable activity from an aperture in the mountain a little below the crater. On the 5th I ascended the mountain, and examined the crater and the stream of lava. The crater emitted so large a quantity of smoke, with muriatic and sulphurous acid fumes, that it was impossible to approach it except in the direction of the wind; and it threw up every two or three minutes showers of red hot stones. The lava was flowing from an aperture about one hundred yards below it, being apparently forced out by elastic fluids with a noise like that made by the steam disengaged from a pressure engine; it rose, perfectly fluid, forming a stream of from five to six feet in diameter, and immediately fell, as a cataract, into a chasm about forty feet below, where it was lost under a kind of bridge formed of cooled lava; but it re-appeared sixty or seventy yards further down. Where it issued from the mountain, it was nearly white hot, and exhibited an appearance similar to that which is shown when a pole of wood is introduced into the melted copper of a foundry, its surface appearing in violent agitation, large bubbles rising, which in bursting produced a white smoke; but the lava became of a red colour, though still visible in the sunshine, where it issued from under the bridge. The force with which it flowed was so great, that the strength of the guide, a very stout young man, was insufficient to keep a long iron rod in the current. The whole of its course, with two or three interruptions where it flowed under a cooled surface, was nearly three quarters of a mile, and it threw off clouds of a white smoke. It smoked less as it cooled and became pasty; but even where it terminated in moving masses of scoria, smoke was still visible, which became more distinct whenever the scoria was moved, or the red hot lava in the interior exposed.

Having ascertained that it was possible to approach within four or five feet of the lava, and to examine the vapour immediately close to the aperture, I returned the next day, having provided the means of making a number of experiments on the nature of the lava, and of the elastic fluids with which it was accompanied. I found the aperture nearly in the same state as the day before, but the lava spread over a larger surface, forming an eddy in the hollow of the rock, over which it fell, from which it could be raised in an iron ladle more easily than from the current, and where there was much more facility of placing and withdrawing substances intended to be exposed to its agency.

One of the most important points to be ascertained was, whether any combustion was going on at the moment the lava issued from the mountain. There was certainly no appearance of more vivid ignition when it was exposed to air, nor did it glow with more intensity when it was raised into the air by an iron ladle. I put the circumstance, however, beyond the possibility of doubt: I threw some of the fused lava into a glass bottle furnished with a ground stopper, containing siliceous sand in the bottom: I closed it at the moment, and examined the air on my return. A measure of it mixed with a measure of nitrous gas gave exactly the same degree of diminution as a measure of common air which had been collected in another bottle on the mountain.

I threw upon the surface of the lava nitre, both in mass and in powder. After this salt had fused, there was a little increase of vividness in the ignition of the lava, but much too slight to be referred to pure combustible matter in any quantity; and on making the experiment on a portion of lava taken up in the ladle, it appeared that the disengagement of heat was partly owing to the peroxidation of the protoxide of iron, and to the combination of the alkali of the nitre with the earthy basis of the lava; for where the nitre had melted, the colour had changed from olive to brown. This conclusion was still further proved by the circumstance that chlorate of potash thrown upon the lava did not increase its degree of ignition so much as nitre. When a stick of wood was introduced into a portion of the lava so as to leave a little carbonaceous matter on its surface, nitre or chlorate of potassa then thrown upon it caused it to glow with great brilliancy. Some fused lava was thrown into water, and a glass bottle filled with water held over it to collect the gas disengaged; it was in very minute quantity only, and when analysed on my return proved to be common air a little less pure than that disengaged from the water by boiling. A wire of copper of $\frac{1}{20}$th of an inch in diameter, and a wire of silver of $\frac{1}{30}$th, introduced into the lava near its source, were instantly fused: an iron rod of $\frac{1}{5}$th of an inch, with a piece of iron wire of about $\frac{1}{30}$th, were kept for five minutes in the eddy in the stream of lava; they were not fused; they did not produce any smell of sulphuretted hydrogen when acted on by muriatic acid. A tin-plate funnel filled with cold water was held in the fumes disengaged with so much violence from the aperture through which the lava issued: fluid was immediately condensed upon it, which was of an acid and subastringent taste. It did not precipitate muriate of baryta; but copiously precipitated nitrate of silver, and rendered the triple prussiate of potassa of a bright blue. When the same funnel was held in the white fumes above the lava where it entered the bridge, no fluid was precipitated upon it, but it became coated with a white powder which had the taste and chemical qualities of common salt, and proved to be this substance absolutely pure. A bottle of water holding about $\frac{3}{4}$ of a pint, with a long narrow neck, was emptied immediately in the aperture from which the vapours pressing out the lava issued, and the neck was immediately closed. This air examined on my return was found to give no absorption with solution of potassa; so that it contained no notable proportion of carbonic acid, and it consisted of 9 parts of oxygen and 91 of azote. There was not the least smell of sulphurous acid in the vapour from the aperture, nor were the fumes of muriatic acid so strong as to be unpleasant; but during the last quarter of an hour that I was engaged in these experiments, the wind changed, and blew the smoke from the crater upon the spot where I was standing: the sulphurous acid gas in the fumes was highly irritating to the organs of respiration, and I suffered so much from the exposure to them that I was obliged to descend; and the effect was not transient, for a violent catarrhal affection ensued, which prevented me for a month from again ascending the mountain.

On the 6th of January I made another visit to Vesuvius. I found the appearance of the lava considerably changed; the bocca from which it issued on the 5th of December was closed, and the current now flowed quietly and without noise from a chasm in the cooled lava about three hundred feet lower down. The heat was evidently less intense. I repeated my experiments with nitre with the same results, and exposed pure silver and platinum to the fused lava: they were not at all changed in colour. I collected the sublimations from various parts of the cooled lava above. The rocks near the ancient bocca were entirely covered with white, yellow, and reddish saline substances. I found one specimen of large saline crystals in a cavity, which had a slight tint of purple: this examined, proved to be common salt with a minute portion of muriate of cobalt. The other sublimations consisted of common salt in great excess, much chloride of iron, some sulphate of soda; and by the test of muriate of platinum, there appeared to exist in them a small quantity of sulphate or muriate of potassa; and a solution of ammonia detected the presence of a minute quantity of the oxide of copper.

During the months of January and February I made several visits to the top of Vesuvius: I shall not particularize them all; but shall mention only such as afforded me some new observations. On the 26th of January, the lava was seen nearly white hot through a chasm near the place where it flowed from the mountain. I threw nitre upon it in large quantities through this chasm, in the presence of H. R. H. the Prince of Denmark, whom I had the honour of accompanying in this excursion to the mountain, and my friend the Cavaliere Monticelli: there was no more increase of ignition than when the experiment was made on lava exposed to the free air. The appearance of the sublimations was now considerably changed: those near the aperture were coloured green and blue by salts of copper; but there was still a great quantity of muriate of iron. I have mentioned, that on the 5th the sublimate of the lava was pure chloride of sodium: in the sublimate of January 6th, there were both sulphate of soda and indications of sulphate of potassa. In the sublimates that I collected on the 26th, the sulphate of soda was in much larger quantities, and there was much more of a salt of potassa. From the 5th of December to the 20th of February, the lava flowed in larger or smaller quantities, so that at night a stream of ignited matter was always visible, more or less interrupted by cooled lava. It changed its direction according to the obstacles it met with; and never, according to appearances, extended so much as a mile from its source. During the whole of this time the craters, of which there were two, were in activity. The large crater threw up showers of ignited ashes and stones to a height apparently of from 200 to 500 feet; and from a smaller crater, to the right of the large one on the side of Naples, steam arose with great violence. Whenever the crater could be approached it was found incrusted with saline incrustations; and the walk to the edge of the small crater on the 6th of January was through a mass of loose saline matter, principally common salt coloured by muriate of iron, in which the foot sunk to some depth. It was easy, even at a great distance, to distinguish between the steam disengaged by one of the craters, and the earthy matter thrown up by the other. The steam appeared white in the day, and formed perfectly white clouds, which reflected the morning and evening light of the purest tints of red and orange. The earthy matter always appeared as a black smoke, forming black clouds; and in the night it was highly luminous at the moment of the explosion.

On the 20th of February, the small crater which had been disengaging steam and elastic matter, began to throw out showers of stones; and both craters from the 20th to the 23rd were more than usually active. On the night of the 23rd, at half past 11 o'clock, being in my bed-room at Chiatimone, Naples, I heard the windows shake; and going to the window, I saw ascending from Vesuvius a column of ignited matter to a height at least equal to that of the mountain from its base; and the whole horizon was illuminated, notwithstanding the brightness of the moon, with direct volcanic light, and that reflected from the clouds above the column of ignited matter. Several eruptions of the same kind, but upon a smaller scale, followed at intervals of a minute and a half or two minutes; but there were no more symptoms of earthquake, nor did I hear any noise. On observing the lava, it appeared at its origin much broader and more vivid; and it was evident that a fresh stream had broken out to the right of the former one. On the morning of the 24th I visited the mountain; it was not possible to ascend to the top, which was covered with clouds, nor to examine the orifice from which the lava issued. The stream of lava near the place where it terminated was from 50 to 100 feet broad. It had precisely the same appearances as the lava which had been so long running. I collected the saline matter condensed upon some of the masses of scoria which were carried along by the current and deposited on the edge of the stream; they proved to be the same in the nature of their constituent parts as those of the lava of the 26th of January, but with a larger proportion of sulphate of soda, and a smaller proportion of muriate of iron; and I have no doubt that the dense white smoke which was emitted in immense columns by the lava during the whole of its course, was produced by the same substances.

I shall now mention the state of the volcano at some other periods.

When I was at Naples in May 1814, the crater had the appearance of an immense funnel, closed at the bottom, with many small apertures emitting steam; and on the side towards Torre del Greco, there was a large aperture from which flame issued to a height of at least 60 yards, producing a most violent hissing noise. This phænomenon was constant during the three weeks I remained at Naples. It was impossible to approach sufficiently near the flame to ascertain the results of the combustion; but a considerable quantity of steam ascended from it. When the wind blew the vapours upon us, there was a distinct smell both of sulphurous and muriatic acids. There was no indication of carbonaceous matter from the colour of the smoke; nor was any deposited upon the yellow and white saline matter which surrounded the crater, and which I found to be principally sulphate and muriate of soda, and muriate of iron: in some specimens there was a considerable quantity of muriate of ammonia.

In March 1815, the appearances presented by the crater were entirely different. There was no aperture in the crater; it was often quiet for minutes together, and then burst out into explosions with considerable violence, sending fluid lava and ignited stones and ashes to a considerable height, many hundred feet, in the air.

These eruptions were preceded by subterraneous thunder, which appeared to come from a great distance, and which sometimes lasted for a minute. During the four times that I was upon the crater in the month of March, I had at last learnt to estimate the violence of the eruption from the nature of the sound: loud and long continued subterraneous thunder indicated a considerable explosion. Before the eruption the crater appeared perfectly tranquil; and the bottom, apparently without an aperture, was covered with ashes. Soon, indistinct rumbling sounds were heard as if at a great distance; gradually the sound approached nearer, and was like the noise of artillery fired under our feet. The ashes then began to rise and to be thrown out with smoke from the bottom of the crater; and lastly, the lava and ignited matter was ejected with a most violent explosion. I need not say that when I was standing on the edge of the crater witnessing this phænomenon, the wind was blowing strongly from me: without this circumstance it would have been dangerous to have stood on the edge of the crater; and whenever from the loudness of the thunder the eruption promised to be violent, I always ran as far as possible from the seat of danger.

As soon as the eruption had taken place, the ashes and stones which rolled down the crater seemed to fill up the aperture, so that it appeared as if the ignited and elastic matter were discharged laterally; and the interior of the crater assumed the same appearance as before.

I shall now offer some observations on the theory of these phænomena. It appears almost demonstrable that none of the chemical causes anciently assigned for volcanic fires can be true. Amongst these, the combustion of mineral coal is one of the most current; but it seems wholly inadequate to account for the phænomena. However large a stratum of pit-coal, its combustion under the surface could never produce violent and extensive heat; for the production of carbonic acid gas, when there was no free circulation of air, must tend constantly to impede the process: and it is scarcely possible that carbonaceous matter, if such a cause existed, should not be found in the lava, and be disengaged with the saline or aqueous products from the bocca or craters. There are many instances in England of strata of mineral coal which have been long burning; but the results have been merely baked clay and schists, and it has produced no result similar to lava.

If the idea of Lemery were correct, that the action of sulphur on iron may be a cause of volcanic fires, sulphate of iron ought to be the great product of the volcano; which is known not to be the case; and the heat produced by the action of sulphur on the common metals, is quite inadequate to account for the appearances. When it is considered that volcanic fires occur and intermit with all the phænomena that indicate intense chemical action, it seems not unreasonable to refer them to chemical causes. But for phænomena upon such a scale, an immense mass of matter must be in activity, and the products of the volcano ought to give an idea of the nature of the substances primarily active. Now what are these products? Mixtures of the earths in an oxidated and fused state, and intensely ignited; water and saline substances, such as might be furnished by the sea and air, altered in such a manner as might be expected from the formation of fixed oxidated matter. But it may be said, if the oxidation of the metals of the earths be the causes of the phænomena, some of these substances ought occasionally to be found in the lava, or the combustion ought to be increased at the moment the materials passed into the atmosphere. But the reply to this objection is, that it is evident that the changes which occasion volcanic fires, take place in immense subterranean cavities; and that the access of air to the acting substances occurs long before they reach the exterior surface.

There is no question but that the ground under the solfaterra is hollow, and there is scarcely any reason to doubt of a subterraneous communication between this crater and that of Vesuvius: whenever Vesuvius is in an active state, the solfaterra is comparatively tranquil. I examined the bocca of the solfaterra on the 21st of February 1820, two days before the activity of Vesuvius was at its height: the columns of steam which usually arise in large quantities when Vesuvius is tranquil, were now scarcely visible, and a piece of paper thrown into the aperture did not rise again; so that there was every reason to suppose the existence of a descending current of air*. The subterraneous thunder heard at such great distances under Vesuvius, is almost a demonstration of the existence of great cavities below filled with aëriform matter: and the same excavations which in the active state of the volcano throw out during so great a length of time immense volumes of steam, must, there is every reason to believe, in its quiet state become filled with atmospheric air†.

To what extent subterraneous cavities may exist even in common rocks, is shown in the limestone caverns of Carniola, some of which contain many hundred thousand cubical feet of air; and in proportion as the depth of an excavation is greater, so is the air more fit for combustion.

The same circumstance which would give alloys of the metals of the earths the power of producing volcanic phænomena, namely, their extreme facility of oxidation, must likewise prevent them from ever being found in a pure combustible state in the products of volcanic eruptions; for before they reach the external surface, they must not only be exposed to the air in the subterranean cavities, but be propelled by steam; which must possess, under the circumstances, at least the same facility of oxidating them as air. Assuming the hypothesis of the existence of such alloys of the metals of the earths as may burn into lava in the interior, the whole phænomena may be easily explained from the action of the water of the sea and air on those metals; nor is there any fact or any of the circumstances which I have mentioned in the preceding part of this paper, which cannot be easily explained according to that hypothesis. For almost all the volcanoes in the old world of considerable magnitude are near, or at no considerable distance from the sea: and if it be assumed that the first eruptions are produced by the action of sea water upon the metals of the earths, and that considerable cavities are left by the oxidated metals thrown out as lava, the results of their action are such as might be anticipated; for after the first eruptions, the oxidations which produce the subsequent ones may take place in the caverns below the surface; and when the sea is distant, as in the volcanoes of South America, they may be supplied with water from great subterranean lakes; as Humboldt states that some of them throw up quantities of fish.

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* In 1814, in 1815, and in January 1819, when Vesuvius was comparatively tranquil, I observed the solfaterra in a very active state, throwing up large quantities of steam and some sulphuretted hydrogen.

† Vesuvius is a mountain admirably fitted, from its form and situation, for experiments on the effect of its attraction on the pendulum: and it would be easy in this way to determine the problem of its cavities. On Etna, the problem might be solved on a larger scale.

On the hypothesis of a chemical cause for volcanic fires, and reasoning from known facts, there appears to me no other adequate source than the oxidation of the metals which form the bases of the earths and alkalies; but it must not be denied that considerations derived from thermometrical experiments on the temperature of mines and of sources of hot water, render it probable that the interior of the globe possesses a very high temperature: and the hypothesis of the nucleus of the globe being composed of fluid matter, offers a still more simple solution of the phænomena of volcanic fires than that which has been just developed.

Whatever opinion may be ultimately formed or adopted on this subject, I hope that these inquiries on the actual products of a volcano in eruption will not be without interest for the Royal Society.

XIV. *Rationale of the Difficulty of separating Plane Surfaces by a Blast; in certain Cases. By* R. HARE, *M.D. Professor of Chemistry in the University of Pennsylvania.*[*]

THE phænomenon above alluded to, is usually illustrated by means of two discs, into the centre of one of which, a tube is fastened, so that on blowing through the tube, the current is arrested by the other moveable disc. Under these circumstances, the moveable disc is not removed as would be naturally expected. Supposing the diameter of the discs to be to that of the orifice as 8 to 1, the area of the former to the latter must be as 64 to 1. Hence if the discs were to be separated (their surfaces remaining parallel) with a velocity as great as that of the blast, a column of air must meanwhile be interposed 64 times greater than that which would escape from the tube during the interim. Consequently, if all the air necessary to preserve the equilibrium be supplied from the tube, the discs must be separated with a velocity as much less than that of the blast, as the column, required between them, is greater than that yielded by the tube; and yet the air cannot be supplied from any other source, unless a deficit of pressure be created between the discs, unfavourable to their separation.

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[*] Communicated by the Author.

It follows, then, that under the circumstances in question, the discs cannot be made to move asunder with a velocity greater than 1-64th of that of the blast. Of course all the momentum of the aërial particles which constitute the current through the tube, will be expended on the moveable disc, and the thin ring of air which exists around the orifice between the discs; and since the moveable disc can only move with 1-64th of the velocity of the blast, the ring of air in the interstice must experience nearly all the momentum of the jet, and must be driven outwards; the blast following it in various currents radiating from the common centre of the tube and discs. The effect of such currents in producing an afflux of the adjoining portions of any fluid in which they may be excited, is well known, having been successfully illustrated by Venturi. (See Nicholson's Journal, quarto series, vol. ii. p. 172.) Accordingly the afflux of air towards the discs counteracts the small velocity which the blast would communicate, and thus prevents their separation, and may even cause them to approach each other, if previously situated a small distance apart.

This rationale commences with the assumption, that the discs will remain nearly parallel. That there cannot be much deviation from parallelism must be evident; since any obliquity will make the opening greater on one side than on the other; and the jet proceeding with most force towards the widest opening, will increase the afflux of air upon the outer surface of the moveable disc in the part where the current is strongest, and thus correct the obliquity.

XV. *Chemical Examination of the Oxides of Manganese. By* EDWARD TURNER, *M.D. F.R.S. Ed. Professor of Chemistry in the University of London, and Fellow of the Royal College of Physicians of Edinburgh.*

[Concluded from p. 35.]

PART II.

On the Composition of the Ores of Manganese described by Mr. Haidinger.

Method of Analysis.—PURE fragments of the ores were carefully selected, reduced to fine powder in a mortar of agate, and washed with distilled water. Some of the ores yielded nothing to the action of water; but from some of them, especially from those of Ihlefeld, minute quantities of the muriate and sulphate of lime, and sometimes of soda, were separated by the action of water. It is the accidental presence of the muriates which gives rise to the disengagement of chlorine when sulphuric acid is added to some of the native oxides of manganese, and which induced Mr. Macmullin to regard chloric acid as a constituent of these ores. For the correction of this error we are indebted to Mr. Richard Phillips*, with whose observation my own experiments correspond;—none of the native oxides yield a trace of chlorine on the addition of sulphuric acid, provided the muriates have been previously removed by washing.

The ores, before being submitted to analysis, were dried at 212° F., by which means they were brought to the same degree of dryness which they possessed before being washed. The water naturally contained in them was ascertained in every instance by heating a known quantity of the ore to redness, and collecting the water in a tube filled with fragments of the chloride of calcium.

The quantity of oxygen was in most cases ascertained both by bringing the ore to the state of red oxide by exposure to a white heat, and by converting it into the protoxide by means of heat and hydrogen gas. When performed with the precautions stated in the first part of this communication, either of these methods may be relied on with confidence; but the first is more convenient in general practice, because it requires less time and a more simple apparatus. The latter is sometimes very troublesome, owing to the difficulty with which some of the ores of manganese, the native peroxide for example, are reduced by hydrogen to the state of pure protoxide. I have in no instance estimated the quantity of oxygen by means of the deutoxide, the formation of this compound being in my opinion too uncertain to admit of any analytic process being founded upon it.

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* Phil. Mag. and Annals, vol. i. p. 313.

In searching for the presence of foreign matters I have employed the following processes. The water which was expelled from the ores by heat, was examined with test paper, but was always found quite free from alkaline or acid reaction. The absence of carbonates was ascertained by the entire want of effervescence on the addition of dilute nitric acid. Strong sulphuric acid did not cause the evolution of chlorine or any acid fumes.

On dissolving the ores in muriatic acid and evaporating the solution to perfect dryness, the residue, with the exception of a little siliceous matter and red oxide of manganese proceeding from slight decomposition of the chloride, was always completely redissolved by water. This circumstance demonstrates the absence of phosphoric and arsenic acids, which, if present, would have been left as the insoluble phosphate or arseniate of manganese. By well known methods I satisfied myself of the absence of sulphuric acid, alumina, and magnesia. In several of the ores the oxalate of ammonia detected a trace of lime. It is remarkable that every species, with one exception, contains baryta. In most of them, indeed, it is present only as an impurity; but in two of the ores, the uncleavable manganese-ore or black hematite, and the *manganèse oxidé noir barytifère* of Haüy, it is an essential ingredient of the mixture. In those species in which this earth exists as an impurity, it is not united with the sulphuric or carbonic acid; but is most probably combined with the peroxide of manganese.

From the frequency with which iron has been found accompanying the ores of manganese, I was led to expect its presence, and employed the ferrocyanate of potash and hydrosulphuret of ammonia as re-agents for its detection. The muriatic solution of the different species yielded a white precipitate with the ferrocyanate of potash, and the characteristic flesh-coloured sulphuret of manganese with the hydrosulphuret of ammonia. It hence follows that all the ores submitted to analysis, even the uncleavable manganese-ore, which has been placed among the ores of iron, are perfectly free from iron, as well as from copper, lead, and similar metallic substances.

Analysis of Manganite or the Prismatoidal Manganese-ore.—This ore, even when selected with the greatest care, yields to distilled water traces of the muriates and sulphates of lime and soda. It dissolves without residue in muriatic acid, and is free from siliceous earth, lime, baryta, and every other impurity. It is the purest native oxide of manganese which has fallen under my notice. Its powder has a uniform brown tint, and I have been unable to observe in it any tendency to pass into the peroxide by absorbing oxygen from the air. After exposure to the air for six months, during which it was frequently moistened with distilled water, it underwent no change of weight. Cold sulphuric acid acts very feebly on this oxide. M. Gmelin* of Heidelberg states that it is not dissolved at all by this acid in the cold, and I was at first of the same opinion: but by employing a considerable quantity of the oxide, and agitating the mixture frequently, the acid does acquire a red tint in the course of two or three days. In this respect manganite agrees with the peroxide; but differs from all the other species, which communicate a red colour to cold sulphuric acid with much greater facility.

When manganite is heated to redness it gives out 10·10 per cent of water; and the total loss from exposure to a white heat is 13·15 per cent. Deducting from the last number the amount of water, 3·05 remain as the loss in oxygen. The result of this analysis is therefore,

Red oxide	86·85
Oxygen	3·05
Water	10·10
	100·00

According to this analysis, manganite contains an oxide of manganese, 89·9 parts of which yield 3·05 of oxygen, on being converted into the red oxide. An equal quantity of pure deutoxide, in undergoing a similar change, should lose 2·997 of oxygen.

Exposed to a strong red heat and a current of hydrogen gas, 100 parts of manganite lost 19·09 parts in one experiment, and 19·07 in another. The mean is 19·08, and subtracting 10·10 as water, 8·98 remain as oxygen. According to this analysis the manganite is composed of

Protoxide	80·92
Oxygen	8·98
Water	10·10
	100·00

Now as 80·92 : 8·98 : : 36 : 3·995.

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* I regret that I have been unable to obtain a sight of that volume of the *Zeitschrift der Mineralogie*, which contains M. Gmelin's paper on the composition of the oxides of manganese. My knowledge of his labours is solely derived from M. Leonhard's *Handbuch der Oryktognosie*.

From the result of both analyses it is apparent that manganite, in relation to manganese and oxygen, is a deutoxide.

Also as 89·90 : 10·10 : : 40 : 4·494.
The fourth number is so near 4·5, half an equivalent of water, that we may safely regard manganite as a compound of 80 parts or two equivalents of the deutoxide of manganese, and 9 parts or one equivalent of water.

The material for the preceding analysis was taken from a very fine crystallized specimen from Ihlefeld. The result of Gmelin's analysis of the same variety is as follows:—Red oxide 87·1, oxygen 3·4, water 9·5. The water is here certainly underrated.

The grey oxide from Undenaes in West Gothland, analysed by Arfwedson, is a similar compound.

Analysis of the Brachytypous Manganese-ore or Braunite.— The colour of this ore, both in mass and in powder, is nearly black. With sulphuric acid it yields no distinct odour of chlorine. It dissolves in muriatic acid, leaving a trace of siliceous matter. The solution gives a precipitate of sulphate of baryta with sulphuric acid, but does not contain any other impurity. Of all the native oxides this is the most easily reduced to the state of protoxide by the action of hydrogen gas. The material for analysis formed part of a specimen from Elgersburg.

As a mean of two closely corresponding experiments, this oxide contains 0·949 per cent of water.

To ascertain the quantity of oxygen, 16·634 grains were exposed for half an hour to the action of hydrogen gas at a red heat. The residue weighed 14·837 grains, and had the light green tint of the protoxide. The total loss was 1·797 grains, or 10·80 per cent; and subtracting 0·949 for water, there remains 9·851 per cent as the loss in oxygen.

The baryta was precipitated by sulphuric acid from a solution in muriatic acid of 42·09 grains of the mineral. The precipitate after being heated to redness amounted to 1·44 grains, equivalent to 0·951 of a grain or 2·26 per cent of pure baryta. According to this analysis, 100 parts of the ore contain

Protoxide	86·94
Oxygen	9·851
Water.......................	0·949
Baryta	2·260
Silica	a trace.
	100·000

Now 86·94 : 9·851 : : 36 : 4·079; and as the presence of water and baryta, from the small quantity of these substances,

must

must be regarded rather as accidental than essential to the mixture, it follows that braunite is an anhydrous deutoxide of manganese. I apprehend the baryta must be in combination with deutoxide of manganese; since, were it united with peroxide, the loss in oxygen would exceed the quantity above stated.

I am not acquainted with any analysis of this mineral by other chemists.

Analysis of the Pyramidal Manganese-ore or Hausmannite.— Hausmannite, before being washed, yields a faint odour of chlorine by the action of sulphuric acid. When heated to redness it gives off 0·435 per cent of water; and at a white heat the loss is only 0·65 per cent, indicating 0·215 of oxygen. When dissolved in muriatic acid, a small quantity of silica is left, amounting to 0·337 per cent; and on adding sulphuric acid to the solution, a little sulphate of baryta subsides, indicating 0·111 per cent of the pure earth. Hausmannite is accordingly resolved by this analysis into

Red oxide	98·098
Oxygen	0·215
Water	0·435
Baryta	0·111
Silica	0·337
	100·000

This oxide is manifestly an anhydrous red oxide of manganese. The small quantity of oxygen lost at a white heat is probably owing to the admixture of a little deutoxide or peroxide, combined with the baryta.

From some preliminary experiments on hausmannite, M. Gmelin of Heidelberg* inferred that it is a pretty pure red oxide, an inference which entirely agrees with the result of the preceding analysis. This is the only chemical examination of hausmannite by other chemists, which I have met with. The material for my analysis was part of a specimen from Ihlefeld, for which I am indebted to the kindness of Professor Stromeyer.

Analysis of Pyrolusite, or the Prismatic Manganese-ore.— The following analysis was made with a compact columnar variety from Elgersburg, which has a specific gravity of 4·94, and the individuals of which have a parallel direction. With sulphuric acid it does not yield a trace of chlorine; and the only impurities which I could discover in it are silica and baryta, the former amounting to 0·513, and the latter to 0·532 per cent.

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* Leonhard's *Handbuch der Oryktognosia*.

The quantity of water was determined as usual by means of the chloride of calcium, and amounted to 1·12 per cent.

On exposing 23·746 grains of this oxide to a white heat, the loss proved to be 3·064 grains or 12·90 per cent. Subtracting 1·12 for water, there remain 11·78 as the loss of oxygen.

Accordingly, 100 parts of the pyrolusite were resolved into

Red oxide	84·055
Oxygen	11·78
Water	1·12
Baryta	0·532
Silica	0·513
	100·000

Now, omitting the water, baryta, and silica as accidental impurities, the remaining 97·835 parts lose 11·78 parts, or 12·04 per cent of oxygen in being converted into the red oxide. On the supposition that pyrolusite is composed of one equivalent of manganese and two equivalents of oxygen, it should lose in passing into the state of red oxide exactly 12·122 per cent of oxygen, a quantity which corresponds closely with the result of analysis. It is therefore an anhydrous peroxide of manganese.

I have analysed another columnar variety of pyrolusite, which has a density of 4·819, and of which the individuals radiate from a common centre. I brought it with me from Germany, and believed it to be from Ihlefeld, as the ticket indicated; but Mr. Haidinger, after carefully inspecting several large cabinets in Germany, has been unable to discover any similar specimen which is known to have been found in that place. Its locality therefore is doubtful.

This variety is less pure than the foregoing. Before being washed, it yields chlorine on the addition of sulphuric acid; and after the muriates have been removed by distilled water, the neutral solution in muriatic acid gives traces of lime with oxalate of potash. It contains silica and baryta nearly in the same proportion as the first variety.

The following is the result of my analysis:

Red oxide	85·617
Oxygen	11·599
Water	1·566
Silica	0·553
Baryta	0·665
Lime	a trace.
	100·000

Subtracting 2·784 as impurities, there remain 97·214 parts, which lose 11·599, or 11·931 per cent, of oxygen in being converted into the red oxide. It is therefore an anhydrous peroxide, most probably containing an admixture of some other oxide.

Analysis of Psilomelane, or the Uncleavable Manganese-ore. —This mineral when reduced to powder has a brownish-black colour. With sulphuric acid it does not emit any odour of chlorine. It dissolves completely in muriatic acid, excepting a small quantity of silica which amounts to 0·26 per cent; and the only substances which I could detect in the solution are baryta and the oxide of manganese. Though this ore has been placed by mineralogists among the oxides of iron, under the names of Black Hematite and Black Iron-ore, pure fragments of it do not contain a trace of that metal.

When heated to redness psilomelane gives out 6·216 per cent of water. The diminution in weight occasioned by exposure to a white heat is 13·58 per cent; and on subtracting 6·216 for water, there remains 7·364 as the loss in oxygen.

To ascertain the quantity of baryta 30·028 grains of the mineral were dissolved in muriatic acid, and the baryta precipitated by means of the sulphate of soda, a considerable excess of muriatic acid being allowed to remain in the liquid, to prevent any manganese from adhering to the precipitate. The sulphate of baryta, after exposure to a red heat, amounted to 7·434 grains, equivalent, according to the atomic numbers of Dr. Thomson, to 4·914 grains, or 16·365 per cent of pure baryta.

According to this analysis, 100 parts of psilomelane have yielded of

Red oxide............	69·795
Oxygen...............	7·364
Baryta	16·365
Silica................	0·260
Water	6·216
	100·000

The precise atomic constitution of psilomelane is not made apparent by this analysis; and, indeed, the result is of such a nature as to leave no doubt of this mineral containing more than one oxide of manganese. For it follows, from the quantity of oxygen expelled by heat, that a considerable part of the manganese must be in the form of peroxide; but it is equally clear that the whole of it cannot be in that state, because 69·795 parts of red oxide require 9·627 instead of 7·364 parts of oxygen to constitute the peroxide. On perceiving this deficiency of oxygen, I at first suspected that the baryta might prevent the usual quantity of oxygen from being expelled from the peroxide by heat. Accordingly I ascertained the quantity of pure red oxide by the way of precipitation; but its amount corresponded closely with the number already stated. Psilomelane must therefore, I conceive, be a mixed mineral. I was at first disposed to regard it as a compound of baryta and peroxide of manganese, accidentally containing an admixture of some other oxide in a lower stage of oxidation; but the fact noticed by Mr. Haidinger of psilomelane being frequently and intimately associated with pyrolusite in the mineral kingdom, appears to justify the inference, that the uncleavable manganese-ore consists essentially of some compound, in proportions not yet ascertained, of baryta and the deutoxide of manganese, and that pyrolusite is the accidental ingredient. The propriety of this view is further shown by an analysis of the following ore from Romanèche, a mineral which is analogous to psilomelane in the proportion of its ingredients, and in which an admixture of pyrolusite may be detected by the eye.

Analysis of the Manganèse oxidé noir Barytifère *from Romanèche.*—The observations of Mr. Haidinger leave no doubt of this ore being a mixed mineral; and according to my analysis it is very analogous to psilomelane. The specific gravity of some of the purest fragments which I could select, is 4·365; and the density of psilomelane, according to Mr. Haidinger, is 4·145. The colour of both minerals is similar.

The black oxide of Romanèche yields a very faint odour of chlorine with sulphuric acid. When heated to redness it gives out 4·13 per cent of water. At a white heat it loses 11·39 per cent; and after subtracting 4·13 for water, there remain 7·26 as the loss in oxygen.

In order to ascertain the quantity of baryta, 32·13 grains were dissolved in muriatic acid; and after separating a small portion of silica, which amounted to 0·953 per cent, I precipitated the baryta by means of the sulphate of soda. The insoluble sulphate, after exposure to a red heat, weighed 8·113 grains, equivalent to 5·363 grains, or 16·69 per cent of pure baryta. 100 parts of the oxide are accordingly resolved into

Red oxide	70·967
Oxygen	7·260
Baryta	16·690
Silica	0·953
Water	4·130
	100·000

This mineral was analysed some years ago by Vauquelin and Dolomieu; but the numbers which they have mentioned, owing to the insufficient mode of analysis employed at that time, are not entitled to any confidence.—(*Journal des Mines* ix. 778.)

XVI. *General Solution of the Problem: to represent the Parts of a given Surface on another given Surface, so that the smallest Parts of the Representation shall be similar to the corresponding Parts of the Surface represented.* By C. F. Gauss. *Answer to the Prize Question proposed by the Royal Society of Sciences at Copenhagen*[*].

Ab his via sternitur ad majora.

THE author of this paper believes that he must consider the repeated selection by the Royal Society of the question which forms the subjects of it, as a proof of the importance which the Royal Society attaches to it; and has thereby been induced to submit a solution found by him some considerable time since, as the lateness of the time at which he was informed of the prize question would otherwise have prevented him from sending an answer. He regrets that the latter circumstance has obliged him to limit his inquiry to the essential part only, besides hinting some obvious applications to the projection of maps and the higher branches of geodetics. Had it not been for the near approach of the term fixed by the Society, he would have followed up several inquiries, and have detailed numerous applications of the subject to geodetical operations; all which he must now reserve to himself for another moment and another place.

December 1822.

1. The nature of a curve surface is determined by an equation between the coordinates belonging to every point of the same x, y, z. In consequence of this equation, every one of these three variable quantities may be considered as a function of the two others. It is still more general to introduce two new variable quantities t, u, and to represent each of the quantities x, y, z as a function of t and u, by which at least generally speaking, determinate values of t and u always belong to every determinate point of the surface, and *vice versâ*.

2. Let X, Y, Z, T, U have the same signification for a second surface, which x, y, z, t, u had in reference to the first.

3. To *represent* the former surface on the second means to

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[*] From Prof. Schumacher's *Astronomische Abhandlungen*, No. 3.

establish a law by which a determinate point of the second surface is to correspond to every point of the first. This will have been effected if T and U have been made equal to two functions of t and u. These functions will cease to be arbitrary as soon as they are required to satisfy certain conditions. As X, Y, Z next become likewise functions of t and u, these functions must, therefore, besides satisfying the conditions required by the nature of the second surface, also fulfill those of the representation.

The problem of the Royal Society of Sciences prescribes that the representation shall be similar to the object represented in the smallest parts. It is, therefore, first required to find an analytical expression for this condition. Let us suppose that the following equations are the result of the differentiation of the functions of t and u expressing the values of x, y, z, X, Y, Z.

$$\begin{aligned}
dx &= a\,dt + a'du \\
dy &= b\,dt + b'du \\
dz &= c\,dt + c'du \\
d\mathrm{X} &= \mathrm{A}\,dt + \mathrm{A}'du \\
d\mathrm{Y} &= \mathrm{B}\,dt + \mathrm{B}'du \\
d\mathrm{Z} &= \mathrm{C}\,dt + \mathrm{C}'du
\end{aligned}$$

The condition prescribed requires first that all infinitely small lines proceeding from one point of the first surface and situate in it, shall be proportionate to the corresponding lines on the second surface; and next, that the former shall form between them the same angles as the latter.

Such a linear element on the first surface has this expression

$$\sqrt{\left((a^2+b^2+c^2)dt^2+2(aa'+bb'+cc')\,dt\,du+(a'^2+b'^2+c'^2)du^2\right)}$$

and the corresponding one on the second surface is

$$\sqrt{\left((\mathrm{A}^2+\mathrm{B}^2+\mathrm{C}^2)\,dt^2+2\,(\mathrm{AA}'+\mathrm{BB}'+\mathrm{CC}')\,dt\,du+(\mathrm{A}'^2+\mathrm{B}'^2+\mathrm{C}'^2)du^2\right)}.$$

If both are to be in a certain ratio independent of dt and du, the three quantities

$$a^2+b^2+c^2,\ aa'+bb'+cc',\ a'^2+b'^2+c'^2$$

must evidently be respectively proportional to the three quantities $\mathrm{A}^2+\mathrm{B}^2+\mathrm{C}^2,\ \mathrm{AA}'+\mathrm{BB}'+\mathrm{CC}',\ \mathrm{A}'^2+\mathrm{B}'^2+\mathrm{C}'^2$.

If we suppose that the values t, u and $t+\delta t$, $u+\delta u$ correspond to the extreme points of a second element on the first surface, the cosine of the angle formed between the two elements on that surface, will be

$$\frac{(a\,dt+a'du)(a\delta t+a'\delta u)+(b\,dt+b'du)(b\delta t+b'\delta u)+(c\,dt+c'du)(c\delta t+c'\delta u)}{\sqrt{\left[(a\,dt+a'du)^2+(b\,dt+b'du)^2+(c\,dt+c'du)^2\right]\cdot\left[(a\delta t+a'\delta u)^2+(b\delta t+b'\delta u)^2+(c\delta t+c'\delta u)^2\right]}}$$

and we shall obtain an exactly similar expression for the cosine of the corresponding angle on the second surface by changing a, b, c, a', b', c' into A, B, C, A′, B′, C′. The two expressions become clearly equal if the above-mentioned proportionality takes place, and the second condition is already comprehended in the first, as a little reflection will easily show.

The analytical expression of the condition of our problem is, therefore, this:

$$\frac{A^2+B^2+C^2}{a^2+b^2+c^2} = \frac{AA'+B \,.\, B'+C \,.\, C'}{aa'+bb'+cc'} = \frac{A'A'+B'B'+C'C'}{a'^2+b'^2+c'^2}.$$

Let the value of these equal quantities, which must be a finite function of t and u, be $= m^2$. The quantity m is therefore the index of the ratio in which linear quantities on the first surface are increased or diminished in their representation on the second surface (according as m is greater or smaller than 1). This ratio will, generally speaking, be different in different places: in the particular case in which m is constant, there will be a perfect similarity also in the finite parts; and if m is besides $= 1$, there will be a perfect equality, and the one surface may be developed on the other. Putting for brevity

$$(a^2+b^2+c^2)\,dt^2+2(aa'+bb'+cc')\,dt.du+(a'^2+b'^2+c'^2)du^2=\omega$$

we remark that the differential equation $\omega=0$ admits of two integrations. Representing the trinomial ω as the product of two factors linear with respect to dt and du, either of the two may be $= 0$, which will give two different integrations. One of the integrations will be derived from the equation : $0 =$

$$(a^2+b^2+c^2)\,dt+\{aa'+bb'+cc'+i\sqrt{[(a^2+b^2+c^2)\,(a'^2+b'^2+c'^2)-(aa'+bb'+cc'^2)]}\}du$$

(where i is written for brevity instead of $\sqrt{-1}$, for it will be easily seen that the irrational part of the expression must become imaginary), the other integration will be the result of a similar equation, which will be obtained by putting $-i$ in place of i in the former. If the integral of the first equation be this

$$p+iq = \text{const.}$$

where p and q denote real functions of t and u, the other integral will be

$$p-iq = \text{const.}$$

It follows from this, that $(dp+idq)\,(dp-idq)$ or dp^2+dq^2 must be a factor of ω, or that

$$\omega = n\,(dp^2+dq^2)$$

where n is a finite function of t and u.

Let us now denote by Ω the trinomial into which dX^2+

$dY^2 + dZ^2$ will be converted by substituting for dX, dY, and dZ their values expressed by T, U, dT and dU; and let us assume that in a similar manner, as before, the two integrals of the equation $\Omega = 0$ are as follow:

$$P + iQ = \text{const.} \quad ; \; P - iQ = \text{const. and}$$

$\Omega = N.(dP^2 + dQ^2)$ where P, Q, N denote real functions of T and U.

These integrations may evidently be effected (without taking into consideration the general difficulties of integrating) before the solution of our principal problem.

Now, if for T, U such functions of t and u are substituted as will fulfill the condition of our principal problem, Ω will be changed into $m^2 \omega$, and we shall have

$$\frac{(dP + idQ).(dP - idQ)}{(dp + idq) \cdot (dp - idq)} = \frac{m^2 n}{N}.$$

But it will be easily seen that the numerator in the first part of this equation cannot be divisible by the denominator, except if either

$dP + idQ$ is divisible by $dp + idq$, and $dP - idQ$ by $dp - idq$,

or,

$dP + idQ$ is divisible by $dp - idq$, and $dP - idQ$ by $dp + idq$.

In the former case $dP + idQ$ will therefore vanish if $dp + idq = 0$, or $P + iQ$ will be constant if $p + iq$ is supposed to be constant; that is to say, $P + iQ$ will be a function of $p + iq$ only, and in the same manner $P - iQ$ will be a function of $p - iq$. In the latter case $P + iQ$ will be a function of $p - iq$, and $P - iQ$ a function of $p + iq$. It is easy to perceive that the reverse of these positions likewise holds good, or that if for $P + iQ$ and $P - iQ$ functions of $p + iq$ or $p - iq$ (either respectively or inversely) are assumed the divisibility of Ω by ω, and consequently the above required proportionality will take place.

It will easily be conceived that if, for example, we suppose

$$P + iQ = f(p + iq), \; P - iQ = f'(pe - iq)$$

the nature of the function f' is already given by that of f. For if among the constant quantities which it involves, there are none but real quantities, the function f' must be identical with f; in order that real values of P and Q may correspond to real values of p and q: in the contrary case, f' will only be distinguished from f by having in the imaginary quantities which f involves $-i$ instead of $+i$.

We have next, $P = \frac{1}{2}f(p + iq) + \frac{1}{2}f'(p - iq)$

$$iQ = \tfrac{1}{2}f(p + iq) - \tfrac{1}{2}f'(p - iq),$$

or, which is the same, as the function f is assumed quite arbitrarily

bitrarily (involving at pleasure constant imaginary quantities), P is equal to the real, and iQ (or $-iQ$ in the second solution) equal to the imaginary part of $f(p+iq)$, and by elimination T and U will be represented as functions of t and u. Thus the proposed problem is solved quite generally and completely.

6. If we represent any determinate function of $p+iq$ by $p'+iq'$ (where p' and q' are real functions of p and q), it will be easily seen that likewise the equations

$$p'+iq' = \text{const. and } p'-iq' = \text{const.}$$

will represent the integrals of the differential equation $\omega = 0$; indeed, these equations will respectively agree with the above

$$p+iq = \text{const. and } p-iq = \text{const.}$$

In like manner the integrals of the differential equation $\Omega = 0$, viz.

$$P'+iQ' = \text{const. and } P'-iQ' = \text{const.}$$

will agree with the above,

$$P+iQ = \text{const. and } P-iQ = \text{const.}$$

if $P'+iQ'$ represents any determinate function of $P+iQ$ (while P' and Q' are real functions of P and Q). Hence it is clear that in the general solution of our problem which we have given in the preceding article, p' and q' may be substituted for p and q, and P' and Q' for P and Q. Although this change does not add to the generality of the solution, yet in practice one form may be more applicable to one, and another to another purpose.

7. If the functions arising from the differentiation of the arbitrary function f and f' are denoted respectively by ϕ and ϕ', so that $d.f'v = \phi v.dv$ and $d.f'v = \phi'v.dv$. we shall have in conformity with our general solution

$$\frac{dP+idQ}{dp+idq} = \varphi(p+iq), \quad \frac{dP-idQ}{dp-idq} = \phi'(p-iq),$$

therefore,

$$\frac{m^2n}{N} = \phi(p+iq).\phi(p-iq).$$

The scale of linear dimensions is determined by

$$m = \surd\left\{\frac{dp^2+dq^2}{\omega.} \cdot \frac{\Omega}{dP^2+dQ^2} \cdot \phi(p+iq) \cdot \phi'(p-iq)\right\}.$$

8. We shall now illustrate our general solution by some examples by which the manner of applying it, as well as the nature of some circumstances which may come into consideration, may be best explained.

Let the two surfaces be in the first place planes, in which case we may put

$$x = t, \quad y = u, \quad z = 0$$
$$X = T, \; Y = U, \; Z = 0.$$

The differential equation $\omega = dt^2 + du^2 = 0$ gives these two integrals

$$t + iu = \text{const.},\quad t - iu = \text{const.}$$

and in like manner the two integrals of the equation $\Omega = d\mathrm{T}^2 + d\mathrm{U}^2 = 0$, are the following $\mathrm{T} + i\mathrm{U} = \text{const.}$, $\mathrm{T} - i\mathrm{U} = \text{const.}$ The two general solutions of the problem are accordingly

$$\text{I. } \mathrm{T}+i\mathrm{U} = f(t+iu),\quad \mathrm{T}-i\mathrm{U} = f'(t-iu)$$
$$\text{II. } \mathrm{T}+i\mathrm{U} = f(t-iu),\quad \mathrm{T}-i\mathrm{U} = f'(t+iu).$$

This result may be thus expressed: f signifying an arbitrary function, the real part of $f(x+iy)$ is to be taken for x, and the imaginary part divided by i for y or for $-y$.

If the functional characteristics ϕ, ϕ' are taken in the same signification which they have in article 7, and if we put

$$\phi(x+iy) = \xi + i\eta,\quad \phi'(x-iy) = \xi - i\eta$$

where ξ and η will be clearly real functions of x and y, we have by the first solution

$$d\mathrm{X} + i\,d\mathrm{Y} = (\xi + i\eta)(dx + i\,dy)$$
$$d\mathrm{X} - i\,d\mathrm{Y} = (\xi - i\eta)(dx - i\,dy)$$

and consequently, $d\mathrm{X} = \xi\,dx - \eta\,dy$

$$d\mathrm{Y} = \eta\,dx + \xi\,dy$$

If we now put $\xi = \sigma . \cos j \quad , \quad \eta = \sigma . \sin j$

$$dx = ds . \cos g,\quad dy = ds . \sin g$$
$$d\mathrm{X} = d\mathrm{S} . \cos \mathrm{G},\quad d\mathrm{Y} = d\mathrm{S} . \sin \mathrm{G},$$

so that ds is a linear element in the first plane, g its inclination to the line of abscissae, $d\mathrm{S}$ the corresponding linear element in the second plane, and G its inclination to the line of abscissae, the above equations give

$$d\mathrm{S} . \cos \mathrm{G} = \sigma . ds . \cos(g+j)$$
$$d\mathrm{S} . \sin \mathrm{G} = \sigma\, ds \quad \sin(g+j),$$ and consequently,

if we consider σ as positive, as we may do

$$d\mathrm{S} = \sigma . ds \quad , \quad \mathrm{G} = g + j.$$

We see, therefore (in conformity to article 7), that σ is the index of the ratio of increase of the element ds in the representation ds, and is, as it ought to be, independent of g; and in the same way the angle j being independent of g, proves that all linear elements of the first plane proceeding from one point are represented by elements in the second plane which form to each other, and, as we may add, in the same direction, the same angles.

If we now choose for f a linear function, so that $fv = \mathrm{A} + \mathrm{B}v$ where the constant coefficients are of the form $\mathrm{A} = a + b . i$, $\mathrm{B} =$

$B = c + e\,i$, we shall have $\varphi v = B = c + e\,i$, therefore $\sigma = \surd(c^2 + e^2)$, $j = \text{arc tang}\,\frac{c}{c}$.

The ratio of increase or the scale is consequently constant throughout, and the whole representation similar to the surface represented. For every other function f, it may be easily proved that the scale cannot be constant, and that the similarity can only take place in the smallest part. If the places are given which are to correspond in the representation to a determinate number of given points of the first plane, we may easily determine by the common method of interpolation the simplest algebraical function f, which will fulfill those conditions. If we denote the values of $x + i\,y$ for the given points by a, b, c, &c. and the corresponding values of $X + i\,Y$ by A, B, C, &c. then it will be necessary to put

$$f v = \frac{(v-b)(v-c)\ldots}{(a-b)(a-c)\ldots}\,A + \frac{(v-a)(v-c)\ldots}{(b-a)(b-c)\ldots}\,B + \frac{(v-a)(v-b)\ldots}{(c-a)(c-b)\ldots}\,C + \text{\&c.}$$

which is an algebraical function of v of a degree one unity lower than the number of given points. For two points, where the function becomes linear, a perfect resemblance will consequently take place.

An useful application may be made of this in geodetics, for converting a map founded on moderately good measurements, which in its minute detail is good, but on the whole somewhat distorted, into a better one, if the correct position of a number of points is known.

Going through the second solution in the same manner, it will be found that the only difference is, that the similarity is a reversed one; that all elements form indeed with each other the same angles as in the original, but in a contrary direction, so that that which is to the right in the one, is to the left in the other. But this difference is not an essential one, and vanishes if the side of the plane which was first considered as the upper one is made the lower one. This latter remark may be always applied whenever one of the surfaces is a plane; and we shall confine ourselves in the following examples of this kind to the first solution.

9. Let us now consider (as a second example) the representation of the surface of a perpendicular cone in a plane. As the equation of the former, we take

$$x^2 + y^2 - K^2 z^2 = 0$$

where we put $x = K\,t \,.\, \cos u$, $y = K\,t \,.\, \sin u$, $z = t$, and as before, $X = T$, $Y = U$, $Z = 0$.

The differential equation,

$$\omega = (K^2+1)\,dt^2 + K^2 t^2 du^2 = 0, \text{ gives the two integrals,}$$

$$\log t \pm i\sqrt{\frac{K^2}{K^2+1}}\,.\,u = \text{const.}$$

We have, accordingly, the solution

$$X + iY = f\left(\log t + i\sqrt{\frac{K^2}{K^2+1}}\,u\right),$$

$$X - iY = f'\left(\log t + i\sqrt{\frac{K^2}{K^2+1}}\,.\,u\right);$$

that is to say, f denoting an arbitrary function, X is to be the real part of $f\left(\log t + i\sqrt{\frac{K^2}{K^2+1}}\,u\right)$, and Y the imaginary part, leaving out the factor i.

Let an exponential quantity be taken for f, or let $fv = he^v$ where h is constant and e the base of the hyperbolical logarithms, and the most simple representation will be

$$X = ht\,.\cos\sqrt{\frac{K^2}{K^2+1}}\,.\,u\,.\quad Y = ht\,.\sin\sqrt{\frac{K^2}{K^2+1}}\,.\,u.$$

The application of the formulæ of article 7, gives in this case

$$n = (K^2+1)\,t^2 \quad N = 1$$

and ϕv being $= \phi' v = h e^v$,

$$\phi\left(\log t + i\sqrt{\frac{K^2}{K^2+1}}\,.\,u\right).\,\phi'\left(\log t - i\sqrt{\frac{K^2}{K^2+1}}\,.\,u\right) = h^2 t^2$$

consequently $m = \frac{h}{\sqrt{(K^2+1)}}$, and therefore constant. If now, besides, h is made $= \sqrt{(K^2+1)}$, the representation becomes a perfect development.

10. Let it next be required; to represent in a plane the surface of a sphere whose radius $= a$. We put here

$$x = a\,.\cos t\,.\sin u, \quad y = a\,.\sin t\,.\sin u,$$

$$Z = a\,.\cos u, \text{ by which we obtain}$$

$\omega = a^2 \sin u^2\, dt^2 + a^2 du^2$. The differential equation $\omega = 0$ gives consequently

$$dt \mp i\,.\frac{du}{\sin u} = 0, \text{ and its integration}$$

$$t \pm i \log.\text{cotang}.\tfrac{1}{2}u = \text{const.}$$

If we denote therefore again by f an arbitrary function, X is to be put equal to the real, and iY to the imaginary part of $f(t + i \log \text{cotang } \frac{1}{2}u)$. We shall adduce some particular cases of this general solution. If we choose for f a linear function by putting $fv = kv$, we shall have $X = kt$ $Y = k \log \text{cotang } \frac{1}{2}u$.

This agrees evidently when applied to the earth with Mercator's projection, if we make t the geographical longitude, and $90^\circ - a$ the latitude. For the scale of linear dimensions the formulæ of article 7 give $m = \frac{k}{a \sin u}$.

If we assume for f an imaginary exponential function, and in the first place the simplest of all, $f v = k c^{iv}$, we have $f(t + i \log \text{cotang} \frac{1}{2} u) = k c^{\log \text{tang} \frac{1}{2} u + it} =$ $k \text{ tang } \frac{1}{2} u (\cos t + i \sin t)$ and $X = k \text{ tang } \frac{1}{2} u . \cos t$, $Y = k \text{ tang } \frac{1}{2} u . \sin t$ which is, as will be easily seen, the stereographical projection.

If we put more generally $f v = k e^{i\lambda v}$, we have

$$X = k \text{ tang } \tfrac{1}{2} u^{\lambda} . \cos \lambda t \quad , \quad Y = k \text{ tang } \tfrac{1}{2} u^{\lambda} . \sin \lambda t.$$

For the scale of linear dimensions in the representation, we obtain here $n = a^2 \sin u^2$, $N = 1$, $\phi k v = i \lambda k e^{i \lambda v}$, and hence

$$m = \frac{\lambda k \text{ tang } \frac{1}{2} u}{a \sin u}.$$

It is evident that the representation of all points for which u is the same, will form a circle, and the representation of those points for which t is constant, a straight line, as also that the different circles corresponding to the different values of u are concentric. This affords a very useful projection for maps, if a part only of a sphere is to be represented. It will then be best to choose λ in such a manner as to make the scale the same for the extreme values of u which will make it smallest towards the middle. If we suppose the extreme values of u to be u° and u', we must put

$$\lambda = \frac{\log \sin u' - \log \sin u^\circ}{\log \text{tang } \frac{1}{2} u' - \log \text{tang } \frac{1}{2} u^\circ}.$$

The sheets Nos. 19—26 of Prof. Harding's Celestial Maps, are drawn agreeably to this projection.

11. The general solution of the example given in the preceding article, may be exhibited in another form, which deserves to be mentioned on account of its neatness.

In conformity to what has been proved in article 6, we have [tang $\frac{1}{2} u (\cos t + i \sin t)$ being a function of $t + i \log \text{cotang} \frac{1}{2} u$ and $\text{tang } \frac{1}{2} u (\cos t + i \sin t) = \frac{\sin u . \cos t + i \sin u . \sin t}{i + \cos u} = \frac{x + iy}{a + z}$] for the general solution likewise these formulæ:

$X + iY = f \frac{x + iy}{a + z}$, $X - iY = f' \frac{x - iy}{a + z}$ that is, X must be made equal to the real, and iY to the imaginary part of $f' \frac{x + iy}{a + z}$

f' denoting an arbitrary function. It will be easily seen that instead of $f\frac{x+iy}{a+z}$, any arbitrary function of $\frac{y+iz}{a+x}$, or of $\frac{z+ix}{a+y}$, may be taken.

[To be continued.]

XVII. *Analysis of two new Mineral Substances, consisting of Bi-seleniuret of Zinc and Sulphuret of Mercury, found at Culebras in Mexico. By Professor* Del Rio*.

EACH step of the traveller in this Republic discovers to him something new. Mr. Joseph Manuel Herrera, in an excursion to Culebras, near the mining district of El Doctor, found a mineral resembling cinnabar, accompanied by metallic quicksilver, in the limestone which overlies the red sandstone (*arenisca roxa*), and he gave me a few small specimens of this substance. Some considerable time afterwards Col. Robinson gave me an additional quantity, informing me at the same time that Dr. Magos had obtained two ounces and a half of quicksilver from sixteen ounces of the ore.

Under the blowpipe the red ore burns with a beautiful violet-coloured flame, accompanied by much smoke of a most offensive smell, resembling that of rotten cabbage: the residue is a grayish-white earthy matter.

Intimately admixed with the red mineral is another substance so strongly resembling light gray silver ore, that I acknowledge that I mistook it, at first, for this ore of silver. My only doubt on the subject arose from the consideration that gray silver ore and cinnabar are never found together. It differs, however, from gray silver in yielding a blacker powder when scraped, and which stains more than the powder of the latter. Under the blowpipe nearly the same phænomena are observed as when the red mineral is submitted to the same test. According to Mr. Chovell, the specific gravity of the gray substance is 5·56, after having been carefully cleared by washing from the calcareous spar of the matrix. That of the red substance, after having also been carefully separated from the spar, is 5·66; while the specific gravity of hepatic mercury exceeds 5·8.

The analysis of these minerals is very easy where great precision is not required. Nothing more is necessary than to put fifty grains of the ore in a small retort on the fire; mercury, selenium, and a small quantity of sulphur are immediately sublimed, and a sub-oxide of zinc remains at the bottom of the retort. That the metallic gray powder attached to the upper part of the retort is selenium, is proved by the red colour of the light transmitted through it, and by the high metallic lustre of the surface in contact with the glass. The residuum is shown to be sub-oxide of zinc by its solubility in acids, and by its being redissolved by an excess of potass, soda or ammonia, after having been precipitated by the alkali from an acid solution: also by its phosphorescence when fused by the blowpipe, by the white smoke which it emits and which attaches itself to the charcoal, and by the enamel which it forms with borax and microcosmic salt.

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* Communicated by A. F. Mornay, Esq.

In order to determine the proportions of the component parts of the gray substance, I first treated it with concentrated sulphuric acid, which dissolved the mercury and some of the zinc; I then applied nitric acid, which dissolved the remainder of the zinc, and I finally employed nitro-muriatic acid to oxidate the selenium. By these operations 1·5 grain of sulphur, without any red tinge, and which I therefore suppose to be pure, was separated; and after the nitro-muriatic acid had been distilled off, selenic acid was sublimed: this was partly in acicular crystals, and partly in a dense white mass half fused and semi-transparent. There remained at the bottom of the retort the sulphate of lime formed by the sulphuric acid used in the first process, and the lime of the calcareous spar accidentally mixed with the mineral.

I think that it may be deduced from the foregoing experiments and others, that the gray mineral is composed of

Selenium	49
Zinc	24
Mercury	19
Sulphur	1·5
	93·5

which, with the addition of six grains of lime obtained, will amount to 99·5. But the lime merely accompanies the ore, and does not enter into its composition.

The gray mineral is therefore a bi-seleniuret of zinc united to a protosulphuret of mercury, the latter giving, in my opinion, the dark or gray colour to the mineral.

The red mineral will also be a bi-seleniuret of zinc, but the mercury will be in the state of a bi-sulphuret or cinnabar, which will give the red colour to the mineral.

These two minerals are therefore in my view, and according to Berzelius, two distinct genera, because they are expressed by two distinct formulæ, as is the case with orpiment and realgar; that is to say, that the gray mineral will be expressed by the formula $\ddot{Z}n\,\ddot{S}e^4 + HgS$. The red mineral by the formula $\ddot{Z}n\,\ddot{S}e^4 + HgS^2$.

Mexico, December 1, 1827. (Signed) A. DEL RIO.

On one occasion I distilled the mineral alone, and I poured spirit of wine into the receiver, when I observed at the bottom a drop of yellow oil, which in time tinged the alcohol of a beautiful yellow: on the addition of water, the colour disappeared without any precipitate being thrown down. I presume that this was the same substance noticed by Berzelius, as being formed on the admixture of selenic acid and anhydrous muriatic acid with selenium, in which case both these acids must exist in the mineral. I detected the muriatic by means of nitrate of silver; but no sensible precipitate of seleniate of silver was obtained by the addition of cold water to a boiling nitric solution, perhaps because the quantity was too small.

XVIII. *Observations on the Peruvian Andes, in reply to a Paper by* M. Coquebert de Montbret, *in the* Annales des Sciences Naturelles. *By* J. B. PENTLAND, *Esq.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE last Number of the *Annales des Sciences Naturelles,* (vol. xiii. p. 420) contains a paper by Mons. Coquebert de Montbret, entitled "*Note sur quelques Montagnes du Haut Pérou,*" purporting to be founded on a memorandum of some of my measurements of the Peruvian Andes, which I had communicated to that gentleman in February last.

The memorandum in question was drawn up, in the course of conversation, for Mons. de Montbret's private information; and I distinctly stated to him at the time that the results ought merely to be considered as approximative; since I had not the detailed notes of my observations at hand, and the calculations on which they were founded, required a careful revisal, before they were submitted to the public,—having been made in the midst of the fatigues and annoyances attendant on a tedious journey. I have therefore seen with regret, that a part of my observations has been rendered public in the unconnected and inaccurate manner in which they are brought forward in the paper in question: and I shall feel sincerely obliged by your giving an early insertion to this letter, in which I shall confine myself to the correction of some of the errors into which Mons. de Montbret has fallen, and to rebutting the conclusions drawn by this writer and Mons. Brué against the accuracy of my measurements.

The principal objection raised against the great elevation which I have attributed to certain peaks of the Peruvian Andes, situated between the 14th and 17th parallels of south latitude, consists in an assumption, that if the geographical position in which my astronomical observations have placed these mountains be correct, they must be easily seen from the coasts of the Pacific Ocean, and could not have hitherto escaped the attention of the navigators frequenting the ports of Peru situated in or near the same latitude.

To meet this inference, I take the liberty of annexing an extract from a letter, which, in my own justification, I have judged it necessary to address to Mons. Coquebert de Montbret, in reply to his observations:—

"The great chain of the Andes, between the 14th and 20th parallels of southern latitude, is divided into two longitudinal and parallel ridges, or *Cordilleras* (the name by which they are designated by the Creole population of Peru). These two cordilleras are separated by a very extensive interalpine valley, the mean elevation of which is 12,600 feet; its southern portion is traversed by the river Desaguadero, whilst its northern is occupied by the celebrated lake of Titicaca, on the shores and in the islands of which Peruvian civilization and the Empire of the Ingas had their origin.

"The western cordillera, or, as it is called, the Cordillera of the Coast, separates the valley of the Desaguadero (the Thibet of the new world), and the basin of the lake of Titicaca, from the shores of the Pacific. Many of its peaks exceed 20,000 feet in elevation, and in it are situated several active volcanoes; whilst the eastern cordillera, composed chiefly of transition and secondary rocks (grauwacke-slate and new-red-sandstone), separates the same valley from the extensive plains of Chiquitos and Moxos, and the confluents of the rivers Beni, Mamoré and Paraguay, from those streams which empty themselves into the lake of Titicaca, and into the river Desaguadero.

"The eastern cordillera of the Peruvian Andes is situated within the political limits of the Republic of Bolivia; and it presents, between the 14th and 17th degrees of latitude, an almost continuous ridge of snow-capped mountains, the mean elevation of which exceeds 19,000 feet. It is upon this snowy range of the eastern cordillera that rise the most elevated mountains hitherto determined throughout the entire extent of the Andean chain. The Nevados of Illimani and of Sorata (those referred to in Mons. de Montbret's paper) surpassing in height the giants of the Columbian prolongation of the Andes, Chimborazo, Cayambé and Antisana,—and approaching near to the most elevated peaks of the Himalaya range.

"The mountain of Illimani is situated in the Bolivian province of La Paz, twenty leagues E.S.E. of the city of the same name. Like Chimborazo it forms the most southern limit of the snowy range to which it belongs; and according to my astronomical observations, (made at La Paz, and at the hamlet of Jotoral near to its northern base,) it is placed between 16° 35′ and 16° 40′ south latitude; and between the 67th and 68th degrees of west longitude, reckoned from the meridian of Greenwich. Its summit forms an elevated ridge, surmounted by four peaks, disposed on a line from north to south and parallel to the axis of the chain. The most northern of these eminences attains an elevation, according to my measurement, of 24,200 British feet, or 12,000 feet above the city of La Paz; but the southernmost peak appeared to me to be still more elevated, although it was impossible to ascertain the exact difference from my station. This stupendous mountain is composed of grauwacke and transition-slate, with frequent interstratifications of quartz-rock and flinty-slate; which in their mineralogical structure and geological relations entirely resemble those of the valleys of the Maurienne and Tarentaise in the Savoy Alps: and with these schistose rocks are associated large masses of porphyry, of sienite, and of true granite, in the form of veins and beds. The transition-slate is traversed by numerous veins of quartz, containing minute portions of gold and of auriferous pyrites; many of which veins were worked by the aboriginal Peruvians, at an elevation of 16,000 feet above the level of the sea, at a very remote period, prior to the arrival of their Europæan invaders.

"The most eastern point of the coast of the Pacific, on the same parallel of latitude with the mountain of Illimani, is situated between the roads of Quilca (latitude 16° 42′), and the headland or Morio of Arequipa (latitude 16° 30′ S.); and between the meridians of 72° 40′, and 73° 20′ W. of Greenwich,—adopting a mean of the observations of Captain Basil Hall, and of Alessandro Malespina. Illimani is consequently separated from the nearest point of the coast of Peru, by an horizontal distance equal to 5° 30′ of longitude, or to 310 nautical miles. This fact in itself is sufficient to show the impossibility of discovering from the coast of Peru, that mountain, or indeed any part of the eastern cordillera of the Andes (the axis of which, between the 14th and 17th degrees of latitude,

tude, is nearly parallel to the meridian),—even supposing the intervening space to be perfectly horizontal, and not interrupted, as I have already shown it to be, by the elevated mass of the western cordillera,—some of the peaks of which, as well as the trachytic dome which towers over the valley of Chuquibamba, N.N.W. of Arequipa, rise to an elevation exceeding 22,000 feet*."

I am therefore at a loss to imagine how two gentlemen, possessing the acknowledged acquirements of Messrs. Brué and de Montbret, could have raised such an objection to the accuracy of my observations as that conveyed in the paper of the latter; since a reference even to the old and inaccurate map of South America, by Olmedilla de la Cruz, or to the incorrect compilation of Alcedo, must have rendered evident to the merest tyro in geographical science, the physical impossibility of descrying an eminence no more than 24,200 British feet above the level of the ocean, from a distance which exceeds 100 nautical leagues.

On the northern prolongation of the eastern cordillera of the Bolivian Andes, and nearly in the centre of the snowy range above mentioned, rises, in latitude 15° 30′ S., the Nevado of Sorata, from the midst of a group of snow-capped pinnacles, some of which attain an elevation of 23,000 feet. The Nevado of Sorata is situated to the east of the large Indian village of the same name, and is elevated 25,200 feet above the level of the sea, or 12,450 feet above the waters of the lake of Titicaca:—as deduced from a trigonometrical measurement taken from the shores of the lake, and from a determination (made at a less distant station) of the height of that portion of the summit which is placed above the superior limit of perpetual snow;

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* The city of Arequipa, one of the handsomest in South America, is situated at the western base of the western cordillera, in the midst of a fertile valley, watered by the streams of the Arequipa and Inquocajo, which descend from the adjoining Andes. The valley of Arequipa is bounded on its northern and eastern sides, by three snow-capped mountains; that in the centre, the volcano of Arequipa, resembling in form, and being nearly equal in elevation to Cotopaxi: whilst towards the south and west the valley is separated from the shores of the Pacific Ocean, by a low range of trachytic eminences; and by an arid sandy desert which occupies an extent of fifty miles in breadth. The mean of my observations, as deduced from an extensive series of meridian altitudes of *Achernar, Canopus, α Arietis, Capella,* and *Saturn,* places the house of the British consulate at Arequipa in latitude 16° 23′ 58″; and in longitude 71° 20′ 0″ W. resulting from observations made with two good chronometers, and from several sets of lunar distances. The elevation of Arequipa above the level of the neighbouring ocean, is 7797 feet; being the mean of 170 barometrical observations, made during thirteen successive days with an excellent barometer by Fortin, and calculated according to the formula of Laplace.

a limit which, between the 15th and 17th degrees of south latitude, and on the sides of the Bolivian Andes, seldom descends below 17,100 feet above the sea.

The great mass of the eastern cordillera, situated north of the parallel of 17° S. is likewise formed of the transition rocks above enumerated; the sienitic or crystalline rocks becoming more abundant on its northern prolongation. The schistose rocks here also abound in auriferous veins; and through the deep dells which intersect them, descend the numerous auriferous torrents, which empty themselves into the river Beni and its confluents, and give to the tropical district bordering on the river of Tipuacio (in the province of Larecaja), the fairest claim to the title of the El Dorado of the new world,—from the great quantities of gold, which have been and are still collected from the alluvial deposits that form its banks.

I am, Gentlemen, yours, &c.

June 25, 1828. J. B. Pentland.

XIX. *On the Penetration of Water into stoppered and corked Bottles sunk to a great Depth in the Sea. By* J. de C. Sowerby, *Esq. F.L.S. &c.*

To Richard Taylor, Esq.

Dear Sir,

MANY papers having at different times appeared upon the popular paradox, a bottle filling with water when sunk to a great depth in the sea, however well it may have been corked and sealed, without any satisfactory explanation having been given, and seeing the subject resumed by Dr. Green, in your Philosophical Magazine for this month,—I am induced to send you my explanation of the phænomenon.

Dr. Green thinks that by proving (as others had done) that the water would not penetrate glass, he had reduced the question into very narrow limits; and that the water enters glass vessels through the "cork and all its coverings in consequence of the vast pressure of superincumbent water, in the same manner as blocks of wood are penetrated by mercury in the pneumatic experiment of the mercurial shower."

It may be concluded from recorded experiments, that well-fitted glass stoppers (by the bye, every chemist knows such are rarely to be obtained as will confine the vapour of nitric acid) will exclude the water; corks when properly protected will also prevent the water from entering. When mercury is made to pass through a block of wood by pneumatic pressure, it finds its way by the longitudinal tubes; such tubes do not exist in cork. My explanation is this; cork is elastic, and by the pressure of the sea is readily condensed, and consequently much diminished in bulk, first that part out of the bottle where the sides are not protected by the neck, and then gradually the remaining length until the cork, separated entirely from the glass, affords a free passage for the water, unless the sealing or wrapper be of such a tenacious and ductile nature as to adhere to the glass and the cork so as to fill up the space that would otherwise be left, and yet not yield completely to the pressure; if it be brittle, it either separates from the glass, or cracks, or both, allowing a free passage to the water. Even pitch when cooled in the deep water would be very brittle and crack or separate from the bottle readily, and it would resume its former ductility and appearance upon returning through the warm surface: this and similar considerations will show how a trifling difference in closing the bottles may produce considerable differences in the results of the experiments. I remain, yours truly,

2, Mead Place, Lambeth, July 12, 1828. J. DE C. SOWERBY.

XX. *Some further Remarks on* Messrs. Tiedemann *and* Gmelin's *Observations on the Acids of the Stomach. By* WM. PROUT, *M.D. F.R.S.*[*]

THE observations of Messrs. Tiedemann and Gmelin on my paper published in the last Number of the Philosophical Magazine and Annals, seem to me to be intelligible only on the two following assumptions. First, that the method employed was adopted at random and without any preliminary inquiry, and was intended to include every possible case; and secondly, that on the faith of this random method, *I denied generally and under all circumstances* the existence of every other acid except the muriatic acid, in the stomachs of animals. Now whether these assumptions can be fairly drawn from my paper, I, as an interested individual, can scarcely, perhaps, be admitted as competent to decide; but I can truly say at least, that I never intended that such inferences should be drawn, nor was aware that any thing had been stated to authorize them.

With respect to the first of these assumptions it may be said, that the nature of the gastric fluids, and especially the acid, had occasionally occupied my particular attention for many years; and that during the summer before my paper was published, I had set about the inquiry in earnest, and with the determination,

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[*] Communicated by the Author.

nation, if possible, of putting the matter at rest. With this view a number of animals were fed in various ways; that is to say, on substances both natural and unnatural to them, and the contents of their stomachs subjected to analysis. The examination was conducted in the most rigorous manner, and varied in every possible way that I could devise; and up to the period at which my paper was sent to the Royal Society, I completely satisfied myself, that in every instance the acid present was the muriatic acid and no other, at least in any appreciable quantity. Now it was in the knowledge thus previously acquired, and not at random, that the method proposed was founded; and among a variety that were tried, the one in question was ultimately chosen as comprehending every point that had then occurred to me. If it be objected that these preliminary experiments ought to have been given, I can only say, that I did not at the time think this necessary, nor do I now. The muriatic acid was not a new substance, nor one difficult to be identified: besides, such a preliminary inquiry seemed to be sufficiently indicated by the method proposed; for who would ever think of proposing a formal method of analysis, involving the *quantities* of substances, without determining beforehand what those substances were? Further, my paper was intended to be little more than a simple announcement of an important fact, which, before it could be established, I well knew must be corroborated by other experience than mine; and lastly, something must be ascribed to a sort of innate antipathy to long-winded dissertations, which is too apt to cause me to err on the side of brevity.

Messrs. T. and G. observe, that considering my method quite perfect, I infer from it the absence of all other acids, except that of the muriatic acid in the gastric fluids. To this I answer, that under the circumstances to which it was applied, I considered it then, and do still, as quite perfect: and as the residuum after combustion could not have been neutral if the acid had been of a destructible nature, because the quantity of potash required to saturate the free acid was more than sufficient to decompose the whole of the muriate of ammonia present,—the argument even in this point of view was strictly correct, though acknowledged to be imperfect if applied generally*. This argument was given, because it was the only one bearing on the point in question that was strictly deducible from the method employed; and more could not have been well said without destroying the unity of my design, and entering on details which, for the reasons above stated, I concluded would have been taken for granted.

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* Messrs. T. and G. will, I trust, give me credit when I assert that I was perfectly aware of all the chemical objections they have raised, and many more to the same effect; and never should have thought of applying the method in question in a new case when the nature of the acid was unknown, and particularly in the case of a destructible acid in conjunction with the muriate of ammonia. The fact was, that I detected free muriatic acid

With respect to the second assumption; namely, that I denied generally, and under all circumstances, the existence of every other acid in the stomachs of animals, except the muriatic acid,—I can only say, that nothing was further from my intention. On the contrary, I distinctly alluded to the "occasional presence of other acids in the stomach," taking it for granted that such an occurrence must sometimes happen. What I did assert, and what I again assert is, that in the cases related, and in all others in which a rigorous examination was instituted up to the period mentioned, no other acid did occur in any appreciable quantity; and I acknowledge that in consequence of this experience, I was induced to conclude that the presence of other acids was comparatively of rare occurrence, and my subsequent experience decidedly favours this conclusion. I have already said, that since my paper was read before the Royal Society, I have occasionally, by means precisely similar to those formerly employed, detected the presence of combustible acids in the stomach, and have expressed a belief that these acids were probably derived from the food; and in several of the instances, I have no doubt this was the case. I wish however by no means to be understood to deny that the stomach occasionally secretes a combustible acid in a free state*, though I think

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acid in a fluid ejected from the human stomach so long ago as 1820, but then thought that its presence was accidental, or that by some means or other I had deceived myself; and when I commenced the experiments in question, I was actually prejudiced in favour of a destructible acid, viz. the *lactic* acid of Berzelius (though the distinct nature of this acid always, I confess, appeared to me to be somewhat problematical). In consequence of this prejudice, therefore, the inquiry was conducted in a much more rigorous and elaborate manner than it probably otherwise would have been; and after a series of the most complete evidence that perhaps was ever brought to bear on a chemical point, I was obliged to conclude, is opposition to my preconceived notion that the acid was the muriatic and no other. On reflecting, however, on this most unexpected fact, I soon saw its importance, and that, in short, it was one of those leading facts that opens up an entire new field of inquiry. So satisfied indeed was I of this, that a work on the digestive functions, in which I had been long engaged, and which I had actually begun to print, was suppressed; and since that time I have been engaged in an entire new field of research, which I fear will yet occupy me for several years to come.

* Within the last few months I have seen a very remarkable case of disease, where the acetic acid seemed to be formed, not only by the stomach, but the salivary glands, &c. in great abundance. In this case the breath of the

I think it more frequently happens that some salt containing a combustible acid, *e.g.* the acetate of soda, is actually secreted; and that this, by being decomposed by the free muriatic acid, gives origin to the *apparent* presence of free acetic acid.

In conclusion, it may be observed that, during the long period that my attention has been turned to this interesting subject, a great many curious and most important facts have come to my knowledge: in some of these I have been anticipated by Messrs. T. and G.; while others appear to have escaped their observation, or probably did not occur to them. But when I make this statement, I wish it to be distinctly understood that I am very far from accusing these gentlemen of chemical ignorance, because they failed to point out what probably was not present in the substances they examined, or of charging them with denying, generally, the existence of every thing else that did not happen to fall within the limits of their own observation;—charges which these gentlemen, from not sufficiently attending to the general character of my brief announcement, have inadvertently brought against me under very similar circumstances.

XXI. *On the vitrified Fort of Dunnochgoil, in the Isle of Bute.*

By Samuel Sharpe, *Esq. F.G.S.*[*]

THE fort is on a rocky point at the south-west corner of the Isle of Bute, perhaps the point nearest to the Isle of Arran. It is at some distance from trees, habitations, and higher ground.

There remains now little more than the ground-plan, which may be traced by the vitrified foundations; but at one part the wall is more than a foot high, built of rough stones not much larger than bricks, and by vitrifaction formed into one solid mass, much like the slag of a furnace.

The parts can best be described by reference to the following figure.

From *q* there is a gradual ascent to the outer chamber *efgh*, which appears to have been surrounded on two sides

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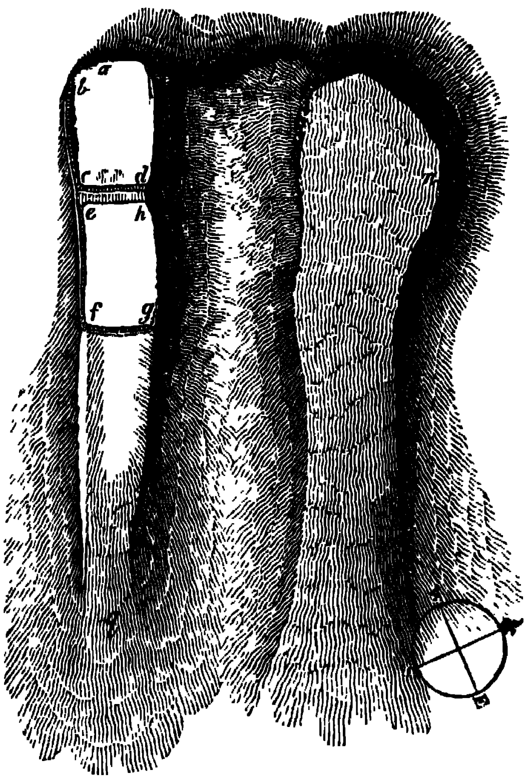
the patient smelt strongly of vinegar; the saliva and fluids occasionally ejected from the stomach contained also the same acid in abundance, as apparently did the perspirable fluid; for the whole body exhaled a strong odour somewhat like sour milk: during this time the urine was strongly alkalescent. In another anomalous case, I have seen the blood itself strongly acid; the acid was of a combustible nature, but from peculiar circumstances it was not satisfactorily proved to be vinegar, though this was probably the case.

[*] Communicated by the Author.

ef

e f and *f g* by vitrified walls. Between the outer chamber and the inner one, *a b c d*, there is a slight descent, which may however formerly have been a ditch of some depth. This chamber was apparently fortified by vitrified walls, not only outwards on the sides *a b* and *b c*, but also on the side *c d* against the outer chamber.



The remains of the wall are mostly little more than foundations, but for part of the way between *b* and *c* it is more than a foot high.

I found no traces of art to prove that the neighbouring height *n* was any part of the fort, though it is made probable by the absence of all remains of wall on the side *a d h g*. The walls were probably only two or three feet thick, which, at least on three sides, was all that was necessary where the situation made them only accessible to missiles; and if there were originally any others besides those mentioned, they were probably not vitrified, as no traces of them are now apparent: the ground below is scattered with fragments of rock, some of which doubtless formed the walls.

The heights were estimated by guess, and the distances by pacing, and have no claims to exactness.

a b perhaps 70 feet above the shore, nearly perpendicular.
b c e f ditto, not so perpendicular.
l 15 } a rather steep ascent.
n 40 }
a d and *h g* 40 nearly perpendicular.

Between *d* and *h* the side is kept perpendicular by building, without vitrifaction or apparent cement. Each chamber is about 40 paces long, and 25 paces wide, the space between the chambers 3 paces, the gradual ascent from *q* above 100 paces.

The sides *b a b* and *b f q* are each about 100 yards from the sea; and near *b* are the traces of a landing-place on the beach, which however must be either modern or accidental, as they could hardly have withstood the waves for so many centuries.

I have nothing to add to the received opinion, that it must have been built before Roman arts and civilization (and in particular the use of mortar) travelled so far north.

Dr. Macculloch, after describing in the Geological Transactions, vol. ii. the Fort of Dun Mac Sniochan near Oban (which I had intended to visit, but being hindered, visited this in Bute instead), combats at length and successfully the opinion, that the vitrifaction was the effect of natural causes; but I think the opinion could never have been held by one who had seen this fort in Bute, where the traces of art are so evident and so undeniable.

The wall must have been first built, and then made compact and solid by vitrifaction, which must have required a considerable fire to be moved from place to place, as the work proceeded.

Samuel Sharpe.

XXII. *Method of solving adfected Quadratic Equations. By Mr.* Joseph Seers; *in a Letter to Mr.* Peter Nicholson*.

Dear Sir,

I BEG leave herein to submit to your inspection, &c. the method I discovered and mentioned to you, about two months ago, of solving adfected quadratic equations. I flatter myself it is quite new; and I think it inferior to none in present use. It is as follows:

Whatever be the original form of a quadratic equation, it must always be reduced to this formula of three terms; viz. $x^2 \pm px \pm q = 0$.

In this formula, it is to be observed that p is the sum of the root, and that q is their product. And having their sum, and substituting (d) for their difference, we have, by a well-known theorem, the two roots in this expression $\frac{\mp p \pm d}{2}$; in which expression the sign of p is always contrary to what it is in the above formula. Moreover, we have, as before observed, $\frac{p+d}{2} \times \frac{p-d}{2} = \pm q$. In which equation $d = \pm \sqrt{p^2 \mp 4q}$: here the sign of q is contrary to what it is in the formula. Hence, $\frac{\mp p \pm d}{2} = \frac{\mp p \pm \sqrt{p^2 \mp 4q}}{2}$: an expression containing the two roots of the given equation in terms of known quantities.

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* Communicated by Mr. P. Nicholson.

Example:—Given $x^2 - 17x + 35 = 0$. To find the values of x.

$$\text{Here } x = \frac{17 \pm d}{2}. \text{ Moreover, } \frac{17+d}{2} \times \frac{17-d}{2} = 35.$$

$$\text{Hence, } 17^2 - d^2 = 140 \;.\;.\;.\;.\;.\; \text{or, } d^2 = 149.$$

$$\therefore d = \pm \sqrt{149} = \pm\, 12.206 + \&c.$$

$$\therefore x = (17 + 12.206 + \&c.) \div 2. \text{ Or, } (17 - 12.206 + \&c.) \div 2. = 14.603 + \&c., \text{ or } 2.397 + \&c.$$

I will not trouble you with any more examples, as there is no need. I remain, Dear Sir,

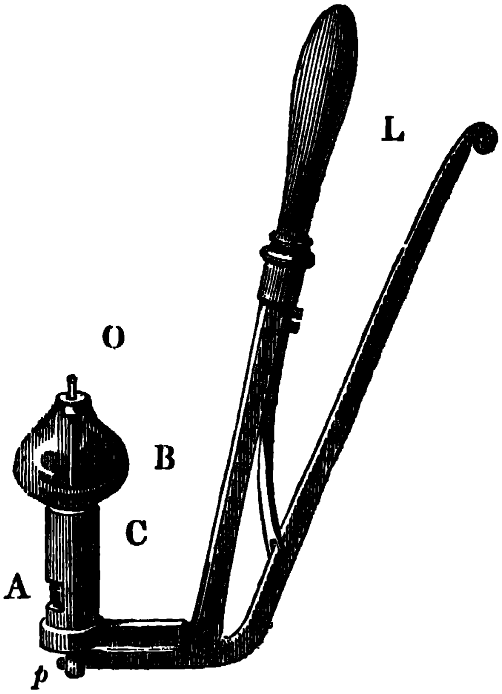
Your most obedient and much obliged servant,

Chelsea, June 20, 1828. JOSEPH SEERS.

XXIII. *Improved Eudiometrical Apparatus.* By R. HARE, *M.D. Professor of Chemistry in the University of Pennsylvania**.

I. *Piston Valve Volumeter.*

I HAVE contrived some instruments for taking volumes of gas at one time, precisely equal to those taken at another time. I call them volumeters, to avoid circumlocution. They are of two kinds, one calculated to be introduced into a bell glass, over water, or mercury; the other may be filled through an orifice, as is usual in the case of filling a common bottle over the pneumatic cistern. The annexed figure will convey a due conception of one of them, which having a piston, I call the piston valve volumeter.



The lever L is attached by a hinge to a piston *p*, which works inside of a chamber C. The rod of this piston extends beyond the packing through the axis of the bulb B to the orifice O in its apex, where it sustains a valve, by which this orifice is kept close, so long as the pressure of the spring, acting on the lever at L, is not counteracted by the hand of the operator.

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* From the American Journal of Science, with corrections and additions by the Author.

Suppose that while the bulb of this instrument, filled with water or mercury, is within a bell glass, containing a gas, the lever be pressed towards the handle, the valve is consequently drawn back so as to open the orifice of the apex of the bulb, and at the same time the piston descends below an aperture A in the chamber. The liquid in the bulb will now of course run out, and be replaced by gas, which is securely included, as soon as the pressure of the spring is allowed to push the piston beyond the lateral aperture in the chamber, and the valve into the orifice O, in the apex of the bulb.

The gas thus included may be transferred to any vessel, inverted over mercury or water, by depressing the orifice of the bulb below that of the vessel, and moving the lever L, so as to open the aperture A in the chamber, and the orifice of the bulb simultaneously.

The bulk of gas, included by the volumeter, will always be the same; but the quantity will be as the density of the gas into which it may be introduced. Hence in order to measure a gas accurately, the liquid, whether water, or mercury, over which it may be confined, should be of the same height within, as without. This is especially important, in the case of mercury, which being nearly fourteen times heavier than water, affects the density of a gas materially, even when its surface within the containing vessel does not deviate sensibly from the level of its surface without.

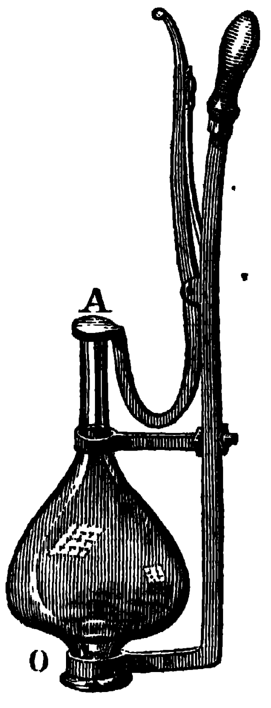
To remove this source of inaccuracy, I employ a small syphon gauge which communicates through a cock, in the neck of the bell, with the gas within. In this gauge any light liquid will answer, which is not absorbent of the gas. In the case of ammonia, liquid ammonia may be used; in the case of muriatic gas, the liquid acid.

The density of the gas will be *in equilibrio* with that of the air, when the bell is supported at such a height as to cause the liquid in each tube of the gauge to be in the same level.

II. *Simple Valve Volumeter.*

Besides the lower orifice O, by which it is filled with gas, the volumeter which the next figure represents, has an orifice at its apex A, closed by a valve attached to a lever. This lever is subjected to a spring, so as to receive the pressure requisite to keep the upper orifice shut, when no effort is made to open it.

When this volumeter is plunged below the surface of the water of a pneumatic cistern, the air being allowed to escape, and the valve then to shut itself under the water, on lifting the vessel it comes up full of the liquid, and will remain so, if the lower orifice be ever so little below the surface of the water in the cistern. Thus situated, it may be filled with hydrogen, proceeding by a tube, from a self-regulating reservoir. If the apex A be then placed under any vessel, inverted duly in the usual way, the gas will pass into it as soon as the valve is lifted.

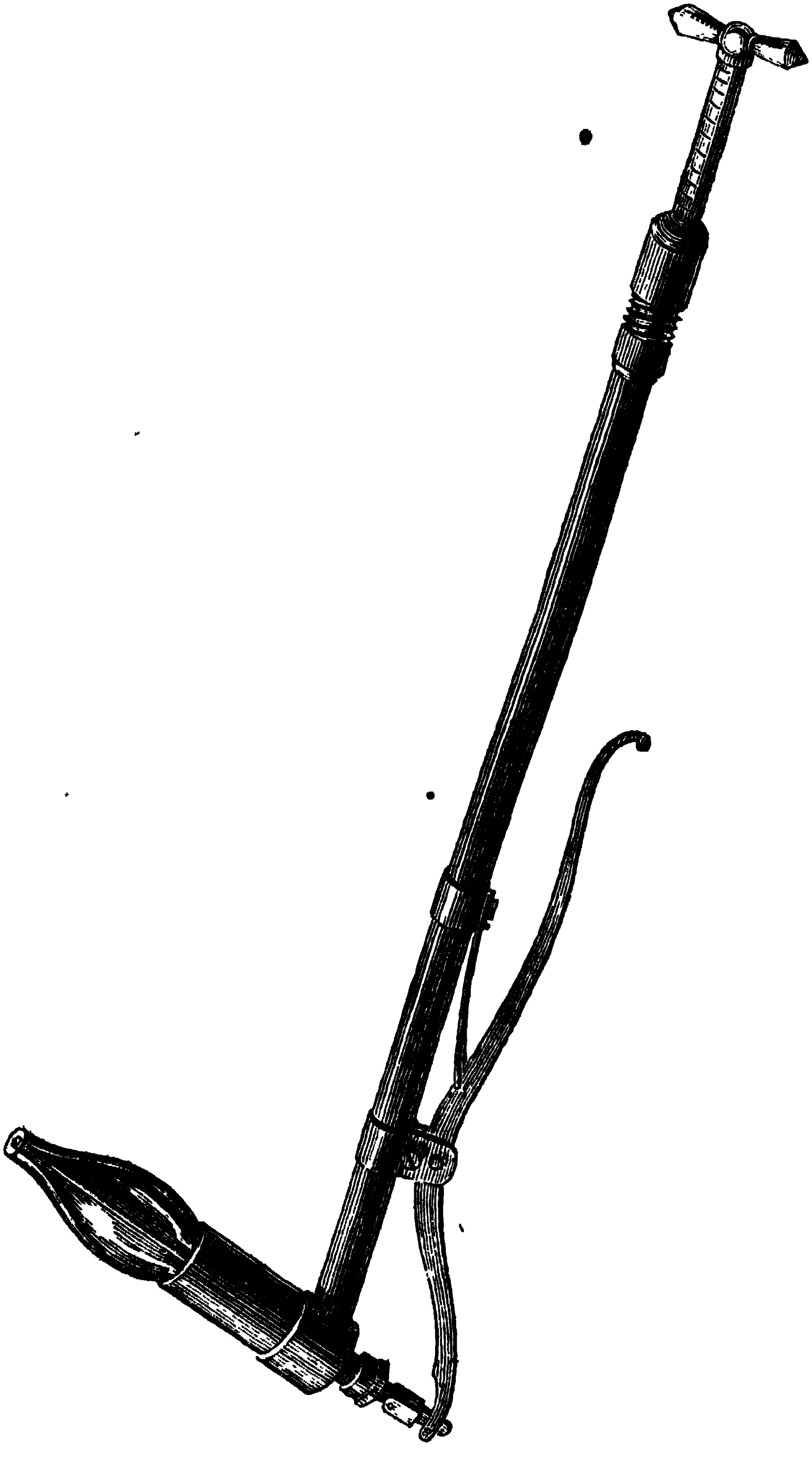


Volumes of atmospheric air are taken by the same instrument, simply by lowering it into the liquid of the cistern, placing the apex under the vessel into which it is to be transferred, and lifting the valve: or preferably by filling it with water, and emptying it in some place out of doors where the atmosphere may be supposed sufficiently pure, and afterwards transferring the air thus obtained, as above described, by opening the valve while the apex is within the vessel, in which the mixture is to be made. In this case, while carrying the volumeter forth and back, the orifice must be closed. This object is best effected by a piece of sheet metal, or pane of glass.

It is necessary that the water, the atmosphere, and the gases should be at the same temperature during this process.

III. *Sliding Rod Gas Measure.*

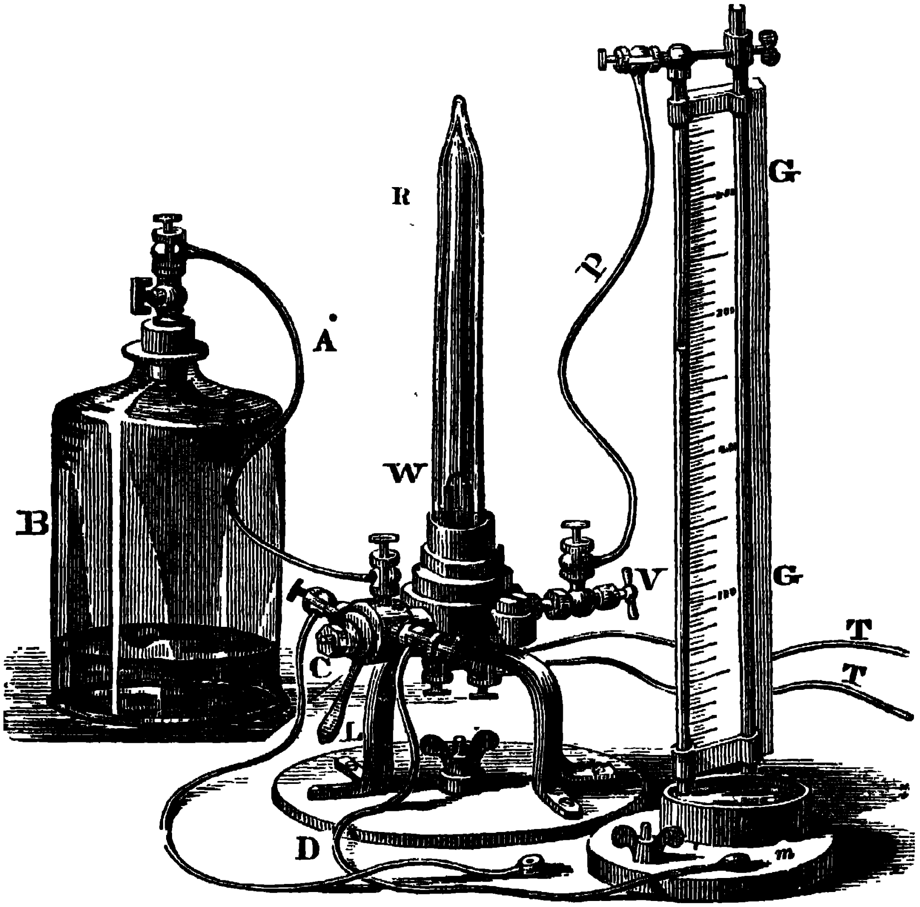
The construction of this instrument differs from that of my sliding rod eudiometers, in having a valve which is opened and shut by a spring and lever, acting upon a rod passing through a collar of leathers. By means of this valve, any gas, drawn into the receiver, is included so as to be free from the possibility of loss, during its transfer from one vessel to another. This instrument is much larger than the eudiometers for explosion, being intended to make mixtures of gas, in those cases where one is to be to the other, in a proportion which cannot be conveniently obtained by taking more or less volumes of the one than the other, by means of the volumeters: as for instance, suppose it were an object to analyse the air according to Dr. Thomson's plan of taking 42 per cent of hydrogen. The only way of mixing the gases by a volumeter, in such a



ratio, would be to take the full of the volumeter, 21 times of hydrogen, and 50 times of atmospheric air. By the large sliding-rod instrument, this object is effected at once by taking 42 measures of the one, and 100 measures of the other.

IV. *Barometer-Gauge Eudiometer.*

The following is an engraving of the barometer-gauge eudiometer for explosive mixtures. R is a glass receiver. Within the receiver near W is an arc of platina, by the ignition of which the gas is inflamed. C is a cock with three orifices, either of which may be made to communicate with the receiver, according to the position of the lever L. More than one of the orifices cannot be open at once, but all may at the same time be closed. The barometer-gauge GG is seen beside the receiver, with which it communicates through the pipe P, and the valve cock V, by means of which the communication between the gauge and receiver may be suspended at pleasure. The pipe A conveys to the receiver the gaseous mixture from the bell-glass B. By one of the pipes D, a communication



munication with the air-pump may be established. The other pipe is used when different kinds of gas are to be successively introduced, or when a portion of residual gas is to be drawn out for examination. T, T are rods, by means of which the platina wire communicates with the poles of a calorimotor, so as to be ignited, by being the medium of discharge, as often as the surfaces are excited by the acid. The calorimotor* employed is so constructed, as that the revolution of a wheel through a quarter of a circle, is sufficient to raise the vessel holding the acid until the galvanic plates are surrounded by it. At *m* is a wooden dish holding mercury for the gauge-tube.

It is well known to those who are familiar with pneumatics, that if a receiver communicate simultaneously with an air-pump and a barometer-gauge, the extent of the exhaustion will be indicated by the height of the mercury in the gauge-tube; so that if there be a scale of equal parts associated with the tube, the quantity of air taken from the receiver at any stage of the exhaustion will be to the quantity held by it when full, as the number opposite the upper extremity of the mercurial column, when the observation is made, to that to which it would extend if the receiver were thoroughly exhausted.

Hence, if on exhausting the vessel thoroughly the mercury rise 450 degrees, on admitting a gas freely, 450 parts of the gas would replace the air withdrawn; but if the entrance of the gas be restricted, so that a mercurial column is still sustained in the tube, the quantity of gas which has entered will be as much less than 450, as the mercury is above 0. Thus for instance, let the mercurial column sustained extend to 150 on the scale, 300 parts of gas will have entered, and if by explosion or any other means any number of parts of the gas, thus introduced, be condensed, the mercury must rise that number of degrees in the gauge†.

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* See Phil. Mag. vol. liv. p. 209.

† That portion of the bore of the tube which is not occupied by mercury, adds to the capacity which influences the gauge, and the portion of the gauge which is emptied of mercury varies in extent; but as the air which remains in the gauge is not subjected to the explosion, the extent of the condensation is uninfluenced by it.

A slight error may arise from the sinking of the mercury in the dish, as the quantity in this receptacle lessens by its rise in the tube; and *vice versâ* when subsidence ensues. This movement will be to the movement of the mercurial column in the tube, as the square of its diameter to the square of the diameter of the mercurial stratum in the dish; and the diameters of these being respectively as 20 to 1, it would be 1-400 of the whole height of the scale: this difference may be allowed for, or may be remedied, by raising or lowering the dish, by an appropriate screw, or employing a dish of a superficies so large, and a gauge-tube with a bore so small, as to render the effect of the rise, or subsidence of the mercury in the gauge, insignificant.

The receiver is a stout glass tube, which tapers from two inches, in diameter, internally, to one inch; being open at the larger end, at the smaller end closed. This form was adopted as combining strength, to resist explosions, with a capacity to hold larger quantities of gas than have heretofore been exploded in eudiometers. It must be evident that the larger the quantities of gas operated with, the less upon the whole will be the influence of any minute leakage, or error in measurement.

The tube is cemented at the larger end into a brass ferrule, which is screwed into a casting of the same metal, furnished with iron feet. Into the same casting, a brass plug screws, through which are inserted stout wires, one of them insulated, for producing galvanic ignition, in an arc of platina wire, as already described in the case of my other eudiometers *.

With the gauge-tube, is associated a scale divided into 450 equal parts. Instead of inhaling successively due portions of hydrogen and atmospheric air, as heretofore described, I have found it better to mix them previously in known volumes, by means of the volumeters, described in the preceding articles. Having by the aid of one of those instruments made a mixture of one part of hydrogen, with two of atmospheric air, it follows, that if 300 measures be taken by a sliding-rod eudiometer, or other adequate means, there will be a mixture, in the quantity so taken, of 200 parts of atmospheric air, and 100 of hydrogen. In case equal volumes of these aëriform fluids be mixed into one bell-glass, 200 measures would contain 100 of each. This mode of procuring such mixtures, is preferable from its saving trouble, and lessening the chances of error in the measurement; and because the gaseous fluids become more thoroughly blended,—a result which does not follow their admixture as immediately as might be expected.

Having prepared a mixture of two volumes of atmospheric air with one of hydrogen, and the receiver being exhausted as far as practicable, if any small quantity of the mixture be exploded in it, by exciting ignition in the platina wire W, all the oxygen will be condensed. The residuum, consisting of hydrogen and nitrogen, will not interfere with the result of any subsequent experiment, although the receiver should not be thoroughly exhausted. Under these circumstances, let the

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* One of the greatest difficulties which I encountered, was in the imperfection of stop-cocks, in the common form. This I obviated by two contrivances of my own; one invented about sixteen years ago, the other in the summer of 1825. Of these I shall publish a description, with engravings, as soon as I conveniently can.

exhaustion be carried to 400 degrees, and let 300 measures of the mixture enter, so as to depress the mercury in the gauge to 100 on the scale. An explosion being effected, the mercury in the gauge will rise at first to about 215 degrees, and after the gas shall have regained its previous temperature, will be found somewhat above 220 degrees.

Of course a deficit will have ensued of more than 120 parts, of which one-third, or a little more than 40 parts, will be the quantity of oxygen in 200 parts of the air, subjected to analysis.

In order to ascertain the influence of temperature, a thermometer is placed in the receiver, the state of which is noted before and after explosion; and the deficit is estimated either by allowing for the difference produced by the temperature, or awaiting the refrigeration until the mercury in the thermometer be at the same height as before the explosion.

From this account of the barometer-gauge eudiometer, and those previously given of the sliding-rod instruments*, it must be evident that I have contrived three methods of analysing the atmosphere, or other mixtures containing oxygen or hydrogen gas.

In the barometer-gauge instrument, the deficit is known by its effect upon the mercury in the gauge-tube; in one of the sliding-rod instruments, the deficit is compensated by water, and the quantity of this liquid which enters for this purpose, is known by the portion of the sliding rod which remains without, after excluding the residual gas. In the instrument with the sliding rod and gauge, the deficit is compensated by introducing the rod, the gauge enabling us to know when it has been introduced sufficiently; while the graduation shows the ratio of the gaseous matter condensed, to the quantity confined.

When the diversity of these methods is considered, it is pleasing to observe but little difference in the results obtained by them.

A great number of experiments performed by means of the barometer-gauge eudiometer, or those of the sliding-rod construction, over water and over mercury, gave 20 $\frac{66}{100}$ as the quantity of oxygen in 100 parts of the air. In twenty experiments the greatest discordancy did not amount to $\frac{1}{1000}$th part in 100 measures of air.

In lieu of the glass receiver a strong metallic vessel may be used, as for instance, one of the iron bottles employed to contain mercury. The igniting wire may be placed so as to be visible in a very stout glass tube projecting from the bottle.

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* See Phil. Mag. vol. liv. p. 209.

But a glass tube is not necessary, as, without seeing the ignition, the explosion will be known to take place by the noise which it makes, and the movement of the mercury in the gauge.

[To be continued.]

XXIV. *Notices respecting New Books.*

An Appendix to the First Volume of an Introduction to Practical Astronomy. By the Rev. W. PEARSON, LL.D. F.R.S. Treasurer of the Astronomical Society.

THE Rev. Dr. W. Pearson has just published an Appendix to his First Volume of An Introduction to Practical Astronomy, containing eleven sheets; in which he has shown that his series of XIV Tables, computed from constants of aberration and nutation approved by Zach, Delambre, Bessel, &c. will determine the corrections due to any other coefficients that may be deemed preferable, in the present improving state of practical astronomy; and without altering the mode of computation otherwise than by the introduction of one constant logarithmic factor in each operation, which converts one result into the other; and thus renders these tables *permanent* under all the changes of coefficients, which future observations may render necessary. The author has also taken occasion to correct some erroneous computations that had before escaped his notice; and has given an additional list of typographical errata. He has likewise added a catalogue of 520 zodiacal stars, including their subsidiary numbers, for facilitating the computation of their corrections; as well in longitude and latitude, as in right ascension and declination; all which stars are subsequently arranged in the order of right ascension, in a table of fourteen quarto pages, in such a manner, that it exhibits those stars in succession that are liable to suffer occultation at any given time, by having reference only to the moon's longitude and place of her node, as given in the Nautical Almanac: and what renders this classification extremely convenient, those stars that will suffer an occultation as observed in England, or in the same parallels of latitude, are distinguished from those that will be seen occulted on other parts of the globe.

We are authorized to state, which we have much pleasure in doing, that the second volume, describing the instruments used in practical astronomy, with the methods of adjusting and using them, is in a state of great forwardness, and will probably be ready for publication in the course of three months from the present time.

On the Curative Influence of the Southern Coast of England, especially that of Hastings: with Observations on Diseases in which a Residence on the Coast is most beneficial. By WILLIAM HARWOOD, M.D. London, 1828; post 8vo, pp. 326.

THIS work is an interesting and useful combination of scientific and medical information, adapted to the use of invalids who are desirous of benefiting by the invigorating breezes of our southern coasts, as well as to that of their medical advisers. It describes the peculiarities of climate, with respect especially to their influence on disease, which are presented on the southern coasts, and discusses, with considerable success, some of the causes of this influence. It comprises at the same time a general view of the various physical characteristics of the district to which it relates; such as is calculated to entertain the mind of the invalid who may consult it. To some of the observations on the effect of atmospheric variations on the human body, we do not feel disposed to subscribe; but the reasoning on this subject does not affect the general merits of the work. The contents of the work are arranged in the following order:

The varied nature of our coasts; causes tending to affect the temperature of coast-situations, more especially that of the southern coasts; the Hastings coast, its geological character, choice of situation it affords to invalids in elevation and in temperature, its other peculiarities, table of its temperature; on the air; influence of atmospherical variations on the constitution; on the effects of sea-air; on the water of the southern coast; general observations on bathing; on cold sea-bathing, its effects on the constitution, circumstances which render it inadmissible, precautions in its employment; warm sea-water bathing, its operation on the system; shower and vapour bathing, observations on indigestion and hypochondriasis; acute rheumatism; chronic rheumatism; gout; consumption; winter cough; asthma; hæmoptysis; diseases of the liver; the effects of mercurial medicines; the effects of loss of blood; other causes of debility; diseases of children; scrofula; rickets; marasmus; spasmodic diseases; hooping-cough; measles; diseases of the skin, &c.; notice concerning the chalybeate waters of the Hastings coast.

The Royal Society of Göttingen have published a new volume of their Transactions, entitled "Commentationes Societatis Göttingensis." It contains three papers by Professor Gauss:—1. Theoria residuorum biquadraticorum.—2. Supplementum theoriæ combinationis observationum minimis erroribus obnoxiæ.—3. Disquisitiones generales circa superficies curvas*.

Professor Gauss has likewise published (in German) his "Determination of the difference of latitude of the observatories at Göttingen and Altona," being the astronomical part of his measurement of an arc of the meridian.

The Berlin "Astronom. Jahrbuch for 1830," has been published for the first time by Prof. Encke, on an extended and improved plan. It contains, besides the ephemeris, four papers by Prof. Encke on the calculations of occultations of Stars; on Interpolation; on Sextants, and on Transits†.

The well-known eccentric Dr. Gruithuisen, now Professor of Astronomy at Munich, who has so frequently amused the public by his discovery of flat-roofed buildings, palm-groves, and macadamized roads in the moon, has begun publishing a Journal, devoted to astronomy and geography, under the title "*Analecten fur Erd-und Kimmels Kunde.*"

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* See Phil. Mag. and Annals, N. S. vol. iii. p. 331.

† See p. 141 of the present Number.

The first paper is a minute description of every part of a zenith telescope, for the use of a Katachthonian observatory to be built sixteen German (seventy-four English) miles below the surface of the earth!!!

XXV. *Proceedings of Learned Societies.*

ASTRONOMICAL SOCIETY.

June 13.—PROF. STRUVE communicated the result of his observations and measurements of the apparent distance between the body and the ring of Saturn: from which it appears that he is decidedly of opinion that the body of Saturn is not in the centre of the ring. From a mean of 15 measurements, he makes the apparent distance on the left side equal to 11″·272, and on the right side equal to 11″·390: the difference is 0″·215. The probable error of these mean measurements he considers equal to 0″·024. M. Struve adds some slight corrections to his former measurements of the diameters, &c. of Jupiter, of Saturn, and of the Ring; which he has deduced from a more accurate determination of the value of his micrometer.

The next communication was from Mr. Prinsep of Benares, giving an account of *two* eclipses which he had observed at that place in the course of the preceding year. The *first* was a solar eclipse on April 26, 1827. The commencement was not observed; but during the course of the eclipse a number of micrometrical measurements were taken by means of the five horizontal wires of a Troughton's 18-inch circle: and he states the end of the eclipse to have taken place at $20^h\ 3^m\ 7^s,5$ mean time at Benares. Mr. Prinsep then adds, "At the period marked as the end of the eclipse, the sun's disc was clear of the moon: but, for 10 or 15 seconds later, I remarked, as it were, a stretching of the sun's edge toward the point which the moon had just quitted. This was apparently the effect of refraction by the moon's atmosphere." The end of this eclipse was also observed by Mr. Walter Ewer at Cawnpore, at $19^h\ 56^m\ 3^s,5$ mean time at that place. The *second* eclipse, alluded to by Mr. Prinsep, was of the Moon, on November 3, 1827. It was observed in the same manner as the solar eclipse above mentioned. The following is the result of his observations, stated in mean time:

		h	m	s
The edge became dull at		8	42	0
The edge invisible		8	44	18
The edge of the same colour as the sky		8	45	30
Decided shadow		8	46	1
A bright spot becomes invisible		8	52	26
Immersion of a small star		10	42	14,2
Moon's transit	1st limb	11	45	48,2
Do. do.	2nd limb	11	47	57,1
End of the eclipse		12	1	6

Mr. Walter Ewer observed the beginning of this eclipse at Cawnpore, $8^h\ 35^m\ 23^s$ mean solar time at that place: the difference of longitude is assumed equal to — 10^{m} 12^{s}. Diagrams, illustrating the several phases of the eclipse, and apparently drawn up with considerable care, accompanied these communications.

Mr. Rumker of Paramatta, in New South Wales, communicated the result of several series of observations in which he had been engaged at the Observatory there. They contained, 1st. The determination of the solstice in December 1826, June 1827, and December 1827. In deducing the results, he notices the insufficiency of Delambre's formula for the *Reduction to the Meridian* in cases of great altitude: and suggests an alteration when the sun culminates near the zenith. 2nd. Observations on the inferior conjunction of Venus in December 1826. 3rd. Observations of moon-culminating stars during parts of the years 1826 and 1827. 4th. Places of some of the fixed stars in the southern hemisphere. 5th. A Catalogue of stars with which the great comet of 1825 was compared. 6th. Corrected observations of the place of the said comet. 7th. A determination of the latitude of the Observatory by reflection with the mural circle.

The next communication was from Mr. Curnin of Bombay, giving a more accurate account of his observations of moon-culminating stars at that Observatory, during the year 1825: from a mean of all which, compared with corresponding observations at Bushey Heath, and at Greenwich, he deduces the longitude of the Observatory equal to 4^{h} 51^{m} 9^{s} east from Greenwich.

Mr. Baily presented a short account of the two invariable pendulums, the one of iron and the other of copper, which he had caused to be made, agreeably to the order of the Council, and which are intrusted to the care of Captain Foster in his present voyage of experiment and observation, for the purpose of investigating the possible effects of the earth's magnetism in various geographical positions. The form and construction of these pendulums are somewhat different from those in general use; consisting merely of a plain, straight, uniform bar of metal: and, as he conceived that there is a considerable advantage in having each pendulum a *convertible* pendulum, he has placed *two* knife-edges on each bar. This property of convertibility, however, instead of being effected by a sliding or moveable weight, is produced by filing away one of the ends of the pendulum, until the number of vibrations on the two knife-edges are equal. The mode of making a pendulum of this kind he then describes in the following manner: Take a plain straight bar of metal, two inches wide, half an inch thick (or $\frac{3}{8}$ths of an inch if thought preferable) and *about* $62\frac{1}{2}$ inches long; at least it should not be shorter than this, prior to the first trial for the adjustment. At five inches from one end of the bar should be placed the apex of one of the knife-edges, which he calls A, and at the distance of 39·3 inches therefrom, should be placed the apex of the other knife-edge, which he calls B; each knife edge being firmly and properly secured in the usual manner. The distance of 39·3 inches is chosen, because the intervals of the coincidences are in such case about 15 minutes;

nutes; but if an interval of about 10 minutes should be preferred, the distance should be about 39·4 inches: and so in proportion.

In order to adjust a pendulum of this kind, it must be placed on the agate planes, and the number of vibrations determined in the usual manner; beginning, for the sake of regularity, with the knife-edge A; then inverting the pendulum, and determining the number of vibrations with the knife-edge B. In these preliminary experiments it is not necessary to extend the observations beyond one coincidence; neither is it requisite to apply any other correction than for the arc of vibration, and for the temperature of the room, since all the other sources of error will be common to the two positions of the pendulum, and therefore may be rejected in these first trials. If it should be found (as will in fact be the case) that the knife-edge B makes a *less* number of vibrations in a day than the knife-edge A, we must file away the bar at the end B, until the two knife-edges are perfectly synchronous. The amount to be taken away can be ascertained by experiment only; and as we approximate towards the truth we must be more cautious in using the file, and more accurate in making the observations. In this last step of the process it is difficult in all cases to determine the exact quantity that has been filed away; and we may sometimes overdo it, so as to cause an inequality in the vibrations of an *opposite* kind, and thus render it necessary to *add* a small quantity to the end B. In order to meet this difficulty, Mr. Baily caused a small hole to be drilled at the end B, into which a screw was fitted; and by means of a small piece of sheet lead inserted underneath, the adjustment could be carried to any required degree of accuracy.

The principal advantages attending the placing of two knife-edges on one and the same bar, Mr. Baily states to be as follows: viz. 1st. That we are thereby possessed of two separate and independent pendulums; the results of which may be used separately or conjointly at pleasure: and each of which is a more complete check on the other than when formed of separate pieces of metal, that may probably be of different specific gravities, and of different expansive qualities. 2nd. That the knife-edges, being once rendered synchronous, will always remain so, into whatever part of the world the pendulum may be carried; and thus enable us, if it should be required, to determine the length of the simple pendulum at any point of the globe at which it may be swung, by merely measuring the distances between the knife edges. 3rd. That we are thus furnished with the means of ascertaining whether the pendulum has sustained any accidental injury; since such a fact would be *immediately* discoverable from the inequality in the number of vibrations between the two knife-edges. And, even in case of such an unforeseen misfortune occurring, the *ratio* between the two would from that moment remain the same in all parts of the world, and answer the same useful purpose of *comparison* during the remainder of the voyage. Whereas, in the pendulum as usually constructed, the effect of such an injury (if not suspected) might be attributed to the errors of observation: and indeed the fact itself could not be ascertained until the return of the observer to some place where the pendulum had been previously swung;

leaving, however, the precise *time* at which the accident happened still undetermined, and not only the observations themselves, during some indefinite period, the subject of doubt and suspicion, but probably the whole series of no utility or avail.

Mr. Baily considers that there are also some advantages attending the *form* of this pendulum. For, being uniform in its dimensions, without any protuberant bob or projecting tail-piece, it is not so liable to accident as the ordinary pendulum, where those parts present opportunities for injury. It is also capable of being packed in a more convenient case, and thus rendered more easy of transportation, when required as a travelling instrument. But he states that if we view it in the light of an instrument intended for the observatory, as a means of determining the length of the simple pendulum, where both knife-edges are necessary for the solution of the problem, the advantages will be more apparent. For, in the construction of the pendulum here proposed, none of the parts slide over one another: there are no shifting weights, no moveable screws: every thing is fixed and stationary, and consequently more peculiarly adapted for the determination of so nice and difficult a problem.

As these pendulums are formed without any tail-piece, it became necessary to adopt some other mode of determining the arc of vibration. This, Mr. Baily has effected by making the edge of the pendulum, which is perfectly straight, answer the purpose of the point in the tail-piece of the ordinary pendulum: and by this method he was also enabled to enlarge the divisions of the scale (which is a diagonal one), so that the hundredth part of a degree occupies the length of three-tenths of an inch, and consequently can be read off with the greatest ease.

[*The remainder of the Proceedings will be continued in our next Number.*]

ROYAL ACADEMY OF SCIENCES OF PARIS.

December 10.—The ordonnance of the king approving of the nomination of M. Savart was read.—M. Anatasi communicated a new plan for the towing of ships.—M. l'Abbé Lachèvre objected to a part of M. Damoiseau's report respecting his chronological tables.—M. Chevreul, in the name of a Commission, made a very favourable report respecting the memoir of MM. Dumas and Boullay, jun. On the formation of sulphuric æther.—MM. Duméril and Dupuytren gave an account of the interesting essays by Dr. Senn of Geneva, respecting the treatment of diseases of the larynx.—M. Geoffroy Saint-Hilaire read a memoir on a small kind of crocodile living in the Nile, its organization and habits, and the motives which occasioned its being anciently honoured with the appellation of the Sacred Crocodile.—M. Cauchy read a memoir on the development of functions and rational fractions.—The observations of Mons. Giroux of Buzareingue, on the reproduction of domestic birds, were read.

December 17.—M. Cassini, in the name of a Commission, made a favourable report respecting Mons. A. Brongniart's memoir On the spermatic granules of vegetables.—M. Chevreul gave an account of several notices of M. Sérullas relating to the bromides of arsenic, antimony,

mony, and bismuth.—M. Girard gave a verbal analysis of several works published in America on the occasion of the opening of the Hudson canal.—M. Cauchy read a memoir, intitled *Usage du calcul des résidus pour la transformation ou la sommation des séries.*—M. Bonnard read a memoir on the locality of the manganese of Romanèche.—M. Féburier read a memoir, intitled *Notice sur la Lune rousse, et sur quelques effets de sa lumière et de celle des autres astres sur les végétaux.*

December 24.—MM. Raspail and Saigey sent a Notice respecting the sizing of paper.—M. Buran transmitted several observations concerning M. Payen's memoir on a new borate of soda.—M. Cagniard-Latour sent a memoir relating to the elasticity and change of size which metallic wires undergo when they are stretched.—M. Chevallier forwarded a sealed packet relating to the extraction of indigo.—M. Tilloy, of Dijon, sent his work on currants.—M. Duméril read a letter from Bretonneau, On the blistering properties of some insects of the cantharides-family.—M. Moreau de Jonnès communicated a Notice respecting the recent employment of mercurial treatment, both internal and external, in Cephalonia, for the prevention of the first symptoms of the plague.—M. de Blainville, in the name of a Commission, made a report on the memoir of M. Jacobson, intitled *Observations sur le prétendu développement des œufs, des moulettes, et des anodontes dans leurs branchies.* The Section of Mineralogy and Geology presented the following list of candidates for the two vacant places of Correspondents. *Geology:* MM. Conybeare, of London; Buckland, of Oxford; MacCulloch, of London; Freisleben, of Freyberg; Charpentier, of Bex.—*Mineralogy:* MM. Mitscherlich, of Berlin; Gustavus Rose, of Berlin; Haidinger, of Edinburgh.

Jan. 7, 1828.—According to the rules, the Academy proceeded to the election of a Vice-President. M. Mirbel had a majority of votes.—Mr. Warden communicated a letter from Mr. Smith, who, towards the end of 1826, explored a country hitherto unknown, to the S.W. and E. of the Rocky Mountains.—M. Thomas Grillon announced his discovery of a new mechanical means for moving vessels.—M. Blainville read a notice respecting the difference of the males and females of a species of gelasin.—M. Gannal read a memoir On the inspiration of chlorine in consumption.—Mr. Ivory was elected a Corresponding Member in the Section of Geometry.—M. Becquerel read a memoir On the electrical properties of the tourmaline.—M. Duvau read a statistical essay On the department of Indre and Loire.

Jan. 14.—M. Biot read a memoir On double refraction. On this occasion a sealed packet deposited by him on the 7th of January 1822, was given up to him; it contained a paper intitled, *Détermination expérimentale des expressions des deux vitesses dans les phénomènes de la double réfraction.*—A secret committee from the Section of Chemistry presented the following list of candidates for the vacant place of Corresponding Member: MM. Arfwedson, of Stockholm; Henry Rose, of Berlin; Thomson, of Glasgow; Houtou Labillardière, of Rouen; Liebig, of Giessen; Brande, of London.

Jan. 22.—M. Arago communicated a letter from Mr. Dalton on the aurora borealis of the 29th of March 1826; and a memoir by Mr. Scoresby On the singular effects produced by lightning on the vessel called the New York. He also gave a verbal account of an important memoir by M. Savary On magnetizing by the electric spark.—M. Legendre added fresh details to those which had been previously given on the interesting analytical researches of M. Jacobi of Kœnigsberg.—M. Cauchy presented a memoir On the remainders of functions expressed by definite integrals.—M. Dupin read a notice respecting early instruction at Touraine.—Mr. Warden communicated a letter relating to some islands recently discovered by Captain Coffin, not far from the coast of Brazil.—M. Arfwedson received the greatest number of votes as a corresponding member of the Section of Chemistry.—The members elected by ballot to constitute Commissions for the adjudging of prizes this year, are; For the mathematical prize relating to the resistance of fluids: MM. Lacroix, Legendre, Poisson, Fourier, and Prony.—For the astronomical prize: MM. Arago, Mathieu, Lalande, Bouvard, and Damoiseau.—For the prize relating to statistics: MM. Coquebert, Fourier, Dupin, Andreossy, and Lacroix.

Jan. 28.—There were read, a letter addressed to M. Delessert, containing information respecting M. Bonpland; A letter from MM. Quoy and Gaimard, containing geological observations; A letter from M. Valz, of Nîmes, On the elements of the two last comets.—After a report by M. Navier, a memoir presented by M. Landormy, On the theory of flying, was not approved of; this was also the case with M. Joseph Anastasi's project for towing.—M. Quenot read a memoir On a wire suspension-bridge, constructed at Jarnac, over the Charente.—M. Geoffroy Saint-Hilâire read a memoir On two species of animals, named *Trochilos* and *Bdella* by Herodotus.—The members for the Commission to decide the medical prize are: MM. Magendie, Boyer, Dumeril, Portal, Blainville, Fréd. Cuvier, Chaptal, Dulong, and Gay-Lussac.—Those for the physical prize are: MM. Magendie, Mirbel, Desfontaines, Dumeril, and Cuvier. The Commission for the mechanical prize is composed of MM. Girard, Navier, Prony, Molard, and Dupin.

XXVI. *Intelligence and Miscellaneous Articles.*

NEW ASTRONOMICAL EPHEMERIS.

WE have to congratulate the public on the appearance of one of the most useful publications in practical astronomy that has ever yet been formed. It is an Ephemeris arranged in an entirely new manner, computed on an entirely new principle, and adapted to the present advanced state of that important science.

Our readers are aware that, for the last fifty years, the celebrated Bode conducted the Berlin Ephemeris with great credit to himself, and with great advantage to astronomy. This work, inferior to none on the subject, contained annually a vast variety of valuable information,

tion, which would probably have perished, had it not been for the interest and zeal that Bode took in every thing relating to astronomy. Notwithstanding the rapid strides which the science has made on the Continent, little or no alteration however was made in the usual columns of this annual publication during Bode's life-time; but on his death M. Encke, who has been appointed to succeed him, determined on re-modelling the work altogether, and on adapting it to the increased and increasing demands of the astronomer. With this view he has abandoned the plan of publishing the voluminous Appendix thereto, which has generally been filled with matter that more properly belongs to a periodical journal, and which will now be transferred to the pages of Professor Schumacher's very valuable *Astronomische Nachrichten*; whilst the monthly columns of the Ephemeris will be consequently enlarged without any additional expense to the reader. On the other hand, Professor Schumacher will in future discontinue his annual *Hülfstafeln*; which will henceforth form part of M. Encke's work above alluded to. This exchange will be highly advantageous to the practical astronomer, who will thus have, in one volume, all the daily information he requires for the use of his observatory. The present volume is for the year 1830.

One principal and great *improvement* in this Ephemeris is the introduction of *mean solar* time into all the computations, instead of *apparent* time, as hitherto adopted in other ephemerides. The latter is never referred to, except in the case of the sun *at the time of its culmination.* In every other instance, the places of the moon and planets (and even the *sun* itself) is computed to mean solar time reckoned from the apparent equinox.

The *arrangement* of the Ephemeris also is very much improved. The places of the sun and moon are, as usual, disposed in monthly columns: but the places of the planets, and the other subjects which compose the body of the Ephemeris, are arranged in their respective orders, each by itself; as will be better understood from the synopsis of the work which we are about to present to our readers.

The *computations* likewise are carried to a greater degree of minuteness than has hitherto been done in any other similar work; and are thus not only better adapted to the more refined wants of the modern astronomer, but also more convenient for interpolation.

On the whole it is a work which we are persuaded will find a place in every observatory. We have often expressed our opinion of the want of such an Ephemeris, having occasionally suggested improvements for our own national production "the *Nautical Almanac*:" and we know that for many years past repeated representations on the same subject have been made to the Board of Longitude, not only by private individuals, but also by the Royal Society; but all to little or no purpose. For, though a gleam of light had lately begun to flitter amongst that learned body (like the expiring flame of a lamp), and they consequently thought it right (unconscious however of their approaching dissolution) to listen at last to the increasing demands of the astronomer; yet they were so tardy in their production, and so sparing in their explanations, that the information they intended to give was more speedily and better supplied from a *foreign* source.

The present, however, forms a new æra in the science; and something may now perhaps be done to place astronomy (as it ought to be) on a better footing in this country. And since œconomy is the order of the day, and has in fact been publicly declared to be one of the causes of the dissolution of the Board of Longitude, we would propose to follow up that system, by getting rid also of the whole of the expense incurred in forming the *Nautical Almanac,* and placing it in totally different hands. For, the computers of the Berlin Ephemeris would (no doubt) for a small additional sum, be very readily induced to adapt their calculations to the meridian of Greenwich: and any respectable booksellers, or other body of men in this country, if the copyright of such work were secured to them, would not only very readily defray that additional sum, and the expenses of printing, for the privilege thus conferred on them, but also employ an English computer to revise the calculations. The astronomer would thus be furnished with a work more fitted for his purpose, and the public be relieved of a considerable expense, which, after all, has hitherto produced only a secondary sort of Ephemeris*.

We come now, however, to a more minute account of the work in question, which is as follows. The ephemeris of the sun is for each month divided into two pages; one of which is devoted to *apparent* noon, and the other to *mean* noon. The former page contains, besides the days of the month and the days of the week, the mean time (to *two* places of decimals in the seconds), the right ascension of the sun (to *two* places of decimals), and its declination (to *one* place of decimals), together with the equation of time (to *two* places of decimals), and the logarithm of the double daily variation in the declination,—a quantity extremely useful in determining the time from altitudes of the sun. The latter page contains the right ascension of the meridian (to *two* places of decimals), the longitude of the sun (to *one* place of decimals), its latitude (to *two* places of decimals), the logarithm of the radius vector (to *seven* places of decimals), and the semi-diameter of the sun (to *two* places of decimals); together with not only the days of the month, but likewise the number of days elapsed from the commencement of the year.

The ephemeris of the moon is also divided into two parts; but as the computations are made for every twelve hours, each month occupies four pages. These contain the moon's longitude, latitude, right-ascension, declination, parallax, and semi-diameter, (each to *one* place of decimals,) for *mean* noon, and mean midnight. There is also given the mean time of the moon's upper and *lower* culmination, (to the *tenth* of a minute in time), as well as her right ascension and declination (to the tenth of a minute in space); together with the time of her rising and setting, the time of her changes, and the time when she is in perigee or apogee.

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* If the Nautical Almanac were made what it *ought* to be, and such as the situation of this country demands, we have no doubt but that its sale might be considerably increased. We know that the American booksellers (who *reprint* that work in the United States) correspond with the German astronomers for the supply of additional matter, to be inserted in the annual volumes. And what is the consequence? *One* bookseller alone (and there are *several* who reprint the work) sells upwards of twelve thousand copies! We believe the *total* sale of the Nautical Almanac, in *this* country, never amounted to seven thousand copies.

At the end of this joint ephemeris of the sun and moon, there is given for every *tenth* day of the year, the apparent obliquity of the ecliptic, the parallax of the sun, the aberration, and the equation of the equinoctial points (each to *two* places of decimals); together with the place of the moon's node (to the nearest *tenth* of a minute).

Then follows an ephemeris of each of the planets, including the four newly discovered ones. The places of Mercury and Venus are computed for mean time at *noon* for every *second* day, and the remaining planets for mean time at *midnight* for every *fourth* day of the year. The columns contain the heliocentric longitude and latitude of the seven principal planets (to one place of decimals in the seconds), the geocentric right ascension (to two places of decimals), and the geocentric declination (to one place of decimals); the radius vector, and the logarithm of the distance from the earth (each to seven places of decimals); together with the time of their rising, setting, and passing the meridian. The computations of the four newly discovered planets are not so minute, except at the time of their opposition; for which period a separate ephemeris is given of the position of the planet for *every day*.

We have next an ephemeris of the time of the eclipses of Jupiter's satellites (to one place of decimals); to which is subjoined (for *each* satellite) a table for computing with the greatest accuracy, not only the configurations at any moment, but also the position of the satellite with respect to Jupiter at the time of its immersion or emersion. At the end of these tables, we are presented with another ephemeris (computed for every fortieth day) of the apparent position and magnitude of Saturn's ring.

After this comes a table of the mean places (for 1830) of 45 principal stars; the right ascensions to *three* places of decimals, and the declinations to *two* places of decimals. From these are computed and given for every tenth day of the year, the apparent places of the same stars (to *two* places of decimals), with their differences. And we have also the apparent places, for *every* day in the year, of *α* and *δ Ursæ Minoris*. To the whole of which are annexed formulæ for determining the amount of the diurnal aberration. Following these is given a table of the constants A, B, C, D, for every tenth day of the year, for the purpose of determining the apparent places of any other stars. It should however be remarked, that these letters do not indicate precisely the same quantities as are so designated in the catalogue of the Astronomical Society: and it should also be noted that the numbers are adapted to *sidereal* time. There is however another table subjoined, for the use of those who are disposed to adopt *mean solar time* in these computations.

Next follows a particular account of all the solar and lunar eclipses that will happen in the course of the year; together with all the necessary elements for computing them. This is followed by three pages of the principal phænomena of the planets: such as the time of their perigee or apogee, their perihelion or aphelion, their greatest elongation, their greatest latitude, their conjunction and opposition, their passing the nodes, their greatest brilliancy, their proximity to the moon and occultation thereby, &c.

Then follows a list of moon-culminating stars, occupying seventeen pages; and (which is equally valuable,) a list of the occultations of all the stars down to the 7th magnitude inclusive, that will take place in the course of the year; wherein the mean time of the immersion and emersion of the star (to the nearest tenth of a minute) is given, as well as the angle from the vertex of the moon at which the phænomenon will take place. To this list is subjoined some auxiliary tables for computing the occultation more minutely, if required.

To the whole is annexed an Appendix, giving an account of the mode in which all the computations are made, and the tables from which they are derived. By this excellent plan, the observer can at any time verify any of the calculations, and detect any error which he may have cause to suspect. The names of the computers also are given, which must materially tend to insure the accuracy of the work.

Such is the substance of the publication now before us, which has just reached this country, and which does so much credit to its distinguished conductor. We hail it as the harbinger of a general improvement in the mode of arranging and forming the ephemerides of different nations. And although it is mortifying to reflect that this country cannot (or will not) maintain its pre-eminence in these and other scientific subjects, yet we are grateful for information wherever it can be found, and trust that we shall be able eventually to emulate the splendid example which has thus been set us. M. Encke, disdaining the trammels of former and less enlightened times, and relying on his own excellent judgement and abilities, has nobly and boldly struck out a new path for himself, which we have no doubt will soon be followed by every nation pretending to encourage the science of astronomy.

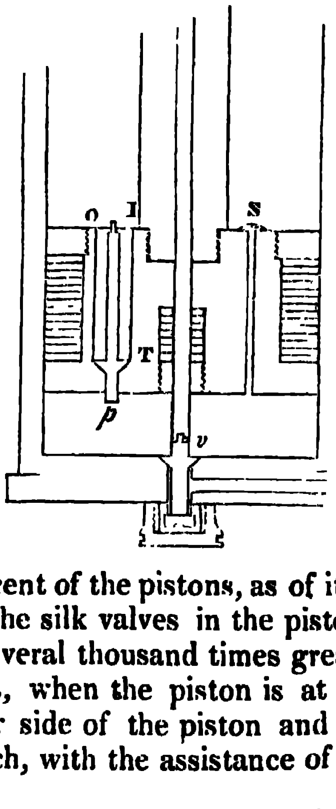
We propose to give, in a subsequent Number of our Journal, a translation of the Appendix above alluded to, which will enable the English reader to make use of this most excellent Ephemeris; since a very minute account is there given of the mode in which the different tabular values are formed. There will then be nothing left for explanation but the *headings* of the different columns; which are in most cases so much like the English names, that little difficulty will occur in understanding them.

IMPROVED AIR-PUMP. BY MR. JOHN DUNN, OPTICIAN, EDINBURGH.

In the course of my business, having often heard it regretted that the cost of apparatus prevented many gentlemen from engaging in philosophical pursuits, I have made it my study to simplify the construction of those which I have been employed to make, wherever this could be done without impairing the accuracy of their performance.

One of my first efforts was directed to that most valuable instrument the air-pump, which I shall endeavour to show I have improved so very materially, as to be able to furnish one capable of effecting as complete an exhaustion as the most perfect form of the instrument hitherto devised, and, at the same time, nearly as simple and as cheap as its most imperfect form. I mentioned my views on the subject to several gentlemen qualified to judge of their correctness, and soon had an opportunity of putting them to the test of experiment. I received an order to make one for Mr. Lees, lecturer on mechanical philosophy in the School of Arts here, on condition that he was to be permitted to return it, if, on trial, it was not found capable of executing all that I had led him to expect. This pump, through the kindness of Mr. Lees, in whose possession it has been for the last eighteen months, was exhibited to the Society for the Improvement of the Useful Arts, on the 19th of December 1827 *.

Believing the only useful part of Cuthbertson's improvement of the air-pump to be the contrivance for opening the valves at the bottoms of the barrels, mechanically, I was of opinion a pump would perform nearly, or altogether as well, divested of all the other peculiarities of his instrument, and possessing the decided advantages of being cheaper and much more easily kept in order.



The Figure is a section of one of the barrels of my pump, in which I employ metallic valves *v v'* at the bottom of the barrels, and waxed silk ones S S' in the pistons, laying aside Cuthbertson's metallic valves in the pistons, removing all his apparatus from the top of the barrels, and *leaving the pistons exposed to the atmosphere*, as I consider all those contrivances to be unnecessary, although it has been uniformly held essential to a good air-pump, since the time of Smeaton's invention, that the pressure of the atmosphere should be taken off the piston-valves; and my reason for doing so is, that the air will be always so compressed in the barrels, by the descent of the pistons, as of itself to have sufficient elastic force to open the silk valves in the pistons, the capacity of the barrels being each several thousand times greater than the space between the two valves, when the piston is at the bottom. In fact, by making the under side of the piston and the bottom of the barrel fit each other, which, with the assistance of the oil employed in the barrels may be done perfectly, there will be no space left but the small hole in the piston leading to its valve.

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* The instrument had been previously submitted to the examination of Dr. Turner, one of the Secretaries of the Society of Arts, who reported that he had minutely examined it, and was perfectly satisfied with its performance. On his representation to the Council of the London University, I have since received an order to make one for the chemical class of that Institution.

For illustration, let us suppose the stroke to be 12 inches, and the diameter of the barrels $2\frac{1}{2}$ inches, or 25 tenths (as is the case in Mr. Lees' pump), the diameter of the hole *e* one-tenth of an inch, and its length 1 inch, then, circles being to each other as the squares of their diameters, we have $1 \times 1 = 1$ for the capacity of the hole, and $25 \times 25 \times 12 = 7500$ for the capacity of the barrels; and consequently air, which in the receiver was 7000 times rarer than the atmosphere, would have sufficient elastic force to open the valve in the piston; but as this is a degree of rarefaction far beyond what has ever been attained, or even expected, it follows that any greater nicety of construction here is unnecessary.

The above plan may, however, be objected to, on account of its still leaving *something* to depend on the elastic force of the air which, should any one consider desirable to be removed, can be done by adopting metallic valves I I'[*] with projecting points *p' p'*, to strike against the bottom of the barrels, having the spaces O' T', O T, filled with oil, to exclude the external air during their shutting, instead of the oiled silk ones S S'; but even this small addition I consider wholly unnecessary.—*Edin. New Phil. Journ.*

GALLATES OF QUINA AND CINCHONIA.

M. Platania forms these compounds in the following way: Pour an infusion of galls into a hot solution of sulphate of quina, wash the precipitate with cold water upon a filter, and dry it at 100° of Fahr. Afterwards dissolve it in warm alcohol; pour off the solution and evaporate it, then add cold water to it, which precipitates pure gallate of quina.

Another process consists in pouring gallic acid into sulphate of quina, and merely washing the precipitate with cold water; and it may also be formed by directly combining the acid and base each separately dissolved in alcohol. Gallate of quina is very white and light; its sp. gr. is 0·816 at 60° Fahr. Its vapour is astringent, and very slightly bitter; it is soluble in alcohol and æther, but almost insoluble in water. It is composed of nearly

Gallic acid..........	14·87
Quina..............	85·13
	100·00

The gallate of cinchonia is obtained by dropping gallic acid into a solution of cinchonia; the gallate precipitates, and is to be redissolved in water and suffered to cool; the liquid becomes opalescent, and deposits granular transparent crystals.—*Hensman's Repertoire de Chimie, &c.* Jan. 1828.

PREPARATION AND PROPERTIES OF ALUMINUM.

On these subjects the following statements are made by M. Woehler. The method of preparing aluminum is founded upon the inoxidability of this metal by water. When an attempt is made to heat chloride of aluminum with potassium in a tube, the action is so strong and the extrication of heat is so considerable, that the apparatus is instantly broken. I therefore employed a small platina crucible, the cover of which was kept on by a wire of the same metal. At the moment of reduction, the crucible became intensely red-hot, both within and without, although it was but slightly heated; the metal of the crucible was not sensibly acted upon. The operation may also be effected in a porcelain crucible with a cover attached. Some small pieces of potassium of about the size of a pea, and not more than ten at once, are placed in the crucible, and upon them are put an equal number of pieces of chloride of aluminum of the same size; the crucible is to be heated with the spirit-lamp, at first gently, and afterwards more strongly, and until the spontaneous incandescence of the matter ceases. Excess of potassium is to be avoided; for after it was oxidized, it would dissolve a portion of the aluminum. The reduced mass is generally completely fused, and is of a blackish-gray colour. When all is cold, the crucible is to be thrown into a large vessel of water; a gray powder is soon deposited, which, when looked at in the sunshine, appears to be entirely composed of small metallic plates; the powder is to be washed with cold water and then dried: it is the metal of alumina.

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[*] The accented letters refer to a suppressed figure of the other barrel of the pump.

Aluminum somewhat resembles platina in powder. I discovered some scaly coherent particles, which had the colour and splendour of tin. Under the burnisher it readily assumes the appearance of this metal; rubbed in an agate mortar, it seems to be a little compressible, and unites into larger scales, with a metallic lustre; and it leaves in the mortar traces of a metallic appearance. When heated in the air, until it is ignited, it inflames and burns with great rapidity; the product is the white oxide of aluminum in a hard mass. Reduced to powder and blown upon in the flame of a candle, each particle suddenly becomes an inflamed point, the splendour of which is not less than that of the sparks of iron burning in oxygen gas. In pure oxygen gas aluminum burns with so dazzling a light, that the eyes can scarcely bear it; the heat generated is so considerable, that the oxide produced is partly fused. The particles which have been fused are yellowish, and as hard as corundum; they do not merely scratch, but they cut glass. In order that aluminum may burn in oxygen gas it must be heated to redness.

Aluminum is not oxidized by water, and this fluid may spontaneously evaporate from the metal without its being in the least tarnished; when however the water is nearly at its boiling point, the metal is slowly oxidized, and hydrogen is liberated.

The sulphuric and nitric acids when cold do not act upon aluminum; when heated, concentrated sulphuric acid readily dissolves it, and without the evolution of sulphurous acid; the sulphuric solution did not by evaporation give the smallest crystal of alum.

Aluminum introduced into a solution of caustic potash, even when weak, dissolves readily, and with the evolution of hydrogen; the solution is perfectly clear; the same solution takes place in ammonia; and it is surprising to observe how much of this earth the ammonia is capable of uniting with: the evolution of hydrogen is similar to that with potash. When aluminum is heated to dull redness, and exposed to a current of chlorine, it inflames and is converted into chloride, which sublimes as fast as it is formed.—*Ibid.*

CHLORIDE AND OTHER COMPOUNDS OF ALUMINUM.

M. Woehler obtains chloride of aluminum, for the purpose of procuring the metal from it, by the following process: alumina precipitated by excess of carbonate of potash, was well washed and dried, and then made into a thick paste with powdered charcoal, sugar and oil; this paste was then heated in a covered crucible until all the organic matter was destroyed. By these means any substance is mixed very intimately with carbon: the product while it was hot, was put into and made to fill a porcelain tube, which was placed in a furnace of an oblong form. One end of the tube was connected with another tube containing fused chloride of calcium, and this with an apparatus for the evolution of chlorine; the other end of the tube opened into a small tubulated receiver, provided with a conducting tube. When the apparatus was full of chlorine, the tube and its contents were made red-hot. The chloride of aluminum was readily formed; a small portion was carried over with oxide of carbon, which fumed strongly on coming into contact with the air. The chlorine was long retained by the mass of matter. The receiver contained chloride of aluminum in the state of powder. After an hour and a half the chloride obstructed the end of the tube (though an inch in diameter) which passed into the receiver; this caused the stoppage of the process.

On taking the apparatus to pieces, it was found that all that part of the tube which passed through the furnace was filled with chloride of aluminum, and it weighed more than an ounce. It consisted partly of an aggregation of long crystals, and partly of a firm mass, which was readily detached from the tube, and was of a pale yellowish-green colour, semitransparent, and of a lamellated and distinctly crystalline texture; but no crystal could be found sufficiently regular to admit of its form being ascertained. When brought into contact with the air, it fumed feebly, gave a smell of muriatic acid, and soon became a transparent fluid. When thrown into water, it dissolved with strong hissing, accompanied with so much heat, that the fluid, when the quantity is small, boils rapidly: according to M. Oersted, the temperature is not much higher than that of boiling water. Its fusing and vaporizing points seem to be the same. Chloride of aluminum may be preserved without any alteration in naptha; when heated with this oil it liquefies, and sinks to the bottom of the vessel, in the form of a reddish-brown liquid, upon which potassium exerts no action. Chloride of boron may be obtained by passing chlorine over calcined borax heated to redness.

Sulphuret of Aluminum.—When sulphur is suffered to drop upon aluminum in a state of vivid ignition, the mixture becomes strongly incandescent, and a black frit is formed: it is semimetallic in appearance, and when polished is of a shining iron-black colour. When exposed to the air, this frit emits a smell of sulphuretted hydrogen, swells, and falls into a grayish-white powder; when applied to the tongue, it occasions a hot penetrating sensation; when thrown into water, it is converted into a gray powder of alumina, accompanied with a rapid disengagement of sulphuretted hydrogen. Sulphate of alumina when heated to redness in contact with hydrogen, loses its acid, but the earth is not reduced.

Sulphuretted Hydrogen and Aluminum.—When chloride of aluminum is sublimed in a small retort, and a strong current of sulphuretted hydrogen gas is at the same time made to enter its neck, it is absorbed; and a very white sublimate is formed, partly in the state of pearly transparent scaly crystals, and partly in that of a brittle mass. The residuum of sulphuretted hydrogen was displaced from the apparatus by simple hydrogen. In the cold, this gas is not absorbed by the chloride of aluminum. In contact with the air, the sublimed matter moistens rapidly, sulphuretted hydrogen is disengaged, and chloride of aluminum remains in solution. When sublimed in a tube, it evaporates with the evolution of sulphuretted hydrogen equal to from 30 to 40 times its volume, which however cannot be the whole of the gas, because the combination is formed at a high temperature. When put into water, the sublimate is decomposed with the same violence as the pure chloride; much sulphuretted hydrogen is disengaged, and the solution is rendered turbid by the precipitation of sulphur. Pass a small piece of the compound into a tube full of mercury, and afterwards some water; a great volume of gas is evolved with great rapidity, which is completely absorbed by solution of acetate of lead, with the precipitation of sulphuret; when thrown into solution of ammonia, alumina is precipitated. No action takes place between the compound of sulphuretted hydrogen and aluminum with muriatic acid gas.

Phosphuret of Aluminum.—Aluminum heated to redness in the vapour of phosphorus, combines with it with vivid inflammation; the product is a blackish-gray pulverulent substance, which under the burnisher assumes a deep gray metallic lustre, and exhales a smell of phosphuretted hydrogen; when thrown into water, it occasions the evolution of phosphuretted hydrogen, which is not spontaneously inflammable. In the cold, the disengagement of this gas is not so rapid as that of sulphuretted hydrogen, but it is quickened by heat.

Seleniuret of Aluminum.—Selenium when mixed with the metal of alumina, and heated to redness, combines with it, producing strong inflammation. The seleniuret thus obtained is a black powder, which being rubbed becomes of a dull metallic aspect. When exposed to the air it continually exhales a smell of seleniuretted hydrogen; in water the disengagement of this gas is very rapid, and the water is quickly reddened by a portion of precipitated selenium.

Arseniuret of Aluminum.—Arsenic reduced to powder and heated to redness with aluminum, combines with it; the inflammation is less vivid than with the preceding combustibles. The compound is a powder of a deep gray colour, which by rubbing acquires a dull metallic appearance, and when exposed to the air it exhales a faint smell of arseniuretted hydrogen; when cold, the disengagement is slow, but it is much accelerated by heat.

Telluret of Aluminum.—When the powder of tellurium was put into a tube with aluminum, much heat was excited, and the mixture was thrown with explosion out of the tube; this inconvenience is avoided by not powdering the tellurium. The product is a metalline, brittle, black frit, which when exposed to the air emits an intolerable odour of telluretted hydrogen; and when thrown into water it evolves the same gas with rapidity: the water at first becomes of a red colour, afterwards brown, and eventually opake, on account of the interposed reduced tellurium: the telluret of aluminum appears to decompose in water with much greater facility than the sulphuret of the same metal.—*Ibid.*

NATIVE IODIDE OF MERCURY.

M. Del Rio has already mentioned that he has discovered iodide of silver in America, and he has mentioned its locality. He has since discovered another iodide; and he is of opinion that the metal in combination with it is mercury. It perfectly resembles dark-coloured cinnabar, except that its colour is deeper and its streak paler; it is however certain, that it accompanies an earthy iodide, which M. Del Rio believes to be the metal of magnesia mineralized by iodine.—*Ibid.*

CORYDALIN,—A NEW VEGETABLE ALKALI.

According to M. Wackenroder, this alkali is contained in the root of the fumitory (not the common fumitory, *fumaria officinalis*, but the *fumaria cava*, and *corydalis tuberosa* of Decandolle). The dry root is to be coarsely powdered and digested for some days in water; filter the infusion, and precipitate with excess of potash; dry the precipitate and treat it with boiling alcohol, until it ceases to dissolve anything. It sometimes happens that during the cooling of the alcohol, crystals of corydalin are deposited. The solution is to be evaporated to dryness, and the residuum is to be dissolved in weak sulphuric acid; this solution is then to be decomposed by an alkali either caustic or carbonated. A white deposit is formed, which by drying becomes of a light gray colour.

Dry corydalin soils the fingers very much; it is insipid and inodorous. It is soluble in alcohol; and this solution when hot and saturated deposits colourless prismatic crystals of a line in length. By slow spontaneous evaporation, fine laminæ are formed. The solution acts as an alkali upon vegetable blue colours. At a temperature below that of boiling water, it melts into a deep green-coloured fluid, which, when solidified, has a crystalline texture, and is transparent in thin laminæ. At a higher temperature it yields water and ammonia, and is converted into a transparent brown mass. Æther dissolves corydalin with the same facility as alcohol; caustic potash dissolves it in considerable quantity.

This alkali forms extremely bitter salts with acids; sulphuric acid forms two different salts; one which crystallizes is obtained when the acid is digested with excess of base; the solution is to be filtered and evaporated: the product is very slightly soluble in water. When a small quantity of sulphuric acid is added to a solution of corydalin in alcohol, so as not to saturate the base perfectly, a portion of crystalline matter is deposited; and there remains a stratum of a greenish transparent substance, which is unalterable by exposure to the air, and readily soluble in water: the solution reddens litmus paper slightly; an excess of acid renders it purple, and eventually blackens it. Nitric acid when diluted and cold dissolves and forms a colourless solution with corydalin; but when heated it becomes of a red colour, which, when the solution is concentrated, becomes of a blood-red colour. This action is so strong, that by the aid of heat the smallest quantity of corydalin may be discovered in a fluid. Muriatic acid forms with this alkali an uncrystallizable salt; acetic acid is still more difficult of combination with it than sulphuric acid; but it forms a crystalline salt, which may be redissolved a second time in water and crystallized. Tannin is one of the most sensible tests of corydalin, as for all other vegetable bases. The precipitate is white when the solution is dilute, and grayish-yellow if concentrated.—*Ibid.*

ACTION OF ALKALIES AND THEIR CARBONATES, &C. ON IODIDES.

M. Berthemot, having made numerous experiments on the action of alkalies, and some metals on the iodides, concludes:—That the earthy oxides and their carbonates do not act upon iodide of mercury; —that potash, soda, barytes and strontia, decompose iodide of mercury by the intervention of water or alcohol, and there result oxide of mercury and tri-iodo-hydrargyrate of potash, which on the cooling of the liquors, successively deposit iodide of mercury, and bi-iodo-hydrargyrate of potash;—that lime produces the same phænomena, with this difference however, that the action occurs only by the intervention of alcohol;—that the soluble carbonates of the alkaline oxides also decompose iodide of mercury, and yield analogous products, but only with the intervention of alcohol;—that the insoluble carbonates of the alkaline oxides do not act upon iodide of mercury, either by the intervention of water or alcohol;—that the protoxide of mercury decomposes the iodide of potassium, forming potash, and metallic mercury, or protiodide of mercury and iodo-hydrargyrate of potash;—that the remaining alkaline iodides have a similar action, except that of calcium, which does not appear susceptible of it;—that peroxide of mercury decomposes the alkaline iodides, forming an alkaline oxide and bi-iodo-hydrargyrate.—*Journal de Pharmacie, April* 1828.

CITRIC ACID FROM GOOSEBERRIES.

M. Tilloy, by the annexed process, has prepared citric acid from gooseberries, so as to be able to obtain it for 12 francs, 96 centimes the kilogramme; whereas the price of citric acid in France is from 29 to 30 francs for the same weight.

The gooseberries are to be bruised and fermented; the alcohol is to be separated by distillation; the residuum is to be pressed to extract the whole of the liquid. To this liquor, while hot, carbonate of lime is to be added as long as effervescence takes place: after standing, the citrate of lime is to be collected and suffered to drain; it is to be repeatedly washed and then pressed. The citrate of lime thus obtained, being still coloured and mixed with malate of lime, is to be mixed with water to the consistence of a thin syrup, and is then, while hot, decomposed with sulphuric acid, diluted with twice its weight of water. The liquor resulting from this operation, is a mixture of sulphuric (malic ?) and citric acid, and is to be again treated with carbonate of lime. The precipitate, when collected on a filter, is to be plentifully washed, pressed, and again mixed with sulphuric acid; the clear liquor, containing the acid, is to be decolorized by animal charcoal, and evaporated. When it is sufficiently concentrated, it is suffered to deposit, and the clear liquor poured off is put into stoves heated from 20° to 25° Centig. Coloured crystals are thus obtained, which are to be drained, slightly washed, and recrystallized.—*Ibid.*

SOLANINE.

M. Pelletier could not obtain solanine from the *solana* of Europe, but he procured it from the *solanum mammosum* of the Antilles.—*Ibid. May* 1828.

BLUE COLOUR BY THE ACTION OF MURIATIC ACID UPON ALBUMEN.

Various unsuccessful experiments appear to have been made to produce this blue colour; first observed, we believe, by M. Caventou. According to M. Robiquet, the more acid employed, the more readily is the blue colour produced, to a certain extent. He finds that seven or eight parts of acid, to one part of albumen, yield the most intense blue, even at a low temperature; but its development is favoured by a temperature of 25° to 30° Centig.—*Ibid.*

BOTRYOGENE, OR NATIVE RED IRON-VITRIOL OF FAHLUN.

Berzelius gave an analysis of this salt some time since; of its physical properties very little was however then known. Mr. Haidinger, having been furnished with specimens by Berzelius, and M. Pohlheimer of Fahlun, has given an account of its crystalline form and qualities in Brewster's Journal for July last.

It occurs in the great copper-mine at Fahlun in Sweden. The regular forms of botryogene belong to the hemiprismatic system of Mohs; they are in general pretty distinct, but too imperfectly formed to permit any thing more than an approximation to the real angles within ten minutes of a degree. The lustre of botryogene is vitreous. It is translucent. Its colour is a deep hyacinth-red; which, however, in compound massive varieties, passes into ochre-yellow, which is likewise the colour of the streak. It is sectile, and becomes a little shining in the streak, and its hardness is a little inferior to that of alum. Its specific gravity is 2·039. It is slowly soluble in water, and does not, therefore, possess so powerful an astringent taste as common sulphate of iron. The crystals are not above two lines in length, and are usually aggregated in reniform and botryoidal shapes, consisting of globules with a crystalline surface; the trivial name alludes to the tendency of the salt to produce such imitative shapes.

When exposed to a moist atmosphere, it becomes covered with a dirty yellowish powder, but remains unchanged in a dry atmosphere: before the blowpipe it intumesces, and gives off water in a glass tube, leaving a reddish yellow earth behind, which according to the flame employed may be changed into protoxide or peroxide of iron. With salt of phosphorus it yields a red glass, which loses its colour on cooling. Boiling water dissolves part of it, leaving a yellow ochre, which therefore is an integral portion of the mixture. The solution, nitric acid being added to it, may be precipitated by muriate of barytes, but not by nitrate of silver. Ammonia, with which the salt is digested in a stoppered bottle, takes away all the acid, and leaves the iron in the shape of a slightly greenish black powder. The iron therefore is contained in the salt, not as a pure oxide, but as a compound of the protoxide and peroxide, which is black when pure, and produces red solutions.

The following are the results of three analyses:

	I.	II.	III.
Persulphate of iron, with excess of base,	6·77	6·85	48·3
Bisulphate of the protoxide and peroxide of iron	35·85	39·92	
Sulphate of magnesia	26·88	17·10	20·8
Sulphate of lime	2·22	6·71	0·0
Water and loss	28·28	31·42	30·9

The second analysis is most correct as to the water. Berzelius considers all the substances mixed with salt of iron as foreign to the salt, and uncombined with it.

ERINITE,—A NEW MINERAL SPECIES.

This substance is arseniate of copper, contained in Mr. Allan's cabinet. Mr. Haidinger makes the following observations on it. "Though not presenting determinable crystals, the appearance of the specimens in Mr. Allan's cabinet, the only ones which I remember to have ever met with, are highly crystalline. The individuals are arranged in concentric coats with rough surfaces, produced by the termination of exceedingly small crystals, the layers often not firmly cohering, so that they may be easily separated from each other. These layers themselves are very compact; they show an uneven, or sometimes imperfect conchoidal fracture, and traces of cleavage. The cleavage probably takes place parallel to the broad faces of rectangular four-sided plates, into which the individual terminates. I have, in several instances, observed something like them by means of a compound microscope, but always very indistinctly. These plates form crest-like aggregations. A circumstance which greatly increases the difficulty of observing the regular forms, is the complete absence of lustre, the surface of the concentric layers being quite dull, while there is only a slight degree of resinous lustre on the fracture.

The colour of erinite is a beautiful emerald-green, slightly inclining to grass-green; the streak, likewise green, is a little paler, and approaches to apple-green. It is slightly translucent on the edges. The substance of erinite is brittle; its hardness I found = 4·5 . . . 5·0 of the scale of Mohs; its specific gravity = 4·043.

According to the locality attached to the specimens in Mr. Allan's cabinet, they are natives of the county of Limerick in Ireland. For the name of Erinite, which is here proposed for this mineral, the mineralogical public is indebted to Mr. Allan. It unites, what is rarely the case with mineralogical names, the comparatively trite and prosaical allusion to the native country, with the poetical recollection of the characteristic verdure of the "*Emerald Isle.*"

Erinite is associated with two of the species containing arsenic acid and copper, described by Count Bournon; the common arseniate of copper (prismatic olive-malachite of Mohs), and the dark blue arseniate, both of them crystallized and disposed between the concentric layers of erinite. Dr. Turner gives the following as an approximation of the analysis of erinite:—

Oxide of copper	59·44
Alumina	1·77
Arsenic acid	33·78
Water	5·01
	100·00

Brewster's Journal, July 1828.

ALTERATION OF CRYSTALLINE STATE IN SOLIDS.

M. Mitscherlich finds that when sulphate of magnesia or sulphate of zinc is slowly heated in alcohol, and the heat be gradually increased to boiling, the crystals lose their transparency by degrees; and when broken they are found to consist of a great number of new crystals entirely different from those of the salt employed, owing to the change in the position of the atoms, by internal motion, without the occurrence of solution. *Ann. de Chim.* xxxvi. p. 206.

DECOMPOSITION OF AMMONIA BY METALS.

M. Savart found that 141·91 grains of thin copper wire became 142·382 grains, or acquired an increase of 0·472 in weight, when used for four hours to decompose ammonia: as the wire was in a slight degree oxidized, the experiment was repeated; and when every precaution was employed, the increase amounted to $\frac{1}{275}$, and 0·105 of an unknown substance was absorbed by the copper, and its specific gravity was diminished from 8·8659 to 7·7919.

Iron also increases in weight, and diminishes in specific gravity by similar treatment, and will strike fire with flint like ordinary steel.

Ibid. xxxvii. p. 326.

IODIDES OF CARBON.

Whilst experimenting for a peculiar purpose, M. Mitscherlich mingled the alcoholic solutions of iodine and soda. "There was formed immediately," he observes, "the compound obtained by Serullas. But Serullas, to whom we are indebted for a great many interesting experiments on this compound, says, that there is formed simultaneously, iodate of soda, iodide of sodium, and hydriodide of carbon; but I have not found the slightest trace of iodate of soda. On decomposing the substance obtained by Serullas by means of copper, iron, and mercury, I obtained no hydrogen, nor any other kind of gas, but only a combination of iodine and carbon. We should therefore consider this substance as a compound of carbon and iodine formed in the following manner:—When the two alcoholic solutions are mixed, the iodine combines with the sodium; and the oxygen set free, unites to the hydrogen of the alcohol to form water; whilst the carbon of the alcohol (the latter being considered as a compound of water and olefiant gas) combines with another portion of the iodine to produce the iodide of carbon.

"This iodide of carbon, distilled with corrosive sublimate, yields a liquid analogous to that which Serullas obtained by employing dry chloride of phosphorus. It is also a compound of carbon and iodine; so that we now know two combinations of iodine and carbon, and one with carburetted hydrogen, discovered by Faraday, which is distinguished from the two others by its chemical properties and crystalline form."

The experiments of M. Mitscherlich, by showing the true nature of M. Serullas' compound, remove the difficulty of supposing that two hydriodides of carbon could exist of exactly the same composition, but different in properties.

Ann. de Chim. xxxvii. p. 85. *Roy. Inst. Journal, July* 1828.

SOLAR SPOTS.

The large solar spot, whose appearance we described under our last monthly meteorological report, came round on the sun's eastern limb in the night of the 12th instant, as we supposed it would, and was well-defined by the 14th, when the nucleus had assumed the shape of a pear: on the 17th it was bell-shaped, and on the 19th, when nearest to the sun's centre, the umbra and nucleus were nearly circular, with a few indentations on the edge of the latter, and but little apparent diminution in the size of either since the 27th of May. At 7 A.M. on the 23rd it was, as nearly as could be ascertained from a drawing, in the same position on the sun's disc as on the 27th ultimo; and on the 25th at sunset it was very near his lower limb in a very contracted state, resembling a line without any perceptible umbra, and went off on his posterior side again in the night, making a complete revolution in both cases in 27 days, and thus travelling, when the necessary correction is made for the earth's annual motion in the ecliptic during the period of its revolution, at the rate of 1454 miles per hour, which is to the velocity of a point on the earth's equator as 7 to 5 nearly. Early in the morning of the 19th, this spot was within 9 degrees of the sun's equator, or its declination was 9 degrees North. Its largest diameter, from a mean of several admeasurements, was 1½ diameter of the earth, or about 12000 miles. Although it had undergone considerable changes in respect to figure, yet it appeared strong enough to last another revolution, by which means more favourable opportunities may offer to obtain the number of hours (if any) in addition to the first observed 27 days of its revolution. It appears from a drawing of the positions of this spot, and another large one that accompanied it, whose declination was about 19 degrees South, that in their daily progress across the sun's disc they moved in slightly parallel curved lines from East to West, and went off nearly at the same time; therefore they will probably appear on the sun's eastern limb at noon of the 9th of July. Since the 27th of May, considerable variations and alterations have taken place in the positions and number of the solar spots; some have entirely disappeared, while new ones have appeared on other parts of the sun's disc.

LIST OF NEW PATENTS.

To S. Pratt, of New Bond-street, camp equipage maker, for improvements on elastic beds, cushions, seats, pads, and other articles of that kind.—Dated the 25th of June 1828.—6 months allowed to enrol specification.

To J. Baring, of Broad-street Buildings, merchant, for a new and improved mode of making machines for cutting fur from skins for the use of hatters. Communicated from abroad.—3rd of July.—6 months.

To J. Johnston Isaac, of Star-street, Edgeware Road, Middlesex, for improvements in propelling vessels, boats, &c.—5th of July.—6 months.

To T. Revis, of Kennington-street, Walworth, Surrey, for an improved method of lifting weights.—10th of July.—6 months.

To J. Hawks, of Weymouth-street, Portland Place, iron manufacturer, for an improvement in the construction of ships' cable and hawser chains.—10th of July.—6 months.

To J. H. A. Gunther, of Camden Town, Middlesex, for improvements on piano-fortes.—10th of July.—2 months.

To Captain W. Muller, of Doughty-street, Bedford-row, for an instrument or apparatus for teaching mathematical geography, astronomy, and other sciences; and for resolving problems in navigation, spherics, and other sciences.—10th of July.—6 months.

To B. Rider, of Redcross-street, Southwark, for his improvements in the manufacture of hats.—17th of July.—6 months.

To J. Jones, of Amlwch, Anglesea, for his improvement in certain parts of the process of smelting copper ore.—17th of July.—6 months.

⁂ Mr. Herapath informs us that he has perused a copy of the paper written in his defence signed *Veritas*, and requests us to state to our readers, in a form more permanent than a notice on our wrapper, that he cannot consider it as intemperate, nor acquiesce in our reasons for not inserting it; and that he conceives the majority of our readers would coincide with him in this opinion. He adds that he is ready to reply to every point advanced in the papers signed $\alpha\ \beta$, and F.R.S., the names of the writers are commnuicated to him to use in public.

We must therefore repeat in our justification that we object to continue a controversy, when it is degenerating from a philosophical inquiry into a personal dispute. We should not have inserted Mr. H.'s last letter, had we observed that, instead of giving any reply to the objections brought by α β, and F.R.S. against his charge of failure in Lagrange's method, he had passed them by in silence, and aimed at taking up new ground. We submit, however, that the fair course of discussion absolutely requires that he should dispose of the objections, either by admitting their validity, or by refuting them, before he can have a claim further to occupy our pages on this subject. With his suggestion respecting the names of his opponents we cannot comply: nor are names of any consequence in such discussions. As to the letter of *Veritas*, it does not appear to contain any thing by which knowledge may be advanced, or our readers interested.—Ed.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1828.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·33 June 26. Wind S.—Min. 29·35 June 18. Wind N.E.
Range of the index 0·98.
Mean barometrical pressure for the month .... 29·982
Spaces described by the rising and falling of the mercury............ 4·610
Greatest variation in 24 hours 0·550.—Number of changes 18.
Therm. Max. 81° June 28. Wind S.E.—Min. 48° June 6. Wind N.W.
Range 33°.—Mean temp. of exter. air 63°·63. For 30 days with ☉ in ♊ 62·10
Max. var. in 24 hours 24°·00—Mean temp. of spring water at 8 A.M. 52°·44

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 22nd.............. 86°
Greatest dryness of the air in the afternoon of the 8th............... 40
Range of the index... 46
Mean at 2 P.M. 51°·2—Mean at 8 A.M. 57°·7—Mean at 8 P.M. 63·7
—— of three observations each day at 8, 2, and 8 o'clock......... 57·5
Evaporation for the month 4·65 inches.
Rain near ground 1·98 inches.
Prevailing wind, S.W.

Summary of the Weather.

A clear sky, 5; fine, with various modifications of clouds, 17; an overcast sky without rain, 4; rain, 4.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus
25	16	29	0	29	23	14

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
2	3	2½	4	3	8	2½	5	30

General Observations.—The first part of this month was alternately wet and dry, but from the 20th to the end, more favourable weather could not have happened for hay-making, which was performed generally in Hampshire with expedition, and the crops got in, in excellent condition. Early in the morning of the 5th a storm passed over to the eastward, with lightning and thunder: showers of rain and hail with distant thunder also occurred in the afternoon of the 6th. In the evening of the 7th a parhelion appeared in a *cirrostratus* cloud on the south side of the sun. From between two and three till nearly six o'clock in the morning of the 15th, the weather was very awful here; the lightning, which was chiefly forked, flashed vividly at short intervals; its colour was dark-red; it continued a long time in the zenith, and the explosions were so near, that the rushing of the adjacent air into the displacement to restore its equilibrium, shook the foundations of the houses. There were two winds at the same time; viz. a strong gale next the earth from due North, surmounted by a slow moving current from S.E. as ascertained by the black thunder-clouds which the latter wind carried with it; by the inosculation of these winds, and clouds of unequal temperatures from nearly opposite points of the compass, the lightning was generated, and was awfully grand for upwards of two hours. No damage was done by the storm in this neighbourhood, but it was severely felt in the upper part of the country. The same morning between four and five o'clock, just before the heavy rain came on, a beautiful double rainbow appeared in a large *nimbus* to the westward; the arc of the exterior bow extended from about S.S.W. to W.N.W. The electric fluid which accompanied the recent thunder-storms in this county was very powerful, having killed several men and horses.

On the 20th, about 5 o'clock in the afternoon, an anthelion appeared in the eastern prime vertical, opposite to and 120 degrees distant from the sun. This rare phænomenon presented itself in a light brown *cumulostratus* cloud, from which it was distinguished by its circular silvery colour, which repeatedly contracted and expanded according to the effulgence of the sun. It continued in sight nearly two minutes, by which time the cloud had moved off too far to the north-east to exhibit the sun by reflection.

The 28th and 29th were hot sunny days, the thermometer in the shade in the afternoons being at 80° and 81°, and in the sun's rays at 106°.

The mean temperature of the external air this month is 2¼ degrees higher than the mean of June for the last twelve years.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are one anthelion, one parhelion, four solar halos, two double rainbows, lightning and thunder on four different days; and eight gales of wind, or days on which they have prevailed; namely, one from the North, one from the North-east, one from the East, three from the South-west, and two from the West.

REMARKS.

London. — June 1—3. Very fine. 4. Rainy. 5. Cold and cloudy. 6. Sultry: with thunder. 7. Clear and fair. 8. Fine: showery at night. 9—11. Very fine. 12. Sultry, and warm. 13—15. Very fine. 16. Drizzly. 17. Sultry, and warm. 18. Showery in the morning: fine. 19. Cloudy, and warm. 20. Very fine. 21. Wet morning: fine. 22. Drizzly: fine. 23—30. Very fine and warm.

Boston.—June 1. Cloudy. 2—4. Fine: rain A.M. 5. Cloudy: rain A.M. hail-storm 1 P.M. rain at night. 6, 7. Cloudy. 8. Cloudy: rain A.M. 9. Fine. 10—13. Cloudy. 14. Fine. 15. Cloudy. 16. Fine. 17. Cloudy: rain A.M. 18. Fine: rain A.M. and P.M. 19, 20. Fine. 21. Rain: thunder and lightning with rain P.M. 22. Fine: rain P.M. 23. Cloudy: rain P.M. 24—27. Fine. 28—30. Cloudy.

Penzance.—June 1. Fair: rain. 2. Clear. 3, 4. Rain: showers. 5. Clear: showers. 6. Fair: showers. 7. Fair. 8. Fair: clear. 9, 10. Clear. 11. Clear: fair. 12. Fair: at times clear. 13. Fair: clear. 14. Clear: cloudy: lightning. 15. Misty. 16. Misty: showers. 17. Rain. 18. Rain: fair. 19. Fair: showers. 20. Fair. 21. Clear: showers at night. 22. Clear. 23. Fair: clear. 24, 25. Clear. 26. Fair: clear. 27—29. Clear. 30. Fair.

Meteorological Observations made at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY *at Penzance, Dr.* BURNEY *at Gosport, and Mr.* VEALL *at Boston.*

Days of Month, 1828.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost. 8½ A.M.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.										
June 1	30·073	30·047	30·15	30·12	30·06	30·02	29·45	69	52	63	53	69	55	62·5	SW.	W.	SW.	W.	...	...	...	0·055	...
2	29·966	29·927	30·08	30·00	29·94	29·92	29·33	67	49	61	50	66	52	59	SW.	NW.	W.	W.	...	...	0·145	·030	0·06
3	29·991	29·919	30·05	30·00	29·99	29·96	29·44	65	54	61	51	69	55	58	SW.	W.	SW.	W.	0·45	...	0·100	·230	...
☾ 4	29·537	29·477	29·75	29·54	29·63	29·48	29·12	65	50	58	51	62	49	58	W.	NW.	SW.	W.	...	·76	0·475	·255	·14
5	29·543	29·323	29·73	29·55	29·55	29·38	28·85	62	45	60	50	66	50	55	SW.	NW.	W.	W.	...	·03	0·175	·070	·08
6	29·802	29·626	30·00	29·83	29·78	29·64	29·15	65	49	57	51	66	48	54·5	W.	NW.	SW.	W.	·35	·01	0·050	·070	·22
7	30·137	30·001	30·20	30·14	30·11	30·00	29·53	61	44	58	49	64	52	55	W.	N.	NW.	W.	...	...	...	...	...
8	30·206	30·186	30·25	30·25	30·18	30·17	29·70	64	47	63	48	70	55	58	W.	N.	NW.	W.	...	·07	...	...	...
9	30·252	30·132	30·26	30·25	30·20	30·20	29·75	70	47	62	52	74	51	58·5	NW.	N.	N.	NW.	·35	...	...	...	·30
10	30·266	30·249	30·30	30·28	30·26	30·24	29·72	70	55	61	51	75	56	61·5	W.	N.	NW.	NW.	...	...	...	...	...
11	30·247	30·219	30·30	30·28	30·23	30·20	29·66	73	56	62	51	75	55	61	W.	N.	NW.	NW.	...	...	...	...	...
● 12	30·245	30·233	30·25	30·20	30·22	30·20	29·70	65	47	66	54	67	55	62	NW.	N.	N.	NW.	·60	...	...	...	...
13	30·251	30·235	30·15	30·12	30·20	30·16	29·75	77	48	67	55	76	54	61·5	E.	SE.	E.	calm	...	...	...	...	...
14	30·233	30·185	30·10	30·04	30·17	30·08	29·75	72	54	69	55	72	56	67	NE.	SE.	SE.	calm	...	...	...	·350	...
15	30·086	30·032	30·04	30·00	30·01	29·97	29·67	73	58	65	57	73	60	57·5	NE.	SW.	SE.	E.	·45	·06	...	·150	...
16	30·010	29·852	29·95	29·73	29·92	29·77	29·53	71	59	66	56	75	59	64·5	E.	NE.	NE.	E.	...	·01	...	...	...
17	29·541	29·517	29·50	29·25	29·52	29·49	29·12	71	55	64	56	71	58	63	NW.	N.	NE.	E.	...	...	0·215	·280	·16
18	29·721	29·503	29·75	29·45	29·72	29·35	28·97	72	55	62	56	70	58	66	W.	NW.	SW.	E.	·30	·05	0·555	·020	·07
19	29·974	29·947	29·86	29·85	29·94	29·90	29·32	72	54	66	56	76	58	65	SW.	SW.	S.	W.	...	·02	0·025	·005	·29
☽ 20	29·976	29·968	29·86	29·86	29·96	29·94	29·40	73	58	68	56	76	57	67	SW.	SW.	S.	NW.	...	·21	0·075	·465	...
21	29·904	29·867	29·88	29·86	29·86	29·84	29·32	70	55	67	56	70	56	61·5	SW.	W.	SW.	NW.	·25	·68	...	...	·33
22	29·972	29·920	30·00	29·95	29·93	29·90	29·34	72	48	65	57	73	53	64	SW.	NW.	SW.	W.	...	·04	0·140	...	·35
23	30·182	30·081	30·15	30·12	30·10	30·06	29·53	73	52	67	56	68	53	60	W.	NW.	N.	W.	...	...	...	...	·05
24	30·308	30·268	30·25	30·25	30·26	30·24	29·78	75	53	68	53	76	54	63	W.	W.	SE.	W.	·40	...	...	...	...
25	30·301	30·271	30·26	30·24	30·28	30·28	29·68	81	53	68	53	71	54	69	SE.	N.	SW.	calm	...	...	...	...	...
26	30·337	30·305	30·30	30·30	30·33	30·26	29·71	85	54	71	58	74	57	69	E.	SE.	S.	calm	...	...	...	...	...
○ 27	30·168	30·130	30·18	30·10	30·21	30·10	29·60	83	57	74	58	74	62	73	E.	SE.	SE.	calm	·80	...	...	...	...
28	30·045	29·975	30·07	30·00	30·02	29·92	29·46	79	56	75	59	81	62	69·5	E.	SE.	NE.	calm	...	...	...	...	...
29	30·013	29·924	29·95	29·90	29·91	29·90	29·40	81	49	74	62	80	60	65	NE.	SE.	E.	E.	...	...	...	...	...
30	29·959	29·929	29·90	29·90	29·97	29·93	29·37	84	58	74	62	76	59	66	SE.	S.	SE.	NE.	·70	...	...	...	...
																			4·65	1·94	1·955	1·980	2·05
Aver. :	30·041	29·974	30·30	29·25	30·33	29·35	29·47	72	52	75	48	81	48	62·4									

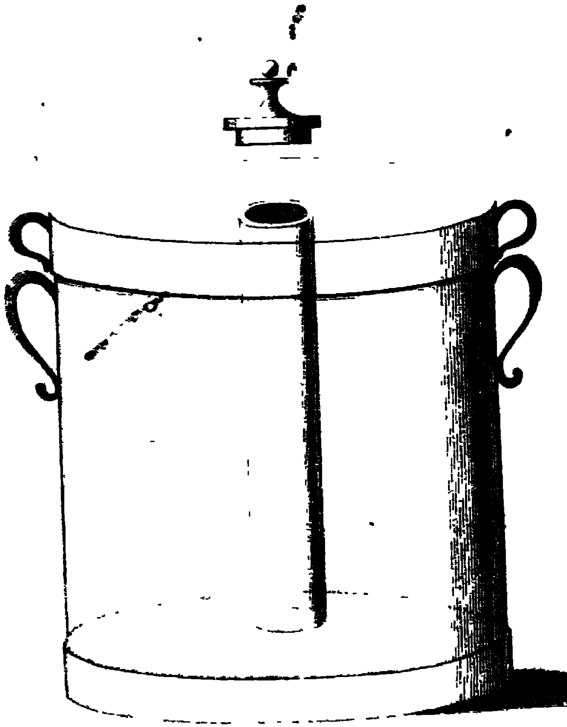

Fig. 1.

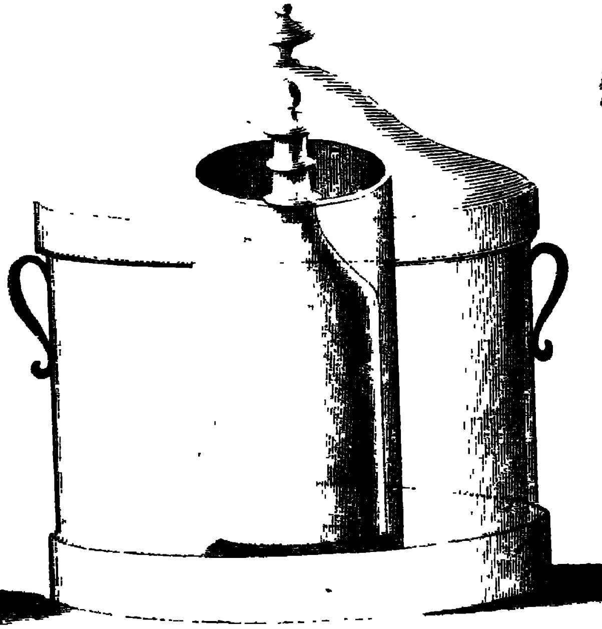

Fig. 2

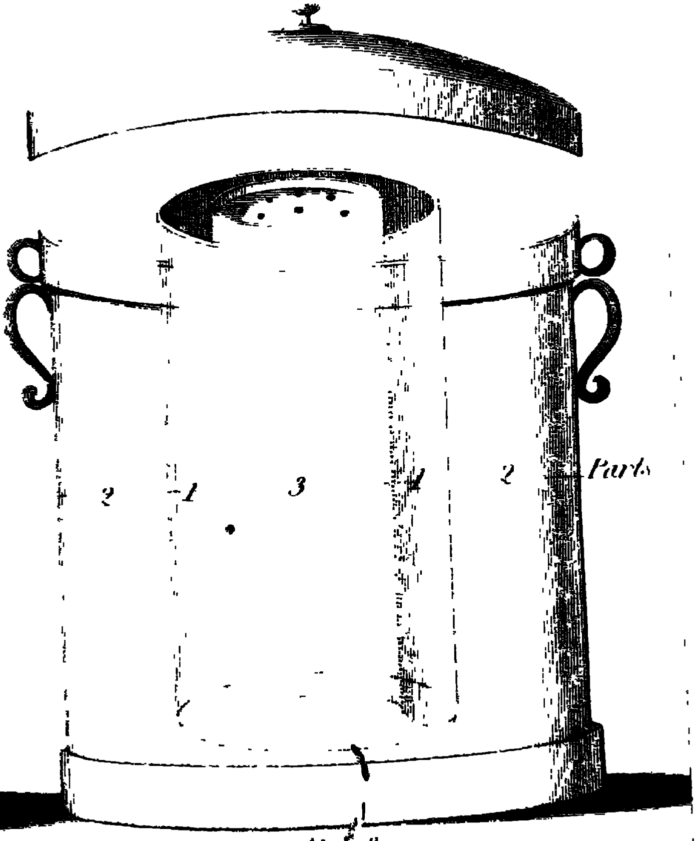

Fig. 3

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

OCTOBER 1828.

XLII. *On the Method employed in the Trigonometrical Survey for finding the Length of a Degree perpendicular to the Meridian.* *By* J. Ivory, *Esq. M.A. F.R.S.*[*]

IN the first volume of the Trigonometrical Survey, a method is given for finding the difference of longitude of two stations lying nearly in the same parallel of latitude, without assuming that the earth has any particular figure, and supposing only that it is nearly a sphere[†]. An important part of the Survey depends upon this method; and, although its accuracy has heretofore been called in question, yet we see from the late volumes of the Phil. Trans. that it is now deemed sufficiently exact and unexceptionable in its principle. It is however remarkable, that this method has never succeeded in practice, or rather, has in every case led to results evidently wide of the truth. This failure it is usual to ascribe to errors in the observed azimuths; but we may reasonably doubt whether this be the sole cause of deficiency, when we observe that the difference of longitude of the two stations, computed by other modes of solution unquestionable in point of accuracy, is always found to depend on the compression of the spheroid. The method of which we are speaking, was first published in the Phil. Trans. for 1790 and 1795; it is therefore time to subject to a strict examination a process which, although it has never been of any utility, is still employed in practice; and as I have thrown out some doubts about the grounds of it in this Journal for May last, I deem it incumbent on me to ascertain fully its real character.

Let λ and m denote the latitude and azimuth at the first station; λ' and m' the same things at the second station;

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[*] Communicated by the Author.

[†] Trigonometrical Survey, vol. i. pp. 154, 293.

γ the rectilineal distance of the two stations, or the chord between them; ω the difference of longitude; a the equatorial semidiameter, and e the excentricity of the elliptical meridians. Further, let ϕ stand for the angle of depression of the chord below the horizon of the first station: then $\gamma \sin \phi$ will be the perpendicular drawn from the second station upon the horizon of the first; and the distance of this perpendicular from the plane of the meridian of the first station, to which plane it is parallel, will be equal to $\gamma \cos \varphi \sin m$, because the azimuth m is the angle which the projection of the chord upon the horizon makes with the meridian. Now let x be the distance of the second station from the polar axis of the spheroid: then, ω being the angle between the two meridians, it is obvious that the distance of the second station from the plane of the meridian of the first, will be equal to $x \sin \omega$; and in consequence of what was before proved, we shall have this equation,

$$x \sin \omega = \gamma \cos \varphi \sin m.$$

Next let R be the radius of a sphere which passes through both the stations, and touches the horizon of the first: then,

$$\text{Sin } \phi = \frac{\gamma}{2\text{R}}, \quad \cos \varphi = \sqrt{1 - \frac{\gamma^2}{4\text{R}^2}}.$$

If we conceive a plane to bisect the chord γ at right angles, this plane will cut off from the normal to the earth's surface at the first station, a part equal to the line R. Therefore R, like the radii of curvature of the spheroid, will always be little different from a, the semidiameter of the equator; and since γ is always a small part of R, or of a, we may substitute a for R in the expression of $\cos \varphi$, without danger of introducing a sensible error in any case that can occur in practice. We have likewise, from the nature of the elliptical spheroid, $x = \frac{a \cos \lambda'}{\sqrt{1 - e^2 \sin^2 \lambda'}}$; and thus we get,

$$\frac{\cos \lambda' \sin \omega}{\sqrt{1 - e^2 \sin^2 \lambda'}} = \frac{\gamma}{a} \sqrt{1 - \frac{\gamma^2}{4a^2}} \times \sin m.$$

Now, put $\sin \frac{\beta}{2} = \frac{\gamma}{2a}$; then,

$$\frac{\gamma}{a} \sqrt{1 - \frac{\gamma^2}{4a^2}} = 2 \sin \frac{\beta}{2} \cos \frac{\beta}{2} = \sin \beta;$$

and the last formula will become,

$$\frac{\cos \lambda' \sin \omega}{\sqrt{1 - e^2 \sin^2 \lambda'}} = \sin \beta \sin m.$$

The like reasoning applied to the second station will furnish another similar equation.

Thus we finally obtain,

$$\begin{aligned} \text{Cos}\,\psi' &= \frac{\cos\lambda'}{\sqrt{1-e^2\sin^2\lambda'}}, \quad \cos\psi'\sin\omega = \sin\beta\sin m, \\ \text{Cos}\,\psi &= \frac{\cos\lambda}{\sqrt{1-e^2\sin^2\lambda}}, \quad \cos\psi\sin\omega = \sin\beta\sin m'. \end{aligned} \tag{x}$$

Suppose now that a spherical triangle is constructed of which the base is equal to the arc β, and the two sides to the arcs $90-\psi$ and $90-\psi'$: I say, that the angle of this triangle opposite to the base β is not sensibly different from ω, the difference of longitude, when the two latitudes are nearly equal; and is exactly equal to it when the two latitudes are equal. In order to prove this, it is to be observed that $a\cos\psi$, $a\cos\psi'$, are the respective distances of the two stations from the polar axis of the spheroid; and $a\sin\psi\sqrt{1-e^2}$, $a\sin\psi'\sqrt{1-e^2}$, are their distances from the plane of the equator. Wherefore, because ω is the angle between the two meridians, we have this expression for the square of the chord, viz.

$$\frac{\gamma^2}{a^2} = (\cos\psi - \cos\psi'\cos\omega)^2 + \cos^2\psi'\sin^2\omega + (1-e^2)(\sin\psi - \sin\psi')^2;$$

or, which is the same thing,

$$\frac{\gamma^2}{a^2} = 2-2(\cos\psi\cos\psi'\cos\omega+\sin\psi\sin\psi')-e^2(\sin\psi-\sin\psi')^2:$$

but $\frac{\gamma^2}{a^2} = 4\sin^2\frac{\beta}{2} = 2(1-\cos\beta)$; and, hence,

$$\cos\beta = \cos\psi\cos\psi'\cos\omega+\sin\psi\sin\psi'+\frac{e^2}{2}(\sin\psi-\sin\psi')^2.$$

Now, from this equation and the relation that is known to subsist between an angle of a spherical triangle and the three sides, it follows that ω may be reckoned equal to the angle of the triangle opposite to the base β, whenever the latitudes are so nearly equal that the term multiplied by e^2, has no sensible value; and when the latitudes are exactly equal, the equality affirmed is rigorously true. But for the greater precision let us inquire, what variation the term multiplied by e^2 will produce in the arc β. For this purpose suppose that β becomes $\beta+\delta\beta$, then,

$$\text{Cos}\,(\beta+\delta\beta) = \cos\psi\cos\psi'\cos\omega+\sin\psi\sin\psi',$$

$$\delta\beta = \frac{\epsilon(\sin\lambda-\sin\lambda')}{\sin 1''}\times\frac{a(\sin\lambda-\sin\lambda')}{\gamma},$$

the true latitudes being written for ψ and ψ', and $\frac{\gamma}{a}$ for $\sin\beta$, in the expression of the small variation. In the instance of Beachy Head and Dunnose, we shall find $\delta\beta = 0''\cdot11$, a quantity far below the errors of observation. In this instance, and in all those where this method has been actually used, or where it can be supposed to apply, we may conclude that the angle of the spherical triangle opposite to the base β, is equal to ω, the difference of longitude of the two stations. But in every spherical triangle, the sines of the angles are proportional to the sines of the opposite sides; and hence, on account of the equations (x), we learn that m is the angle of the same triangle opposite to the side $90-\psi'$, and m' the angle opposite to the side $90-\psi$.

Having obtained a knowledge of all the parts of the spherical triangle, if we apply to it one of the analogies of Napier, we shall get,

$$\text{Tan}\,\frac{\omega}{2} = \frac{\cos\frac{\psi-\psi'}{2}}{\sin\frac{\psi+\psi'}{2}} \times \text{cotan}\,\frac{m+m'}{2}.$$

Now this formula is different from the method in the Trigonometrical Survey in no other respect, except that the arcs ψ and ψ', which are what are called the reduced latitudes, come in place of the true latitudes λ and λ'. But as the reduced latitudes depend upon the excentricity of the spheroid, it follows that the difference of longitude is no more independent of the figure of the earth in this mode of computation, than in any other. Because the difference of latitude is very small, we may write $\cos\frac{\lambda-\lambda'}{2}$, or even the radius of the tables, instead of $\cos\frac{\psi-\psi'}{2}$: and I have found, by reducing properly and putting $\varepsilon = \frac{e^2}{2}$,

$$\text{Sin}\,\frac{\psi+\psi'}{2} = \sin\frac{\lambda+\lambda'}{2} \times \left(1 - \varepsilon\cos^2\frac{\lambda+\lambda'}{2}\right).$$

The foregoing expression will now become

$$\text{Tan}\,\frac{\omega}{2} = \frac{\cos\frac{\lambda-\lambda'}{2}}{\sin\frac{\lambda+\lambda'}{2}} \times \text{cotan}\,\frac{m+m'}{2} \times \left(1 + \varepsilon\cos^2\frac{\lambda+\lambda'}{2}\right);$$

or, in logarithms,

$$\text{Log tan.}\,\frac{\omega}{2} = \log\left(\frac{\cos\frac{\lambda-\lambda'}{2}}{\sin\frac{\lambda+\lambda'}{2}} \times \text{cotan}\,\frac{m+m'}{2}\right) + M\varepsilon\cos^2\frac{\lambda+\lambda'}{2}.$$

This, then, is the formula by which we must compute the difference of longitude on a spheroid of which ε is the compression; and if we make $\varepsilon = 0$, it will coincide with the method in the Trigonometrical Survey, and will give the difference of longitude on a sphere.

But even the correct expression of the difference of longitude, which we have here investigated, has a disadvantage that makes it improper to be applied in practice; namely, the result is always affected with the sum of the errors of the two azimuths, which is likewise heightened by the divisor of the formula. In the instance of Beachy Head and Dunnose, we have

$$\lambda = 50^\circ 44' \, 21'', \qquad m = 96^\circ 55' \, 58''$$
$$\lambda' = 50 \;\; 37 \;\; 5 \qquad m' = 81 \;\; 56 \;\; 53:$$

and with these data, the difference of longitude will be found equal to,

$$1^\circ \, 26' \, 54''{\cdot}76.$$

Now I have computed the three following values of the same difference of longitude from independent data; one, by the formula (A) at p. 190 of the last Number of this Journal, which is independent of the azimuths; and two, by employing the formula (B) at p. 191, combining the azimuth at one station with the latitude of the other, the result not being sensibly affected in this mode of computation by any probable error in the azimuth; viz.

$$1^\circ \;\; 27' \;\; 5''{\cdot}62$$
$$1 \;\; 27 \;\; 5{\cdot}63$$
$$1 \;\; 27 \;\; 5{\cdot}61$$

It appears therefore that the result obtained by the formula investigated in this article is in defect about 11'', which can only arise from an error of about 9'' in the sum of the azimuths.

J. Ivory.

XLIII. *Some Remarks on an Article in the* Bulletin des Sciences Mathematiques Physiques et Chimiques, *for March* 1828. *By* J. Ivory, *Esq. A.M. F.R.S. &c.**

In the *Bulletin des Sciences Mathematiques Physiques et Chimiques* for March last, there is an article relating to the papers inserted in this Journal, which treat of the attraction of spheroids and the figure of equilibrium of a homogeneous planet in a fluid state. The remarks of the author on the opinions I have ventured to advance on these subjects, seem to call for some notice from me, which I shall study to make as brief as possible.

With regard to the attraction of spheroids, the usual ground of the dispute is shifted. The controversy has hitherto been confined to the law of attraction that prevails in nature; namely, when the exponent of the attractive force is -2; but on the present occasion, the author brings forward the case when the exponent of the attractive force is negative and greater than 2. I have no inclination to enter the lists on this new ground; and I am persuaded that whoever will read attentively what is written in the *Bulletin*, will not blame Laplace for departing from the great generality aimed at in the third book of the *Mécanique Céleste*, and in his later writings confining his theory to the single case of an attraction inversely proportional to the square of the distance.

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* Communicated by the Author.

In p. 155, the author proceeds to animadvert on what he calls my new principle of Hydrostatics, which he says has not yet been refuted by any geometer "*d'une manière vraiment scientifique.*" Great was my astonishment on perusing what the author has written, to find that it does not affect in any respect what I have published concerning the equilibrium of a planet in a fluid state. This part of the *Bulletin* is mostly taken up with two demonstrations of the general equation of the level surfaces, or the surfaces of equal pressure, in a fluid mass *in equilibrio*. Now I have nothing to object to these demonstrations. Applied to the case of a homogeneous planet, the meaning of the equation is this: The equilibrium of the planet requires that it be possible to divide the whole fluid mass into any number of strata separated by surfaces of equal pressure, which, beginning at a point within the planet, extend to the outer surface, and are included one within another*. My solution of the problem is so far from being inconsistent with the general equation, or with the equivalent property of the level surfaces, that it is the only general method that has yet been found for rendering the existence of these surfaces demonstrative and certain †.

In the usual theory, and particularly in the theory delivered in the *Mécanique Céleste*, it is affirmed that the perpendicularity of gravity to the outer surface is all that is necessary to insure the existence of the interior level surfaces, their gradual decreasing, and final concurrence in a point. But of this no sufficient demonstration is given; and I contend that none can be deduced from the single principle of equilibrium assumed. I have done nothing more than add a condition which is wanting, without which the problem cannot be solved.

In reference to some of the author's remarks, it is to be ob-

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[*] See Clairaut, *Figure de la Terre*, Part. i. § xxi.

[†] In prop. 4th, p. 166 of this Journal for September 1827, it is shown in what manner the general equation of the level surfaces is fulfilled in my solution: and it is proved that, without the condition I have added, the same equation could not be fulfilled, and there would be no equilibrium.

served, that in the investigation published in this Journal for September 1827, there is no mention made of strata infinitely thin, nor of the addition or subtraction of such strata.

After all, the question is not about this or that principle of Hydrostatics, nor this or that theory. The real question is, Whether the investigation I have published, more especially in this Journal as above cited, is, or is not, rigorous and exact; and whether I have been able to deduce, from the supposition that there is an equilibrium, the conditions without which it cannot subsist, and which are sufficient to determine the figure of the fluid. The proper way of answering this question is to examine the solution itself. And as this is the fairest way of deciding the matter, so it is the shortest and the easiest: for the whole investigation is divided into distinct propositions, which hardly occupy three pages of this Journal; and it involves no nice nor intricate point of analysis, which is often introduced because the true principles of the problem have been viewed in an improper light. One thing at least is certain, that M. Poisson has not succeeded in detecting any flaw in my reasoning; and the animadversions of the present critic leave the matter just where it was.

I shall immediately set about reviewing all that I have written on this subject, in order to correct any inaccuracies that may have escaped me, and to clear up any obscurities that may have occurred in taking a new view of a difficult subject. I hope to be able to guard my theory against the objections and attacks to which it has hitherto been liable, in a short work which I will address to the Royal Society; thinking that, amidst the more interesting and fashionable objects that occupy their attention, that learned body will not entirely proscribe a capital part of the philosophy of Newton, which is still very imperfect, notwithstanding the researches of so many philosophers. I have also another reason for making this destination of my work: the Royal Society, as the public promoters of science, have imposed upon them the duty of securing to every one the discoveries he may make.

But the author of the critique in the *Bulletin* is not content with animadverting on what I have done; he is obliging enough to carry his attention to what he supposes I am doing. I assure him, however, that he has been misinformed about the nature of my present occupations, and that he entirely misconceives my views of the equilibrium of a planet of variable density. This problem I have solved long ago; and the result of my investigation has already appeared in this Journal for July 1826. My analysis is not indeed published; because it is too bulky for the pages of this Journal, in which I have been led, from very peculiar circumstances, to communicate what I write to the public. But in the work I have announced, I will treat this subject fully; and my critic will then see how far he is mistaken in supposing that I wished to introduce the consideration of particles attracting at very small distances.

J. IVORY.

XLIV. *On the Construction and Arrangement of the New Berlin Astronomical Ephemeris.* *By Professor* ENCKE*.

WE observe the bodies of our solar system not from the central point of their motions, but from a point on the surface of the earth which has a double motion about this central point. It would therefore be desirable to learn by the ephemerides the position of the heavenly bodies with respect to three points;—the centres of the sun, and the earth, and the place on the terrestrial surface for which the ephemerides are calculated. The peculiar modifications however, applicable to every body in our system, are such as to leave no more than two points for each of them, for which the more accurate data are requisite. For the planets which are more distant, and which are less regularly observed throughout the whole year, the reduction from the centre of the earth to a definite point of its surface is so simple, that nothing is required to facilitate the calculation; and for the sun and the moon, the relations to the centre of the whole system disappear, being implied with regard to the former, and unnecessary for the latter, whose course is referred only to the earth. In every opening of the book the two pages present, accordingly, for every body of our solar system the polar coordinates with respect to two points; for the moon and the sun, those relating to the centre of the earth and the place on the earth (Berlin) (with respect to the latter point the distances, however, have been omitted, as unnecessary); for the planets and the satellites of Jupiter, the heliocentric and geocentric places, or that which will supply their places. The coordinates which immediately involve the results of the observations are referred to the equator, the others to the ecliptic. The small planets make the only exception to this. The time which is everywhere applied, except where it is expressly mentioned, is mean time. The beginning of the day has been taken at noon, and the hours are counted to 24; so that hours below 12 are those of the afternoon, and the hours above 12 diminished by 12^h are the hours of the forenoon of the civil day following the one corresponding to the given hours of the astronomical day. All longitudes, latitudes, right ascensions and declinations, refer to the true or apparent equinox; and the true or apparent position of the different planets, for which Mr. Bessel's determinations of the nutation and obliquity of the ecliptic have been throughout applied. All data have been calculated from the tables without neglecting any one correction, and have been given exactly as resulting from the tables. A main object of these ephemerides has been to save to the astronomer the trouble of the tedious immediate calculations from the tables.

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* From the New Berlin Ephemeris: being a translation of a portion of the Appendix to that work, as promised in our Number for August, p. 145. —EDIT.

The almanac contains, besides the chronological part and the explanation of the signs, four principal sections; viz. 1. Ephemerides of the sun and moon. 2. Ephemerides of the planets and their satellites. 3. Positions of the stars. 4. Phænomena and objects of observations.

For the sun and the moon every month has six pages, which for the facility of reference have been separately marked with Roman figures, I—VI. The first page contains the data necessary in solar observations. Their epoch is, therefore, the apparent noon at Berlin. After the first two columns, the days of the month and of the week, follows the mean time at the moment of the apparent noon, usually called the equation of time; next, the right ascension of the sun, or the sidereal time at the apparent noon; then the declination accompanied by the column, log. μ (agreeably to Gauss's notation, in the manner of Professor Schumacher's auxiliary tables), which is the log. of the change of declination in 48 hours expressed in seconds of a degree from the preceding noon to the following one, or very nearly the change of declination for 48 hours appertaining to the noon opposite to which it stands in the ephemeris. Lastly, the sidereal time is given which the sun employs in passing over the wire of a transit. The opposite page II. contains the data for the sun which are employed in calculations of the planets. Their epoch is, therefore, the mean noon. The columns of days of the month and of the week are followed by the sidereal time at the moment of the mean noon, which is requisite for reducing an observation made by sidereal time to mean time. Next follow the longitude, latitude and radius vector of the sun. In the former, the aberration has not been applied, so that the given numbers must be immediately used, without any correction, in converting geocentric places into heliocentric ones, and *vice versâ*. Lastly, the semidiameter of the sun is given, which is used in observations of declination.

These data have been derived from the solar tables of Carlini, improved by Mr. Bessel's corrections, which he has had the

kindness

kindness to communicate to me. The columns of the time of transit and semidiameter of the sun are taken from Bessel's tables. The calculations have been most rigorously executed, and the latitude has been duly taken into consideration in the right ascensions and declinations. We may therefore hope, that, should even new tables of the sun be published before 1830, the calculated positions will not greatly deviate from the true ones.

The following four pages III—VI. contain the positions of the moon. The odd ones III. and V. contain the longitude and latitude, and the right ascension and declination, of the moon for every mean noon and midnight at Berlin. The present arrangement of these columns, in which the data for noon are not separated from those for midnight, as was hitherto usually done, appeared to be more convenient for taking the differences. On the opposite pages IV. and VI. are contained in the first place for the same epochs the horizontal equatorial parallax of the moon which supplies the place of the distance, and the diameter as seen from the centre of the earth. Next follow the three columns which refer to the moment of the moon's culmination at Berlin; viz. the mean time of the moon's superior or inferior culmination, with the right ascension and declination corresponding to that moment, likewise referred to the centre of the earth. The upper and lower culminations are distinguished by the letters O and U.

The two last columns give the times of the sun and moon's rising and setting, designated by the letters A and U. Below the odd pages III. and V. are placed the changes of the moon by mean time; below the even ones, the times of perigee and apogee.

The columns contained on the even pages are intended to facilitate the calculation of the apparent place affected by parallax; this will be treated of more in detail in another place. For this reason the apparently misplaced times of the sun's setting have been here inserted. Those times have besides for astronomers no other essential importance, except as far as the visibility of other heavenly bodies is dependent on them. The moments of the sun and moon's setting are, as always where there is no particular mention, given in mean time.

All the calculations for the moon have been deduced from Burckhardt's tables, from which the later ones of Damoiseau do not appear ever to deviate considerably. The accuracy of the tables of Burckhardt having been much confirmed of late years (in the opinion of a very competent judge), they have as yet been preferred to the others founded more on theory.

For the careful execution of this portion of the work, I am indebted to the persevering industry of Messrs. Herter, Wolfers, and Deinhardt, who have divided between them the calculation of all the longitudes and latitudes, as well as right ascensions and declinations, immediately from the tables. This being the first considerable astronomical calculation executed by any of these gentlemen, no pains have been spared to discover and to correct all errors by taking the first four differences. Wherever a correction of 0″·5 would bring out more regular differences, the whole calculation has been revised. In a few such cases it was necessary to retain the calculated numbers unchanged; and this quantity may therefore be considered as the maximum of error. Errors of 0″·3, and less, could not be avoided, on account of the great number of equations. The parallax and semidiameter have been put to the same test. The manner of calculating the other columns will be given in another place. This section is concluded by a table in which the apparent obliquity of the ecliptic, the true parallax of the sun, the aberration of the sun's longitude, the equation of the equinoctial points, and the longitude of the moon's node, have been placed together for every tenth day. The aberration is to be added algebraically with the sign given to it to the values contained on page II., in order to obtain the real longitude of the sun, as it will be observed. The sign of the equation of the equinoctial points denotes that the mean equinox is in this year behind the true equinox, or that all mean longitudes are greater than the true ones. The moon's node is given according to Burckhardt.

Next follow the ephemerides of the planets and satellites. For the older planets the page on the left contains the heliocentric places, together with the columns of rising and setting; while that on the right contains the geocentric places, with the passages over the meridian in mean time, the former referred to the ecliptic, the latter to the equator, the right ascension being given in time. For Mercury and Venus the places have been calculated for every other day, and the mean noon at Berlin; for the others they are calculated for every fourth day, and the mean midnight at Berlin.

The calculations for Mercury have been undertaken by Mr. Herter. One of the corrections of the tables of M. Lindenau, first noticed by Professor Schumacher, which refers to the radius vector, has been inadvertently omitted. This trifling neglect affects, however, the last figures only of the radius vector, and seems to lie within the limits of uncertainty of even Lindenau's tables. The fourth differences have likewise been taken in this case, but their magnitudes were in some parts such as to afford no certain criterion of the absolute correctness of the calculation. The places of Mercury might, should it be wished, in future be given for every day of the year.

The calculations for Venus, and the greater part of those for Jupiter and Saturn, have been performed by Mr. Wolfers. For Venus and Mars, the tables of M. Lindenau; for Jupiter, Saturn, and Uranus, the latest tables of Bouvard,—have been used.

The time of passage over the meridian is meant only approximately; the astronomical use of it being supplied by the right ascension in time, which is likewise given. Calling θ the sidereal time at the moment for which the right ascension α has been calculated, the time given in the column, headed "Planet on the Meridian," is

For the superior planets..... $\alpha - \theta$
For the inferior planets...... $12^h + \alpha - \theta$

A corrective factor $= \frac{\text{mean solar day}}{\text{planetary day}}$ ought to have been applied to $\alpha - \theta$; but this factor would not have produced any great change, and if required, its effect may easily be calculated. From the time of passage over the meridian and the declination on that day, the times of rising and setting have been calculated, which are therefore not to be considered as rigorously correct. The times of rising and setting of the heavenly bodies have in general been calculated with due regard to refraction, for which purpose Bessel's horizontal refraction of $36'$ has been adopted. For the moon the mean parallax of $57'$ has besides been taken into account. The other heavenly bodies are, therefore, in those moments $36'$ below, and the moon $21'$, above the plane passing through the centre of the earth, and parallel to the horizon.

The four new planets make an exception; for there the geocentric place only is given, together with the auxiliary columns. The form in which their perturbations are calculated, is such that the accurate determination of their places would require more extensive calculations than the possible use renders necessary. In the same manner the determination of their heliocentric places would have caused a change in the form of calculation, which is in no proportion to the possible use which might be made of them. For this reason the elements for the time of the opposition have been rigorously deduced, and then have been retained for the whole year. For each planet, however, more accurate daily positions have been given for the 28 days, within which the moment of opposition is contained.

The perturbations of Pallas, Juno, and Vesta, have been calculated as far as the year 1830. For the first, the elements of

Gauss have been used; for the second, those of Nicolai; for the third, mine; which will appear in another place. For these three planets the errors will hardly amount to a minute. The planet Ceres, however, whose elements have not been further investigated since M. Gauss last corrected his Elements in the year 1809 (Elements, xiii.), and whose perturbations have not been completely developed, may deviate more considerably. In accordance with the last oppositions, the epoch of mean longitude has for the present been diminished by 14′ for the year 1830. It is to be hoped that this correction will likewise have nearly approximated it to its true position.

All data for the planets have been given, without any regard to aberration and parallax. On account of the former, the given places do not belong, with respect to the actual observations, to the moments 0^h and 12^h, but (calling the geocentric distance of any planet Δ) to those moments (viz. 0^h or 12^h)$+493''\cdot2.\ \Delta$ (time).

With regard to the satellites of Jupiter, which then follow, it was customary to give, besides the eclipses which supply the place of the heliocentric place, a graphical representation of their geocentric position with respect to Jupiter for a certain definite moment. But as their position is thus only obtained for a single moment of time, it has appeared to me that the advantage of an ocular graphical representation is overbalanced by the concomitant want of being deprived of the means of deducing the geocentric position for any other given time. I have therefore preferred giving the time of the superior geocentric conjunction, together with the corresponding ratio of the axes of the apparent ellipsis of the orbit of the satellite, accompanied by the tables of reduction; by means of which, from the time elapsed since the last preceding conjunction, the geocentric coordinates of the satellite, with respect to the centre of Jupiter, may be derived.

It is to be observed that these results have been founded, both as regards the derivation of the superior geocentric conjunction from the heliocentric conjunction and the time of revolution, on which the tables of reduction are founded, on the hypotheses of the mean heliocentric synodic time of revolution of the satellite, and of the perfectly circular form of their orbits, while the true synodic geocentric revolution ought to have been taken. They may, however, should more accurate measurements render it necessary, be corrected without any great trouble. The difference will always be small, and quite insignificant as to inspection.

By this arrangement the observer of eclipses is, at the same time, enabled accurately to determine the place of emersion, on which account the coordinates for those moments have not been added.

The occultations of the first two satellites are on the page on the left; those visible in Berlin are distinguished by an asterisk (*). On the right hand are contained the times of the superior geocentric conjunction with the corresponding ratio of the axis $\frac{a}{b}$, where a denotes the great, and b the small semi-axis. The sign − signifies that the satellite has in the superior conjunction a southern jovicentric latitude, or that we see the southern part of the plane of the satellite's orbit. The sign + would accordingly denote the visibility of the northern part of it. At the end of the column for each satellite will be found the tables of reduction belonging to it.

Their use may be learnt by the following example. Let it be required to find the position of the two satellites for April 14th, $15^h\ 14'{\cdot}2$; then the next preceding geocentric conjunctions are to be taken together with the value of $\frac{a}{b}$ for the given time.

Sat. I.	April 14.	13^h	$53'{\cdot}5$	$-63{\cdot}7$
II.	—— 13.	9	$57{\cdot}1$	$-63{\cdot}7$

These deducted from the given time present the arguments for the tables of reduction

Sat. I.	0^d	1^h	$20'{\cdot}7$
II.	1	5	$17{\cdot}1$

For these we take from the tables

Sat. I.	$x = +1{\cdot}13$,	$y' = +5{\cdot}59$
II.	$= +7{\cdot}55$,	$= -5{\cdot}01$

The latter ones (viz. y') divided by $\frac{a}{b}$ then give, with proper regard to the signs, these positions:

Sat. I.	$x = +1{\cdot}13$,	Sat. II.	$x = +7{\cdot}55$
	$y = -0{\cdot}09$.		$y = +0{\cdot}08$.

Both x and y are expressed in radii of Jupiter; x is the abscissa on the great axis of the ellipsis of the satellite taken positively in the direction of its motion or eastward; y in the direction of the jovicentric latitude, the northern one being positive. In the field of the telescope a positive x will indicate the satellite's position to the right of Jupiter, and a positive y its position to the south of it. It appeared unnecessary to give the angle which the great or small axis forms with a circle of latitude or declination, as the belts of Jupiter sufficiently exhibit the position of the great axis.

For the third and fourth satellites the eclipses have not been given, but instead of them the times of the middle of the eclipses taken in such a manner as if they were visible; and the semiduration is added in order to reduce to the same form the moments in which no eclipse takes place. The calculation has been executed by M. Wolfers from Delambre's latest tables of the satellites. However well founded M. Hansen's remark respecting the incorrectness of the table of equation C for the first satellite may be, yet it was preferred to retain this table unchanged, partly because some other corrections in the preface likewise require amendments, partly because it is doubtful whether Delambre has not compared the observations with tables which contained the same small deviations. At any rate the difference will seldom amount to 1″ for the first satellite, and for all the others it will always be within this limit. The last page of this section contains the elements for the geocentric form of the ring of Saturn, with the explanation of the notation. For the node and inclination, Bessel's paper in the *Astron. Jahrbuch* for 1829, and for the dimensions of Saturn's ring, the measurements of Struve in the *Astron. Nachr.* v. No. 97, have been used.

The following section contains the places of the pole star, δ *Urs. Min.* and of Bessel's 45 stars, after the model of the excellent auxiliary tables of Prof. Schumacher, with this only difference, that the inferior culminations of the two polar stars have not been given. The apparent places refer to the time of culmination at Berlin, and the asterisk denotes that in that place not ten, as everywhere else, but eleven sidereal days are to be taken.

The mean places on which these positions are founded are enumerated together in the beginning. Their comparison with the corresponding numbers in the auxiliary tables for 1827, has shown that there is a small deviation in some of those stars only, which Bessel has not introduced into his fundamental catalogue, probably owing to a differently assumed proper motion. They are all founded on M. Bessel's latest determinations. The calculation has been performed with great care by M. Dannemann.

The daily aberrations for the polar stars are given below for the culminations. Its values for the other stars are to be found by the side of the last of them, α *Andromedæ.* For the reduction of other stars from their mean places in the beginning of the year to the apparent ones at any other time, two tables have been added, whose construction will be evident from the formulæ* at the beginning of this section. The first, the well-known one of Bessel, is accompanied with the necessary illustration of the manner of forming the argument. It is arranged by sidereal days. The other, which proceeds by mean solar days, is more convenient, if, as is the case in observations of comets, a single position only of a star is required; as it is constructed after Gauss's general tables, and does not require the calculation of the constants *a*, *b*, *c*, *d*. A small difference in the elements on which these tables are founded, which has been noticed too late, causes a want of rigorous agreement in the results; but the difference never exceeding hundredth parts of a second, it is of no consequence for practical purposes.

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* The tables here described, together with the formulæ alluded to, we hope to be enabled to publish in a future Number of the Phil. Mag. and Annals.—EDIT.

The last section contains the principal phænomena for observations.

In the first place the eclipses of the moon and sun are sufficiently described for determining the places on the earth where they will be visible. For those who are fond of constructions, the elements from which the phænomena may be determined, have been added at the end. In future the solar eclipses, which are interesting for our part of the world, will be treated rather more in detail.

Next follow the constellations of the planets. In these, regard has been had to the two principal points of the elliptic orbit, the perihelium and aphelium, the four principal elements of the position of their heliocentric orbit, the two nodes, and the maximum and minimum of their latitude, to the four principal elements of their synodic paths ☍ ☌ □, or those which correspond to them for the sun and superior planets.

I am not acquainted with any astronomical use of the conjunctions of the moon and stars commonly given in astronomical almanacs. It appears to me that the columns of right ascension and declination of the moon render this part quite superfluous; as the arrangement of our present catalogues of stars is such that there can be no trouble in determining the stars near which the moon will pass. These have, therefore, been omitted; but, on the contrary, the possible occultations of planets have been carefully investigated; and where there is a possibility of an occultation, should it even not really take place at Berlin, the conjunction has been noted. In the year 1830, Venus only will undergo an occultation at Berlin.

In the present almanac for the year 1830, no occultations of fixed stars by planets have yet been inserted; as I entertained doubt respecting the extent to which these investigations ought to be carried. An examination has proved that no bright star down to the third magnitude will experience such an occultation. I will leave it to the decision of astronomers whether this examination ought to be extended to the stars of the fourth, fifth, and sixth magnitudes. A rigorous investigation to that extent insuring the certainty of not one having been omitted, would require considerable labour. Next come the moon-culminating stars, or those stars which being near the parallel of the moon at the time of her culmination are likewise not far distant from her in time. It was originally not my intention to give them. However successful the observations have been by the ready publication of them in Prof. Schumacher's *Astr. Nachr.*, they will then only be of use if a single catalogue only is annually published. Nothing but the request of a much esteemed correspondent, in consequence of the fear entertained in the beginning of this year that the publication of such a catalogue would be interrupted, has induced me to give the present one; should, however, another catalogue containing, perhaps, a greater number of bright stars be sent from England, I would request that this one should be laid aside. Its construction has been performed rather hastily; more convenient stars might perhaps have been selected, especially for the second limb of the moon.

In the application of it for ascertaining longitudes even with only moderate transits, it will be adviseable to consult principally the excellent paper by Prof. Nicolai, *Astr. Nachr.* ii. No. 26, and those by Prof. Bessel and M. Hansen on the calculation of the horary motions of the moon, *Astr. Nachr.* ii. No. 33.

The notation and the places of the stars, both of the moon-culminating stars and of those mentioned in the list of occultations which follows, have been taken from the excellent Catalogue of zodiacal stars, by which Mr. Baily has conferred a valuable benefit on the astronomical public.

From this Catalogue all those stars have been inserted which will be occulted during the time of the moon's being above the horizon, while the sun is below it. Sometimes those have likewise been mentioned which are so nearly approached by the moon's limb, that a calculation only could decide whether an occultation would take placeor not. For bright stars, the occultations which happen in the day-time, especially those of *α Tauri*, have likewise been inserted. Some immersions and emersions which happen below the horizon of Berlin, have still been given, because they may be visible in other places.

The headings show the contents of the columns of this section. The two columns "Ort" (place) indicate in what point of the moon's disc the immersions and emersions take place: the degrees being counted from that point of the disc

which

which has most northern declination, through east, south and west, to 360 degrees. The immersions happen, therefore, with few exceptions in the two first quadrants, and the emersions in the two last.

After the occultations of stars follow the mean places of the stars occulted, taken from Baily's Catalogue, and so expressed as they are wanted in the calculation of the occultations, which forms the subject of another part of this book*.

The auxiliary tables which then follow will be explained in the paper just alluded to. The horizontal equatorial parallax here given, belongs to the moment of the moon's culmination; and the quantities $\Delta\alpha$ and $\Delta\delta$ added to the mean place of a star for 1830, give very nearly the apparent place of each star which can be occulted by the moon.

The uncertainty about the extent of the calculation and the distribution of the different parts, however worthy of a place in this almanac, has caused such frequent changing and copying, that possibly some single errors may thereby have been introduced. Should, therefore, some erroneous data occur, (for the communication of which I shall be very thankful,) I request that the blame may not be laid on the labours of my coadjutors, who have performed their part of the work with great sacrifices. Every imperfection which may be pointed out, and every improvement both in form and matter which may be suggested, shall be duly attended to.

The typographical beauty of the printing, a production of the Academical Press of this city, which has been carried to a high degree of perfection by a great sacrifice on the part of the whole Academy, and particularly of an individual member of it, one of those whose services are most in request,—will, it is hoped, satisfy every one as a worthy accompaniment, without laying any claim to ornament or splendour. On a close examination, judges in these matters will not fail to perceive the careful management of the superintendent of the press, who has spared no pains in the first arrangement to give all the aid of his experience to the execution of my wishes.

XLV. *On an improved Syphon-Hydrometer. By Mr.* HENRY MEIKLE†.

IN the Philosophical Magazine for September 1826, (vol. lxviii. p. 166) I proposed an instrument to be used as a hydrometer, consisting of an open glass tube thrice bent, so as to have four straight parallel legs; and though a little complex, it has obviously the property of being free from the effects of capillary action. In the Edinburgh Philosophical Journal for January 1827, a method is given for applying the simple syphon to the same purpose, but which is not so entirely unaffected by capillary action. The same method, however, appears to be still susceptible of considerable improvement, so as to be rendered one of the simplest, and at the same time furnishing a pretty accurate instrument. What I would suggest, as likely to render a simple glass syphon very convenient as a hydrometer, is merely to put a small hole in its upper or bent part. On immersing each leg of such a syphon in a separate liquor, a portion of the air escapes through the hole, and allows the liquids to rise in the tubes to the level of their cisterns. If we now apply the finger to the hole, and raise the instrument, I need scarcely say, not wholly out of the fluids, the liquors will be raised in the tubes by the pressure of the atmosphere, so as to form columns elevated to heights above their respective cisterns inversely proportional to their specific gravities. For the weights of the two columns must obviously be equal; each being the difference between the pressure of the atmosphere and that of the included air.

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* The part of the Ephemeris here mentioned, will probably appear in a future Number of the Phil. Mag. and Annals.—EDIT.

† Communicated by the Author.

If the tube be pretty wide, the effect of capillary action may in most cases be neglected: so that, the one column being water, we divide its length by that of the other liquid, and obtain a quotient which is the specific gravity of the latter. But the effect of capillary action may be easily obviated altogether, by holding the syphon at two different heights, and noting the corresponding columns. Thus if at one height we have a column of water $=W$, and at another height $=w$; while the corresponding columns of another fluid are respectively A and a; the specific gravity of the latter, freed from capillary action, is

$$\frac{W-w}{A-a}.$$

For, if x be the capillary part of the column of water, and y that of the other fluid, we should correct the columns for capillary action, by diminishing them respectively by x and y; whence the specific gravity becomes

$$\frac{W-x}{A-y}=\frac{w-x}{a-y}=\frac{W-w}{A-a}.$$

We thus obtain the specific gravity free from capillary action, by dividing the difference of the columns of water by the difference of those of the other fluid. The greater these differences can be made, so much the better; even using, perhaps, for the shorter columns the mere capillary elevations.

In transparent liquors, we may make the one pair of columns A and W to be depressions under the surfaces of the liquids contained in glass cisterns; which may be effected by immersing the instrument with the hole previously stopped to confine the air. The depths of the depressions, when increased respectively by x and y, should, like the former columns, be inversely as the specific gravities of the liquids. Consequently the required specific gravity becomes

$$\frac{W+x}{A+y} = \frac{w-x}{a-y} = \frac{W+w}{A+a},$$

which, as the numbers may be larger, is likely to be more accurate than the former. For in this case we obtain the specific gravity by dividing the sum of the elevation and depression of the water by the sum of those of the other fluid.

There is still another case in which all the columns may be depressions, and which obviously makes the specific gravity

$$\frac{W+x}{A+y} = \frac{w+x}{a+y} = \frac{W-w}{A-a},$$

where the rule is the same as in the first case. I may remark, that in all the cases, we might use in place of one of the pairs of columns, the mere capillary elevations, with proper signs.

As a hole may be apt to weaken a glass tube, especially at the curved part where it should be strongest; two straight pieces of glass tube may be joined, as I have done, by means of a bit of bent tin-tube. The hole may then be more easily made, and will be less apt to weaken the instrument. The legs of the syphon should be graduated or divided into small equal parts. This may be very easily done by merely transferring to the tubes, with the assistance of a square, the divisions which are already made on any scale of small equal parts. It is obvious that the legs ought to be parallel.

Nearly allied to the syphon-hydrometer, is a more complex instrument, called *a pump-areometer.* It is so named, from its being furnished with a pump at its upper part for exhausting the air to induce the liquids to rise. The requisite degree of exhaustion, however, is so very trifling, that it may be effected, were it in the least necessary, by merely sucking with the mouth at the orifice of a stopcock attached to the top of a syphon.

HENRY MEIKLE.

XLVI. *A new Account of the Genus* Echeveria. *By* A. H. Haworth, *Esq. F.L.S. &c. &c.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

HAVING just examined and described a fine new succulent plant at Mr. Tate's Nursery in Sloane-street, which he has recently raised from Mexican seeds, and which is now blooming for the first time in Europe, amongst many other equally rare and well-managed plants; I send you hereunder a full account of it, and four other species of the same genus; to which I have added all their synonyms.

This new plant, belonging to DeCandolle's new genus *Echeveria*, I have called, from its ample leaves, *Echeveria grandifolia.*

In my ninth decade of new succulent plants, published in your Journal in 1826, I announced to you (*inter alia*) that *Cotyledon coccinea* of Cavanilles, and *Cotyledon umbilicus* of Linné, would each form the type of a new genus, but for want of proper specimens regretted my inability at that time to give you sufficient characters. This, however, is now the less to be lamented, as DeCandolle has done it for us, in the third volume of his *Prodromus*, just published; and to the above-mentioned *Cotyledon coccinea* has added my *Cotyledon cæspitosa*, (which is a native of California, although we used to think it African,) and two new species from Mexico.

If you can find room for this communication in the next Number of your Journal, you will much oblige

Your old correspondent and friend,

Chelsea, Aug. 10, 1828. A. H. Haworth.

Ordo Naturalis.

Crassulaceæ DeCandolle. *Semperviva* Juss. &c.—*Cotyledones americanæ* Auctorum.

Genus, Echeveria *DeCand. Prod.* 3. 401.

Generis Character.

Calyx 5-partitus, sepalis distantibus valdè foliiformibus, præinæqualibus basi coalitis. *Corolla* pentapetaloidea pentagona campanularis. *Petala* infernè concreta erecta rigidula acuta crassa, basi inter calycis folia, extùs gibba, intùs scrobiculata: duobus exterioribus petalis insuper tria interiora, arctè imbricatim adpressa. *Stamina* 10, basi cum petalis longioribus concreta. *Squamæ* ordinariæ

breves subquadratæ albæ, alternæ crassiores cerinæ, omnes in petalorum scrobiculis nidulantes. *Carpella* 5 erecta, in *stylos* acutos desinentia.

Suffrutices Mexicani parvi succulenti glauci. *Folia* basi soluta: rosularia sed alterna, integra, cum mucronulo, at sæpiùs valdè obtusa, et in spicis florigeris foliolosis, pedetentim in *bracteas* numerosas magnas subdistantibus omninò foliiformes abeuntia. *Flores* valdè bracteati, spicati, s. paniculati, vel cymosi, et tunc secus cymæ ramos sessiles, coccinei flavive.

SPECIERUM CHARACTERES.

* Suffrutices, floribus paniculatis spicatisve, coccineis.

grandifolia. E. (great-leaved) foliis orbiculato-cuneatis grossè
1. petiolatis, floribus paniculato-spicatis.

Habitat in Mexico.

Floret Aug. Sept. G. H. ♄.

Caudex in nostro exemplo, in caldario, apud Dom. Tate, in secundo anno triuncialis diametro subunciali, cylindricus carnoso-lignescens radiculos exiguos terram versus exerens. *Folia* numerosa conferta ambienter multifaria, seu in rosulam laxam digesta, patenti-recurvula dodrantalia plùsve incurvo-concavula, et in petiolum carnosum subsemunciam crassum obtusè canaliculatum attenuata, pruinoso glauca rufo marginata integra rariùsve minutim asperiuscula; *subtus*, basin versus præcipuè vivacitèr glauco-purpurascentia: et denique morientia inania lorea persistentia. *Florum* paniculæ sesquipedales, bracteatim foliolosæ, axillares teretes uti folia cæruleo-glaucæ; *bracteis* erectis lanceolatis mucronulatis (magìs quàm vera folia) distantibus sensim sensimque minoribus, et *Sedi* more singulariter basi planè obtusèque solutis. *Calyx* sepalis 5 valdè inæqualibus bracteis brevioribus omninò foliiformibus (excepto basi non soluto) tribus cæteris duplò majoribus, quarto minore, quinto minuto. *Corolla* ferè semunciam longa, calyce brevior rubro-aurantiaca, rore roseo-glauca purpureave. *Stamina* 10, petalis humiliora alba, *antheris* erectis *polline* luteo. *Carpella* grossa, alba in *stylos* virides abeuntia. Cætera ferè ut in *E. coccineâ*, infrà descripta, at non rectè vidi.

gibbiflora. E. (gibbous-flowered) foliis planis cuneiformibus
2. acutè mucronatis ad apices ramorum confertis, paniculâ patente, floribus secus ramos breviter pedicellatis. *DeCand. Prod.* 3. p. 401.

Habitat in Mexico. ♄.

Petala basi albida, apice subcoccinea. *DeCand.* l. c.

coccinea. E. (pubescent) mollis: ramulis foliisque spatulato-
3. lanceolatis pallescente densè puberulis, florum spicis axillaribus elongatis foliolosis.

Cotyledon coccineum. *Cav. Ic.* 2. *t.* 170.—*Nob. in Suppl. Pl. Succ. p.* 26. A.D. 1819.—*Lodd. Bot. Cab. t.* 832.—Echeveria coccinea. *DeCand. Prod. v.* 3. *p.* 401.

Habitat in Mexico.

Floret autumno, hyemeve. G. H. ♄.

Suffrutex sesquipedalis, parùm et alternè ramosus. *Flores* densè elongato-spicati, *spicæ* sub foliorum capitulos adscendentes thyrsiformes bracteatim foliolosæ, supernè ferè comosæ, post florescentiam longum per tempus induratè denudatæ persistentes. *Flores* duplò minores quàm in *E. grandifoliâ*, horizontaliter sessiles. *Calycis* sepala subovato-lanceolata acuminata tumidè carnosa patenti-recurvula et inter bracteas irregulariter intertexta. *Corolla* 5-petaloidea campanulata coccinea, infernè pentagona, basi gibbulis 5, e foveolis totidem internis nectariferis: laciniæ (corollæ) rectæ ovato-lanceolatæ, acuminatæ calyce breviores carinâ densiùs ciliato-puberulâ saturatiore; intùs longè pallidiores glabræ foveolatæ. *Stamina* 10, albo-lutescentia corollâ subdimidiatim breviora, 5 exteriora germinum basi inserta, 5 alia in foveolâ supradictâ dimidiatim flexuosè adnata, solùm supernè libera, et 5 prioribus (staminibus) aliquantillum humiliora. *Antheræ* erectæ subparallelipipedæ emarginatæ, basi cordatæ, *polline* luteo. *Carpella* 5, erecto-adpressa, cum *stylis* continuantibus obclavatis luteis, *stigmate* purpureo inconspicuo, lente hemisphærico, et subinde ad lucem pellucente.

Obs. Corollæ laciniæ intùs argutè canaliculatæ, in quibus canaliculis insident exteriora filamenta, et ad eas adpressa, atque ad earum flexionem gibber parvus exstat utraque insuper apicem singulæ foveolæ supradictæ.

Ad basin singuli *carpelli* rudimentum solum squamulæ ordinariæ exstat tumidulum rhombeum subquadratumve, carpello omninò adnatum, pustulam minutissimam simulante. *Seminula* numerosa incipientia oblonga alba solùm vidi.

teretifolia. E. (cylindric-leaved) foliis teretibus acutis sparsis
4. basi subsolutis, spicis secundis paucifloris. *DeCand. Prod.* 3. *p.* 401.

Habitat in Mexico. ♄. *Flos* omninò prioris. *De Cand.* l. c.

** Subherbaceæ, floribus subcymosis luteis.

cæspitosa. E. (dwarf yellow-flowered) foliis rosularibus an-
5. gustè linguiformibus, apice obcuneatis submucronatis, floribus cymosis.

Cotyledon cæspitosa. *Nob. Misc. Nat. p.* 180. A.D. 1803.—Cotyledon linguiformis. *Ait. Hort. Kew. v.* 3. *p.* 109.—Sedum Cotyledon. *Jacq. f. Eclog.* 1. *f.* 17.—Cotyledon reflexum. *Willd. Enum. Suppl. p.* 24.—Echeveria cæspitosa. *DeCand. Prod. v.* 3. *p.* 401.

Habitat in Californiâ.

Floret Jul. Aug. G. H. ♃. s. ♄.

P.S. I avail myself of the present opportunity of correcting the following errors, which time alone has enabled me to ascertain.

1. *Mesembr. deflexum*, β. Revis. Pl. Succ. p. 140, is the same as *M. imbricans*, p. 139, and the last is a good and most abundant-flowering species.

2. *M. leptaleon* is the young state only of *M. retroflexum*, l.c.

3. *M. flexile* is the young state only of *M. polyphyllum*, l. c.

4. And *M. torquatum* is a casual state only of *M. floribundum*. Revis. Pl. Succ. p. 187.

DeCandolle in vol. 3. p. 416 of his *Prodromus*, just published, says of *Mesembryanthema* : "Species pleræque hortenses ex cl. Haworth, &c.—sed forsan plures ut meræ varietates habendæ."

But all the new species which I have published, I was well and sufficiently assured, were raised from *wild* African, or Australasian seeds, except the following only, whose origin I cannot trace further than I have printed. *M. ficiforme; M. hybridum; M. nobile; M. mustellinum ; M. bigibberatum; M. cruciatum; M. Salmii; M. cultratum; M. coruscans; M. procumbens; M. variabile; M. mucroniferum; M. loratum*, and *M. hispifolium*. And even seven of the above I received from His Highness the Prince de Salm Dyck, whose genuine origin I believe he can point out.

In my last communication Phil. Mag. and Annals, N.S., vol. iii. p. 184, l. 15, for *Crassulam undosam*, read *Crassulam undatam*.

XLVII. *An Account of the Formation of Alcoates, Definite Compounds of Salts and Alcohol analogous to the Hydrates.* *By* THOMAS GRAHAM, *Esq. M.A. F.R.S.E.*[*]

IN determining the solubility of salts and other bodies in alcohol, it is desirable to operate with a spirit wholly free from water. But anhydrous or absolute alcohol is formed with difficulty, even by the most improved process—that of Richter. In rectifying alcohol from chloride of calcium, as recommended by Richter, I have never obtained it under the specific gravity 0·798 at the temperature of 60°, by a single distillation; but upon rectifying this product again from new chloride of calcium, I generally succeeded in reducing it to 0·796, which is the specific gravity of the standard alcohol of that chemist. The following experiment illustrates this process.

Four measures of alcohol of the specific gravity 0·826 were poured into a retort, and a quantity of well-dried chloride of calcium, amounting to three-fourths of the weight of the alcohol, gradually added with occasional agitation. Much of the salt was dissolved with the evolution of heat; and the combination was promoted by boiling the whole for a few minutes, the vapour being condensed in the neck of the retort, and returned to the solution. A receiver was then adjusted to the mouth of the retort, and the distillation conducted so slowly that the alcohol was condensed entirely in the neck of the retort, and fell drop by drop into the receiver,—nearly two seconds elapsing between the fall of each drop. The first measure of alcohol which came over was of the specific gravity 0·800, at 60°; the second measure, 0·798; and the third measure, 0·801: the distillation was then discontinued. These three measures were mixed together, and subjected to a second distillation, which was conducted in the same manner; and two measures of alcohol obtained of the specific gravity 0·796. It was found that further rectification did not reduce the specific weight of the alcohol below 0·796. From the analysis of alcohol by Saussure, and the determination of the specific weight of its vapour by Gay-Lussac, there can be little doubt that the alcohol thus obtained is perfectly anhydrous. It is true that such alcohol still contains oxygen and hydrogen to the amount of an atomic proportion of water; but this proportion of oxygen and hydrogen is essential to the constitution of alcohol,—the partial abstraction of it converting alcohol into æther, and its total abstraction converting alcohol into olefiant gas; while the supposition that the oxygen and hydrogen exist in the state of water, is altogether gratuitous.

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[*] From the Transactions of the Royal Society of Edinburgh: this paper was read before the Society on the 17th of December 1827.

The process of Richter is exceedingly tedious, from the necessity of conducting it so slowly, and the waste of alcohol is considerable. I tried newly burnt quicklime instead of chloride of calcium, and distilled by the heat of a saline water-bath. If it is merely our object to obtain alcohol perfectly free from water, no process could be more effectual. The product was of the specific gravity 0·794; but it contained a trace of æther, to which the extraordinary lowness of its specific gravity is attributable; and had an empyreumatic odour, notwithstanding the moderate temperature at which the distillation was conducted. This likewise is a very slow process.

The process which I preferred is founded on the principle of Mr. Leslie's frigorific apparatus. The alcohol is concentrated by being placed under the receiver of an air-pump, with quicklime. A large shallow basin is covered to a small depth with recently burnt lime in coarse powder, and a smaller basin containing three or four ounces of commercial alcohol is made to rest upon the lime: the whole is placed upon the plate of an air-pump, and covered over by a low receiver. Exhaustion is continued till the alcohol evinces signs of ebullition, but no further. Of the mingled vapours of alcohol and water which now fill the receiver, the quicklime is capable of combining with the aqueous vapour only, which is therefore quickly withdrawn, while the alcohol-vapour is unaffected. But as water, unless it has an atmosphere of its own vapour above it, cannot remain in the alcohol, more aqueous vapour rises. This vapour is likewise absorbed, and the process goes on till the whole water in the alcohol is withdrawn. Several days are always required for this purpose, and in winter a longer time than in summer. The following cases exhibit the rate, according to which the water is withdrawn. The first experiment was made in summer. Four ounces of alcohol of the specific gravity 0·827 were concentrated. The specific gravity was taken every twenty-four hours, and the following series of results obtained:

0·827
0·817
0·808
0·802
0·798
0·796

In this case the whole water was withdrawn in five days, but occasionally a period somewhat longer is required, although it rarely exceeds a week. In winter the alcohol generally requires to be exposed to the lime for a day or two longer than in summer. The following rate of concentration was observed in one case in winter, the quantity of alcohol and other circumstances being the same as in the former experiment:

0·825
0·817
0·809
0·804
0·799
0·797
0·796

Quicklime, as a porous substance, appears to be capable of condensing a small portion of alcohol-vapour. It is therefore improper to use it in great excess. In one case, in which three pounds of quicklime were employed with four ounces of alcohol, about one-sixth of the alcohol was lost from this absorption. The quicklime should never exceed three times the weight of the alcohol, otherwise the quantity of alcohol absorbed becomes sensible. It should be spread over as great a surface within the receiver as possible.

In Richter's process it is improper to operate upon more than a few ounces of alcohol at a time; as when a large quantity of materials is introduced into the retort, the heat necessary to disengage the alcohol in the centre of the mass inevitably expels the water left in the chloride of lime, at the points where it is more exposed to the heat. In the air-pump also, only a few ounces can in general be concentrated at a time. But in a tall receiver, two or three shallow basins of quicklime can be supported at a little height above each other, each of them containing a small basin of alcohol resting in it. Or the process might be conducted with facility on the large scale, by means of a tight box of any size, furnished with numerous shelves, which might be covered with quicklime in powder, and support a large number of basins of alcohol. The box might be sufficiently exhausted of air by means of a syringe, for it is not necessary that the exhaustion be nearly complete; and indeed more inconvenience is to be apprehended from a complete than from an imperfect exhaustion. After producing the exhaustion, no further attention would be necessary; and upon opening the box at the expiration of a week or ten days, the alcohol would be found anhydrous. It is evident that absolute alcohol, procured by this process, could be sold at a price but little exceeding its original cost. It would moreover be of much greater value for the purposes for which it is employed in the arts and in medicine. I believe, however, that, by the excise laws as they at present exist, no rectifier of spirits is permitted to concentrate alcohol beyond a certain strength. Licensed apothecaries alone are allowed to prepare and sell absolute alcohol*.

Alcohol may be concentrated in a close vessel with quicklime, without exhausting; but the process goes on much more slowly, at least at the temperature of the air. The experiment was tried at a high temperature, by heating in a water-bath a large bottle with a very wide mouth, containing a quantity of alcohol at the bottom, and quicklime suspended over it in a linen-bag. When the water-bath attained the temperature of 150°, the bottle was corked, and the bath prevented from becoming hotter. Much of the lime was very quickly converted into hydrate, and the alcohol considerably concentrated. But the process is troublesome, and much inferior to that in which the air-pump is employed.

In the place of quicklime, sulphuric acid cannot be substituted in the foregoing process as an absorbing liquid, from a remarkable property which it possesses. It is capable of absorbing the vapour of absolute alcohol, in the same manner as it absorbs the vapour of water. I was led to make this observation from a consideration of the phænomena which attend the mixing of alcohol and sulphuric acid. Nearly as much heat is evolved as if water had been added to the acid, even although absolute alcohol be employed. Alcohol is also retained by the acid when heated to 500° or 600°, or at a temperature when the alcohol would be decidedly in the state of vapour,—which indicates the possibility of the same relation between sulphuric acid and alcohol vapour, that subsists between water and those gases which it detains in the liquid state, such as ammoniacal gas, when they would naturally assume the elastic form. But besides merely *detaining* such gases, water can condense and absorb them. Sulphuric acid, besides merely detaining alcohol-vapour, might therefore condense and absorb it.

As alcohol, like water, occasions cold by its evaporation, it may be substituted for water in Mr. Leslie's frigorific apparatus, sulphuric acid being retained as the absorbing liquid. In circumstances precisely similar, it was found that a thermometer, the bulb of which was covered with cotton, fell to 7° when moistened with water, but when moistened with absolute alcohol its temperature fell to —24°. Continuance of the pumping during the experiment, as is done in the case of æther, had a prejudicial effect. But alcohol diluted with a third of water was found to have as great a cooling power as absolute alcohol. The advantage to be derived from the great volatility of alcohol appears to be counterbalanced in part by the small latent heat of its vapour. Probably a mixture of alcohol and water, in certain proportions, would produce the greatest degree of cold attainable by this process. Sulphuric acid loses its power to absorb alcohol-vapour by being diluted with water. When impregnated with alcohol-vapour, the acid becomes of a pink colour; but no appretiable quantity of gas is emitted at the temperature of the atmosphere, even in the vacuum of an air-pump.

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* Care should be taken that the temperature be nearly equable during the experiment; otherwise, when the atmosphere becomes cold, a condensation of alcohol-vapour takes place upon the cooled bell-glass, which runs down upon the plate of the pump. The experiment, therefore, should not be performed in a room with a fire, or near a window, but in a dark closet or press. From the manner in which I performed the experiment, this condensation had never been experienced by myself; but Dr. Duncan junior observed it, on repeating the process.

From one experiment, water appears to have the power to induce the evaporation of alcohol by absorbing its vapour, as sulphuric acid does, but much more feebly. Two cups, one containing alcohol and the other pure water, were inclosed together in a tin-canister which was nearly air-tight, and set aside in a quiet place for six weeks. The cups were not in contact, but a little apart from each other. At the expiration of that period it was found, on opening the canister, that the cup which originally contained pure water, now contained a mixture of water and alcohol, while the alcohol remaining in the other cup was of diminished strength. Professor Leslie informs me, that he performed a similar experiment a considerable time ago, although no account of it was published. But the absorption of alcohol-vapour by water is so feeble as not to occasion a sensible reduction of temperature in the alcohol.

Chloride of calcium is disqualified as an absorbent of aqueous vapour in the purification of alcohol, for the same reason as sulphuric acid. I find that chloride of calcium absorbs the vapour of absolute alcohol, and runs into a liquid, or it *deliquesces* in alcohol-vapour. A small quantity of this substance was suspended in a little capsule, at the height of two inches above a quantity of absolute alcohol, in a close vessel. In the course of twenty-four hours it was entirely resolved into a liquid, just as if it had been suspended over water. The liquid proved to be a solution of chloride of calcium in absolute alcohol. The experiment was frequently repeated. As salts which deliquesce from the absorption of aqueous vapour are always capable of forming hydrates, I was led from the observation of this fact to attempt the formation of analogous compounds of alcohol and salts,—to which I now proceed.

These solid compounds of salts and alcohol, which are definite and imperfectly crystallizable, may be denominated Alco*ates*,—a designation which is not unexceptionable, but appeared to me preferable to the name *Vinates*, as there is a sulpho-vinous acid; or to any other name that might have been imposed upon them.

The alcoates which I succeeded in forming are not numerous. They were formed simply by dissolving the salts, previously rendered anhydrous, in absolute alcohol, with the assistance of heat. On cooling, the alcoates were deposited in the solid state. The crystallization was generally confused, but in some cases crystalline forms appeared of a singular description. The crystals are transparent, decidedly soft, and easily fusible by heat in their alcohol of crystallization, which is generally considerable, amounting in one instance to nearly three-fourths of the weight of the crystals.

I. *Alcoate of Chloride of Calcium.*

Pure muriate of lime was dried as much as possible on a sand-bath of the temperature of 600° or 700°, and then slowly heated to redness, and retained for some time at that temperature. The dry chloride of calcium thus obtained dissolves in absolute alcohol at 60° with great facility, and with the production of much heat, sometimes occasioning the boiling of the solution. The quantity of chloride taken up increases with the temperature; and at 173°, the boiling point of alcohol, 10 parts alcohol dissolve 7 parts chloride of calcium. This solution is thick and viscid, but perfectly transparent, provided the chloride be pure. It boils at 195°, alcoholic as well as aqueous solutions boiling at higher temperatures than the pure liquids. The viscidity of the solution of chloride of calcium increases greatly as it cools. Bright crystalline stars soon appear on the surface and on the sides of the vessel, which have been moistened by the solution. The solution, however strong, never crystallizes instantaneously, but gradually, in thin transparent and colourless plates, the forms of which cannot be made out, except on the surface of the solution and sides of the vessel.—To obtain the alcoate in a state of absolute purity, it is necessary to form a solution so weak, that, while hot, it will pass through thin filtering paper; and afterwards to concentrate the filtered solution by heat. A solution of one part chloride of calcium in five parts alcohol passes through the filter. It is remarkable that the most distinct crystalline forms are not obtained from the slow crystallization of comparatively weak solutions; but in solutions which have been fully saturated, or nearly so, at the boiling temperature. In the former case, the crystalline plates are large, but confused, and nothing but angles can be made out; while in the latter, the forms, under which the plates appear on the surface of the solution, and to the greater advantage, on the sides of the vessel, are generally distinct. These plates are always small, often beautiful, and delicately striated; and they always present the form of isosceles triangles. In general, four of these triangular figures are grouped with their apices together; and if similar, they form a square. But, as more frequently happens, the opposite pairs of triangles only are similar; and the figure presented is a rectangular parallelogram, divided by two diagonal lines into four triangles. The resolution of the rectangle into triangular figures is rendered perceptible by the discontinuance of the striæ, and the formation of clear diagonal lines, which have a beautiful effect. These crystals cannot be removed from the phial in which they are formed without injury, from their softness. Exposed to the air, they speedily deliquesce from the absorption of hygrometric moisture. The heat of the hand is sufficient to melt them. The whole of the alcohol is expelled by a heat amounting to 250°, and pure chloride of calcium remains, which emits nothing else upon being heated to redness.

A quantity of this alcoate was dried, first by strong pressure between many folds of linen, and then by pressure between folds of blotting paper. The alcoate, carefully dried in this way, had a white appearance much resembling bleached wax, and was soft, but without tenacity.

Ten grains were heated in a glass capsule, till the whole of the alcohol was driven off. There remained 4·1 grains chloride of calcium. The atomic weight of chloride of calcium is 7, and that of alcohol 2·875. In the alcoate, 4·1 grains chloride of calcium were combined with 5·9 grains alcohol.

$$4·1 : 5·9 :: 7 : 10·0731.$$

In a second analysis, in which 20 grains of alcoate were employed, the result was precisely similar, as 8·2 grains chloride of calcium remained, which is just double what was obtained in the previous case from half the quantity of alcoate. If this alcoate should be considered a compound of one equivalent proportion of chloride of calcium, and three and a half proportions alcohol, the alcohol would amount to 10·0625, which approaches very nearly to the experimental results. But it would be better to express the composition of the alcoate thus:

Two atoms chloride of calcium	14·
Seven atoms alcohol	20·125
	34·125

In the solution of chloride of calcium, no crystallization takes place at the temperature of 50°, when the alcohol exceeds the proportion of 10 parts to 4 parts of the dry salt. But the solution crystallizes readily when further concentrated. A solution saturated at 170°, and which consisted of 10 parts alcohol and 7 parts chloride of calcium, or nearly the atomic proportions of the alcoate, crystallized slowly upon cooling, forming crystals upon the surface of the liquid and sides of the phial, of great regularity and beauty. The whole crystallized during a cold night, leaving no mother-liquor whatever.

The injurious effect of the presence of water, in the formation of this alcoate, was evident in alcohol of the specific gravity 0·798, in which the contaminating water did not amount to 1 per cent. A solution of chloride of calcium in alcohol of this strength did not crystallize readily, and the crystals eventually deposited were small and ill-formed. Chloride of calcium does not crystallize at all in alcohol of the specific gravity 0·827. The same inconvenience arises from employing chloride of calcium containing a little water.

Although the alcoate of chloride of calcium in a state of purity is entirely decomposed at a temperature not exceeding 250°, yet, when water is present, alcohol can be retained by the chloride of calcium at a much higher temperature. Thus I repeatedly found, that chloride of calcium, from which alcohol had been rectified, and which afterwards had been washed out of the retort by water, gave indications of the presence of alcohol, after being exposed on the sand-bath to a heat of 400° or 500° for several hours. Transferred in a crucible to the fire, after it ceased to lose weight on the sand bath, alcohol-vapour was emitted, which took fire and burned.

[To be continued.]

XLVIII. *Remarks on the Influence of Terrestrial Radiation in determining the Site of Malaria. By* Wm. Addison[*].

THE diseases arising from atmospheric impregnations have long formed an important topic of inquiry among medical men, and are generally supposed to have an origin from some subtile poison, prevalent only in certain places, or over very circumscribed situations. Upon considering the various circumstances under which these diseases are produced, and the impossibility of any poison dispersed through the air from the ground becoming partial in its operation, or always confined to any particular district (when every wind must waft it away from the spot of its emanation), unless some adventitious circumstance influences its operations,—I am induced not to subscribe to the doctrine which teaches that they take place from a specific or peculiar and locally acting effluvium. On the contrary, I think we shall find that most of the ordinary atmospheric impregnations will produce the diseases of Malaria, when under certain peculiar circumstances they are liberated from their combinations; diseases which will, no doubt, be violent or not, according to the quantity or quality of the matters developed.

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[*] Communicated by the Author.

The atmosphere, as is well known, retains every where mingled with it variable proportions of aqueous vapour, mixed probably with various effluvia arising from the action of the sun upon the many substances on the surface of the earth. During a bright day, therefore, the air over those portions of the ground subjected to its influence becomes saturated with vapour, and any reduction of temperature by radiation will always be accompanied by the deposition of moisture and the precipitation of a portion of those subtile matters drawn up by the agency of heat; whereas any diminution of sensible caloric, which may ensue from a rush of cold air, may not be accompanied with the same effects: for it very often happens that such currents have not nearly attained their maximum point with respect to vapour, and therefore none of these things happen; or if they do, the deposits occur in the form of rain, far less prejudicial than those chilly fogs produced by the *radiation of caloric from the earth.*

When we think of the important process of radiation, the effects of which have excited the attention of philosophers, especially those connected with horticultural pursuits, it is extraordinary that it should wholly have escaped them to pursue their investigations into this curious subject, with reference to the momentous matter of local salubrity; for little doubt remains upon my mind, that a well conducted series of experiments instituted to discover the phænomena resulting from the *radiation of heat into the heavens*, in different situations and over various surfaces and soils in several places at the same time, would discover to us an important field well worthy of research as connected with the health of mankind.

I have already endeavoured to draw the attention of those who may possess opportunity and the means of entering into this interesting branch of inquiry, towards the benefits their labours are likely to confer upon us in a medical point of view*. I have shown that all those situations where the radiation of caloric goes on with rapidity, are occasionally, if not at all times, extremely unhealthy; while others, where this process is diminished, are on the contrary much less obnoxious to disease. I have shown that debilitated constitutions are invariably found to regain the tone and vigour of health much more perfectly and more quickly in places little influenced by radiation or removed from the sphere of its effects, than in others exposed to the depositions which it causes from the air; and I have endeavoured to confirm these observations, by pointing out that in the *radiation of caloric* may be found *the cause of the activity* of those exhalations with which the sun, in tropical climates especially, saturates the air: in fine, that in this important process one of the principal causes of malaria will be found.

I shall here offer a few more facts in support of the views I have taken. And as nothing has tended more to confirm me in them than the perusal of Dr. Macculloch's Essay on the Production and Propagation of Malaria, I shall proceed to the consideration of some of the passages in that publication.

" The careful observer will often perceive," says the Doctor, "that there are certain determinate places without any marshes, where fevers are almost annually prevalent; while other places in the vicinity are almost wholly or nearly exempt. A proof of this may be drawn from the fact that some localities are known to be unhealthy as compared with other neighbouring places.

" Thus it is the vulgar remark, that in certain houses or places a family is rarely without some sickness; or, to use the strong but coarse language in which it is generally stated, ' that the apothecary is never out of the house.' It is almost equally familiar, that families which before had been healthy, have become the reverse on changing houses or situations; as in the opposite cases, that they have recovered health by change of residence. Of such facts as these there is no observer who must not be able to recollect numerous examples." Again, " If a gravelly soil is healthy, it is because its easy drainage prevents the growth of that particular vegetation which is the cause of malaria; and if a clayey soil is the reverse, it is because by lodging superficial water it generates, however partially, those marshy or undrained spots, or wet woods or moist meadows, which are the sources of *this poison*, and consequently of the various diseases confounded under the vague term of unhealthiness."—*Essay*, pp. 19 & 21.

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* Vide the last section of " A Dissertation on the Nature and Properties of the Malvern Water," &c. &c.

Now, upon this latter passage I may remark, that as water is one of the best radiators of caloric, so all wet, low, and marshy places will be found the most affected by it; and it will follow that any soil whose mechanical texture is such as to allow the water to permeate through it, or to drain off, at the same time that other circumstances combine to arrest the dissipation of heat by radiation, that soil will be found much more salubrious than one retentive of moisture, and particularly if the surface of this latter is covered with low herbage or grass, which is in itself an excellent promoter of terrestrial radiation.

" That woods and jungles in hot countries give origin to miasmata of the worst kind is well known to all medical men; but some doubt may be entertained as to their insalubrity in Europe."—" Dr. Macculloch thinks there is strong reason to believe that close and wet woods generate malaria in this as well as in the warmer countries of Europe. Certain woody districts in Sussex and Kent produce both intermittent and remittent fevers,—at least there is no other assignable cause. The same may be said of some parts of Hampshire and Essex; as about Epping Forest, for example."—" On the other hand, we have positive testimony that lands which were healthy when covered with wood, have become extremely unhealthy when cleared and cultivated."

The thick foliage, as I have elsewhere shown, of the trees composing most of the intertropical forests, and even of some of those also in this country, by obstructing the rays of the sun, preserve in their immediate vicinity a greater degree of stillness and a lower temperature than that attained by the atmosphere over the contiguous grounds; whence the heated air coming to slowly circulate among the branches of the trees of these forests, becomes cooled, and its vapours developed; and it is these which occasion the diseases of malaria.—" Yet it requires much circumspection," says the Doctor, " in deciding upon the propriety of clearing these grounds with the view of rendering them more salubrious."—And why? Because trees naturally tend to obstruct the force of radiation; and, if planted on a good radiating surface, not so close together as entirely to obstruct the genial influence of moderate warmth from the sun's rays, or to prevent the free circulation of the air, will prove a valuable defence against the appearance of malaria, by counteracting that unequal distribution of temperature

which,

which, *I believe*, develops its existence in the air: whereas, if these are cut down and the ground cleared, a good radiating surface becomes immediately exposed, and the dissipation of caloric with its accompanying effects directly ensues.—" A portion of grass-plat," says Mr. Daniell in his Meteorological Essays, " under the protection of a tree or hedge will generally be found on a clear night to be eight or ten degrees warmer than surrounding unsheltered parts; and it is well known to gardeners that less dew and frost are to be found in such situations than in those which are freely exposed."—Dr. Macculloch in noticing the comparative healthiness of ancient and modern Rome, thought it not unimportant to notice what Theophrastus has stated with regard to the plain of Latium, which this historian says was covered with laurel and myrtle-trees of such a size as to be used in ship-building; and this remark, if terrestrial radiation has any thing to do with the development of malaria, is not so fanciful as one of his reviewers seems to imagine it*. Again, If terrestrial radiation is the cause of the deleterious influence of those effluvia existing in the atmosphere, we are no longer surprised at finding rice-grounds, which are kept in a constant state of wet or moisture during the growth of the plants, prolific in the diseases which malaria occasions.

" Dr. Macculloch is convinced that the minute marshy or swampy spots which occur in thousands of low situations, whether on commons, near woods by road sides, or in innumerable other places where they hardly ever attract notice,—are productive of malaria; though their limited range of action generally renders their power insensible, unless when houses happen to be erected in their vicinity."—" In how far meadows which cannot be called marshy are capable of producing malaria, is an intricate and entangled question. It appears certain, however, that there are many tracts of meadow or of alluvial land not marshy, and often not intersected by ditches, at least in a conspicuous manner, which are the sources of malaria all over Europe." *Essay*, p. 69.—" Such is the case with all the alluvial tracts at the entrances and sometimes at the exits of the lakes of Switzerland and elsewhere; and in places innumerable where there is no proper marsh, nor even an approach to such a character, but where the prevalent diseases must be owing to malaria."—" Volney, while travelling in America, has averred that every valley in the country which he visited produced the fevers of malaria, enumerating among the sources of this poison not only marshes and wood, but rivers, millponds, &c."—"The meadow lands about Fontainbleau, at the junction of the Yonne and the Seine, are notorious for the *fièvre du pays;* so injurious are they, that few escape intermittents or remittents over a considerable tract." If some great portions of the meadow-land in England have been recovered by drainage from a state of marsh, and are now as dry as the ordinary low-lands of plains and valleys; and if these localities still produce malaria and its consequences,—it is another point of evidence against the salubrity of meadows generally. "It is a rooted opinion in England, that there can be no malaria on the banks of a running stream; and as far as mountain-torrents are concerned, this is probably true: but where rivers slowly meander through low grounds, we must not trust to the mere motion of the water."—"For whatever persons may still think as to rivers in general in our own country, there is no doubt that such streams as the Ouse, the Lee, and all others flowing with difficulty through fertile meadows, and with a flat vegetable margin, are productive of malaria."

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* Medico-Chirurgical Review, January 1828.

But not to occupy more than necessary the time of the reader by quoting further from Dr. Macculloch's Essay, I shall only observe that this author has found small streams bordered by thin and grassy margins; tranquil and stagnant waters, especially in hot countries; and ponds occupying but a small space,—to be productive of '*evening mists,*' the results of which are autumnal and intermittent fevers." And is not the *terrestrial radiation of caloric,* I would here ask, the cause of those evening mists which favour the attacks of these disorders? Indeed it is remarkable to find that every locality pointed out by the Doctor as productive of malaria, will be found to possess one or other of those circumstances which promote the dissipation of heat from the ground. It has long been known that water and a grassy surface are excellent radiators of caloric; and the effects of this process—fogs, damps and dew—were observed long before the cause of them was properly understood. "A valley," says Mr. Daniell*, "is more liable to the effects of radiation, than the tops or sides of hills; and it is a well-known fact that dew and hoar-frost are always more abundant in the former than in the latter situations. The influence of high hills is, however, often prejudicial to the valleys at their feet; for the condensed and moist air rolls down their sides, and lodges at the bottom: these, therefore, are protected from the chill, while a double portion falls upon what many are apt to consider the more sheltered situation. It is a very old remark, that the injurious effects of cold occur chiefly in hollow places, and that frosts are less severe upon hills than upon the neighbouring plains: and it is consistent with my own observations, that the leaves of the vine, the walnut-tree, and the succulent shoots of Dahlias and potatoes, are often destroyed by frosts in the sheltered valleys, on nights when they are perfectly untouched upon the surrounding eminences." The diminution of temperature which is produced upon the surface of radiating bodies during the night is communicated by slow degrees to the surrounding atmosphere; and if the process goes on for any considerable period, moisture and probably other matters are not only deposited upon them, but are precipitated in the air itself, affecting more or less the feelings of every one within its range, but particularly the weak or unhealthy.

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* Meteorological Essays and Observations.

[To be continued.]

XLIX. *An Abstract of the Characters of* Ochsenheimer's *Genera of the Lepidoptera of Europe; with a List of the Species of each Genus, and Reference to one or more of their respective Icones.* *By* J. G. CHILDREN, *F.R.S. L. & E. F.L.S. &c.*[*]

IN Samouelle's Entomologist's Useful Companion, as well as in several other works of deserved reputation, the names of the Genera established by Ochsenheimer, in his *Schmetterlinge von Europa*, are frequently quoted, but the characters on which they are founded wholly omitted, so that they can only be inferred from a laborious comparison of those of their respective types,—a task few persons will be disposed to submit to, in order to clear up an occasional doubt, as to what genus such or such an insect is to be referred. This inconvenience is attributable to the want of an English edition of Ochsenheimer's work; and in some measure to lessen it, the following translation of his Family and Generic Characters is offered to the British student.

Ochsenheimer died in 1822, leaving his work incomplete, only four volumes having been published in his life-time, the last of which appeared in 1816, and consists chiefly of an improved sketch of his arrangement of the Europæan Lepidoptera from the first genus to the eighty-seventh. Before his death, however, only the first forty-three genera were published in detail, with the characters and descriptions of their respective species; these occupy the first three volumes, the last of which terminates with the genus *Eyprepia;* for the fourth contains, besides the sketch of the arrangement, only notes concerning some of the species published in the former volumes. The work is continued by M. Frederick Treitschke, and the specific descriptions are completed to the hundred-and-sixth genus inclusive; and M. Treitschke has also given a further sketch of the arrangement, including ten additional genera consisting of the *Phalænæ Pyralides* of Linnæus, the specific descriptions of which are not yet published*. More therefore still remains to be done, and we wait anxiously for the completion of the work. In the mean time we lay the present abstract before the reader; and should he entertain any doubts of the value of M.M. Ochsenheimer's and Treitschke's labours, we refer him to the Introduction to Dr. Horsfield's Descriptive Catalogue of the Lepidopterous Insects contained in the Museum of the East India Company, where he will find such ample testimony to their merit as cannot fail (unless he disregard the maxim "*laudari à laudato*,") presently and effectually to remove them.

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[*] Communicated by the Author.

1*st Division.*—PAPILIONES.

Wings when at rest, erect.

Antennæ filiform, generally capitate, or terminated by a knob; sometimes only slightly incrassate at the end.

Flight, diurnal.

Larva with sixteen legs; *head* globular, perfectly distinct from the body; *motion* indolent, and sluggish.

Pupa angular.

Metamorphosis generally naked, or not concealed by a web.

Genus 1. MELITÆA, *Fab.*

Melitæa, *Fab.* Syst. Glossat.
Battus et Graphium, *Scopoli.* Introductio ad Hist. Nat.
Nymphalis, *Latr.* Gen. Crust. et Ins.
Papilio, *Schrank.* Faun. boïc.
Lemoniades, Hübn.

Legs, first pair imperfect.

Wings, roundish; *upper surface* of the *anterior* wings, reddish-yellow with black maculæ and dots, or blackish, with reddish-yellow maculæ and dots; *under surface* of the *posterior wings* with alternate orange-yellow, and yellowish-white cross bands with black spots; not silvered.

Antennæ, knob oval, compressed, obtuse.

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* The last volume as yet published is the sixth, of which, Parts I. and II. appeared in the present year.

Larva with seven or nine conical, fleshy protuberances, covered with short hairs, on each of the middle segments of the body, and two larger on the side of the throat.

Pupa, anteriorly rather obtuse, hinder part usually with elevated points; not suspended in any constant manner.

	Species.	Icon.
1.	M. *Maturna*, Linn...	Ernst, I. Pl. XVII. f. 27. a. b.
2.	— *Cynthia*, Fab......	Ernst, I. Pl. XVII. f. 26. a—d.
3.	— *Artemis*, Fab......	Ernst, I. Pl. XVII. f. 28. a. b.
4.	— *Cinxia*, Linn......	Ernst, I. Pl. XIX. f. 32. a—f.
5.	— *Didyma*, Esp. ...	Ernst, I. Pl. XVIII. f. 29. a—d.
6.	— *Trivia*, Hübn. ...	Ernst, I. Pl. LXI. Suppl. VII. f. 29. a—d. bis.
7.	— *Phœbe*, Hübn. ...	Ernst, I. Pl. LXI. Suppl. VII. f. 28. a. b. bis.
8.	— *Dictynna*, Esp....	Ernst, I. Pl. LXII. Suppl. VIII. f. 31. a—d. bis.
9.	— *Athalia*, Esp.......	Ernst, I. Pl. XIX. f. 31. c. d.
10.	— *Parthenia*, Borkh.	Hübn. Pap. Tab. 4. f. 19. 20. (fœm.)
11.	— *Lucina*, Linn......	Ernst, I. Pl. XVI. f. 25. a. b.

Genus 2. ARGYNNIS, *Fab.*

Argynnis, Fab.
Argyreus, Scop.
Nymphalis, Latr.
Papilio, Schrank.
Dryades, Hübn.

Legs, four perfect, gressorial.

Wings subdentate, *upper surface* generally reddish-brown with black spots; *under surface* with silvery bands or spots.

Antennæ capitate, knob compressed.

Larva with six longitudinal rows of ramose spines, and two others, generally larger than the rest on the first segment; a broad, longitudinal, dorsal band, divided by a medial line.

Pupa, suspended variously; cavity of the neck, and the neck, with brilliant points.

	Species.	Icon.
1.	A. *Aphirape*, Hübn.	Hübn. Pap. Tab. 5. f. 23. 24. (mas.) 25. (fœm).
2.	— *Selene*, Fab........	Ernst, I. Pl. XVI. f. 23. a. b.
3.	— *Euphrosyne*, Linn.	Ernst, I. Pl. XVI. f. 22. a. b.
4.	— *Dia*, Linn.	Ernst, I. Pl. XV. f. 21. a. b.

5. A. *Pales*,

	Species.	Icon.
5.	A. *Pales*, Hübn.	Ernst, I. Pl. LX. Suppl. VI. f. 21. a. b. c. d. bis, a. b. c. d. tert.
6.	— *Hecate*, Fab.	Ernst, I. Pl. LIX. Suppl. V. f. 20. a—d. tert.
7.	— *Ino*, Hübn.	Ernst, I. Pl. XV. f. 20. c.
8.	— *Daphne*, Fab.	Ernst, I. Pl. XV. f. 20. a. b.
9.	— *Frigga*, Hübn.	Hübn. Pap. Tab. 9. f. 49. 50. (fœm.)
10.	— *Thore*, Hübn.	Hübn. Pap. Tab. 3. f. 571—573.
11.	— *Amathusia*, Fab.	Ernst, I. Pl. LXXX. Suppl. II. Pl. I. f. 21. a. b. quart.
12.	— *Chariclea*, Schneid.	Herbst, Pap. Tab. 272. f. 5. 6.
13.	— *Freija*, Schneid.	Herbst, Pap. Tab. 272. f. 7—10.
14.	— *Latonia*, Linn.	Hübn. Pap. Tab. 11. f. 59. 60. (fœm.) Pl. CXX. f. 613. var.
15.	— *Niobe*, Linn.	Ernst, I. Pl. XV. f. 19. a. b. c.
16.	— *Adippe*, Fab.	Ernst, I. Pl. XIII. f. 16. c—i.
17.	— *Aglaia*, Linn.	Hübn. Pap. Tab. 13. f. 65. 66. (fœm.)
18.	— *Laodice*, Fab.	Hübn. Pap. Tab. 13. f. 67. 68. (fœm.)
19.	— *Paphia*, Linn.	Ernst, I. Pl. XII. f. 15. a—f.
20.	— *Pandora*, Fab.	Ernst, I. Pl. XII. f. 15. g. h.

Genus 3. EUPLOEA, *Fab.*

BATTUS, Scop.
DANAIS, Latr.
LIMNADES, Hübn.

Legs, four perfect.
Wings, anterior with the external margin rather curved; colour reddish-yellow, margin black with white spots; a curved band of white spots towards the apex.
Antennæ oval, knob gradually incrassate. Head and breast black, with white spots.
Larva, with sixteen feet; feet sub-spinous, spines simple.
Pupa nearly cylindrical; suspended freely.

	Species.	Icon.
1.	E. *Chrysippus*, Linn.	Hübn. Pap. Tab. 133. f. 678. 679. (mas.)

† The only European species of the genus.

Genus 4. VANESSA, *Fab.*

Cynthia, Fab. Papilio, Schrank.
Nymphalis, Latr. Hamadryades, Hübn.
Battus et Graphium, Scop.

Legs, four perfect, gressorial.

Wings, exterior margin angular; upper surface spotted; under side with transverse bands; bands usually blackish-brown or variegated.

Antennæ capitate.

Larva sub-lanate, with several longitudinal rows of stiff, bristly hairs, or spines. (The first segment naked, second and third with four, and the rest with six spines.) Head blackish, bifid; in some species armed with ramose subobtuse spines.

Pupa suspended vertically; often externally with a brilliant gold or silver hue: head and dorsal segments mucronate; dorsal ridge acuminate.

Species.	Icon.
A. Wings slightly dentate.	
1. V. *Cardui*, Lnin......	Ernst, I. Pl. VII. f. 7. a—g.
2. — *Atalanta*, Linn. ...	Ernst, I. Pl. VI. f. 6. a—i.
B. Wings furcate.	
3. V. *Io*, Linn..........	Ernst, I. Pl. II. f. 2. a—f.
4. — *Antiopa*, Linn.....	Ernst, I. Pl. I. f. 1. a—h.
5. — *V. album*, Fab. ...	Ernst, I. Pl. LVI. Suppl. II. f. 5. a—d. bis.
6. — *Polychloros*, Linn.	Ernst, I. Pl. III. f. 3. a—i.
7. — *Xanthomelas*, Illig.	Ernst, I. Pl. LV. Suppl. I. f. 3. a. b. bis.
8. — *Urticæ*, Linn......	Ernst, I. Pl. IV. f. 4. a—h.
9. — *Triangulum*, Fab.	Ernst, I. Pl. V. f. 5. g. h.
10. — *C. album*, Linn....	Ernst, I. Pl. V. f. 5. a—f.
— *F. album*, Fab. ... (var. *C. album*) ...	Herbst, Schm. Tab. 163. f. 1. 2.
C. Posterior wings slightly acuminate.	
11. V. *Prorsa*, Linn......	Ernst, I. Pl. VIII. f. 8. a—e.
12. — *Levana*, Linn.....	Ernst, I. Pl. VIII. f. 9. a—f.

Genus 5. LIMENITIS, *Fab.*

Neptis, Fab. Papilio, Schrank.
Nymphalis, Latr. Naiades, Hübn.
Battus et Graphium, Scop.

Legs, anterior pair very small; second and third perfect, gressorial.

Wings, dentated, the anterior somewhat repand *; upper surface, black, or blueish-green; under surface reddish-brown, or cinnamon colour; body griseous.

Antennæ clavate.

Larva variegated; head with two elevated points; body with two longitudinal rows of ramose spines.

Pupa variously suspended; head with two small elevations; the dorsal larger, securiform.

Species.	Icon.
1. L. *Aceris*, Fab.	Ernst, I. Pl. II. Suppl. III. f. 12. a—d. bis.
2. — *Lucilla*, Fab.......	Ernst, I. Pl. X. f. 12. a. b.
3. — *Sibylla*, Linn......	Ernst, I. Pl. XI. f. 13. a—f.
4. — *Camilla*, Fab......	Ernst, I. Pl. XI. f. 14. a. b.
5. — *Populi*, Linn......	Ernst, I. Pl. IX. f. 10. a—d.

Genus 6. CHARAXES, *Ochs.*

Paphia, Fab. Nymphalis, Latr.

Legs, first pair imperfect.

Wings, anterior elongated, angular; posterior dentate, the external margin bicaudate near the apex.

Antennæ clavate.

Larva smooth; head with four horns; body bicuspidate at the anal extremity.

Pupa, nearly oval.

Species.	Icon.
1. C. *Jasius*, Linn.......	Drury, Illustr. of Nat. Hist. I. Pl. I. f. 1.

† Only one European species.

Genus 7. APATURA, *Fab.*

Nymphalis, Latr. Maniola, Schrank.
Argus, Scop. Potamides, Hübn.

Legs, first pair imperfect.

Wings, somewhat repand, and dentate; colour changeable according to the direction of the light, between brown and purple; posterior wings ocellated at the interior angle.

Antennæ clavate, knob sub-cylindrical, and rather slender.

Larva similar to that of *Charaxes;* but with faint yellow transverse bands: head smooth anteriorly, with two long, straight, obtuse, or bipartite horns.

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* *Ausgeschweift*, repandus, *repand:* cut into very slight sinuations, so as to run in a serpentine direction.—*Kirby* and *Spence*, iv. 297.

Pupa green, compressed: head bicuspidate; suspended vertically by the posterior extremity.

Species.	Icon.
1. A. *Iris*, Linn.	Ernst, I. Pl. XXXI. f. 62. a. b.
2. — *Ilia*, Fab.	Ernst, I. Pl. XXXI. f. 62. c. d. (fœm.) Pl. XXXII. f. 64. c. f. (mas.)

Genus 8. HIPPARCHIA, *Fab.*

Nymphalis, Latr.	Maniola, Schrank.
Argus, Scop.	Oreades, Hübn.

Legs, first pair less than half the size of the second and third.
Wings, generally brownish, with the margin ocellated.
Antennæ clavate; knob flattened; (often slightly curved.)
Larva, anal extremity bicuspidate: head globular, anteriorly depressed; generally hairy, with dark-coloured longitudinal striæ; hairs whitish.
Pupa short, anteriorly bicuspidate, points erect, small; suspended by the anal extremity.
Metamorphosis, usually in the air, but some species change under ground.

Species.	Icon.
A. 1. H. *Proserpina*, Fab.	Ernst, I. Pl. XX. f. 33. a. b.
2. — *Hermione*, Linn.	Ernst, I. Pl. XX. f. 34. a. b. c.
3. — *Alcyone*, Linn.	Ernst, I. Pl. LXII. Suppl. VIII. f. 35. a. b. c.
4. — *Anthe*, Hübn.	Hübn. Pap. Tab. 115. f. 589. 590. (fœm.)
5. — *Briseis*, Linn..	Ernst, I. Pl. XXI. f. 36. a—d.
6. — *Semele*, Linn..	Ernst, I. Pl. XXII. f. 38. a. b. c.
7. — *Hippolytus*, Fab.	Ernst, I. Pl. VIII. Suppl. III. f. 36. a. b. bis.
8. — *Arethusa*, Fab.	Ernst, I. Pl. XXII. f. 39. a. b. c.
9. — *Fidia*, Linn...	Ernst, I. Pl. XXI. f. 37. c. d.
10. — *Allionia*, Fab.	Ernst, I. Pl. XXI. f. 37. a. b.
11. — *Statilinus*, Fab.	Ernst, I. Pl. LXIII. Suppl. IX. f. 37. a. b. c. bis.
12. — *Phædra*, Linn.	Ernst, I. Pl. XXIII. f. 40. a—e.
13. — *Bryce*, Fab. ...	Hübn. Pap. Tab. 33. f. 149. 150. (fœm.)
14. — *Cordula*, Fab.	Hübn. Pap. Tab. 29. f. 132. 133. (fœm.)
15. — *Actæa*, Hübn.	Ernst, I. Pl. LXIII. Suppl. IX. f. 37. g. h.

		Species.	Icon.
	16.	H. *Podarce*, Ochs.*	— — — —
	17.	— *Aëllo*, Hübn. .	Hübn. Pap. Tab. 102. f. 519. 520. (mas). Tab. 31. f. 141. 142. (fœm.)
	18.	— *Norna*, Thunb.	Hübn. Pap. Tab. 34. f. 152. 153. (mas.) Tab. 30. f. 142. (fœm.)
	19.	— *Tarpeia*, Fab.	Cram. Pap. Exot. Pl. CCCLXXV. E. F.
	20.	— *Bore*, Fab.....	Hübn. Pap. Tab. 29. f. 134. (mas.) 135. 136. (fœm.)
B.	21.	— *Tithonus*, Linn.	Ernst, I. Pl. XXVII. f. 53. a—c.
	22.	— *Ida*, Fab.......	Ernst, I. Pl. V. Suppl. III. f. 53. h.
	23.	— *Pasiphäe*, Fab.	Ernst, I. Pl. LXVI. Suppl. XII. f. 53. a. b. bis.
	24.	— *Clymene*, Fab.	Ernst, I. Pl. V. Suppl. III. f. 50. a. b. tert.
	25.	— *Roxelana*, Fab.	Cram. Pap. Exot. Pl. CLXI. fig. C. D. E. F.
	26.	— *Janira*, Linn..	Ernst, I. Pl. XXVIII. f. 54. a—h.
	27.	— *Eudora*, Fab..	Ernst, I. Pl. XXVIII. f. 55. a. b.
C.	28.	— *Hyperanthus*, Linn.	Ernst, I. Pl. XXVII. f. 52. a—f.
	29.	— *Dejanira*, Linn.	Ernst, I. Pl. XXV. f. 48. a. b.
	30.	— *Hiera*, Hübn.	Hübn. Pap. Tab. 39. f. 176. (fœm).
	31.	— *Mœra*, Linn. .	Ernst, I. Pl. XXVI. f. 51. a. b.
	32.	— *Adrasta*, Hoffmansegg......	Ernst, I. Pl. LXXXII. Suppl. II. Pl. 3. fig. 50. a. b. c. bis.
	33.	— *Megæra*, Linn.	Ernst, I. Pl. XXVI. f. 50. a. b. c. d. (e. f. var.)
	34.	— *Egeria*, Linn.	Ernst, I. Pl. XXV. f. 49. a—d.
	35.	— *Meone*, Hübn.	Cram. Pap. Exot. Pl. CCCXIV. f. E. F.
D.	36.	— *Galatea*, Linn.	Ernst, I. Pl. XXX. f. 60. a—d.
	37.	— *Lachesis*, Hübn.	Hübn. Pap. Tab. 41. f. 186. 187. (mas.) Tab. 42. f. 188. 189. (fœm.)
	38.	— *Clotho*, Fab....	Ernst, I. Pl. V. Suppl. III. f. 61. a. b. bis.
	39.	— *Ines*, Hoffm...	— — — —
	40.	— *Arge*, Sulzer. .	Ernst, I. Pl. XXX. f. 61. a. b.
	41.	— *Syllius*, Herbst.	Ernst, I. Pl. XXX. f. e. f.

* Sp. n.—H. alis subdentatis fuscis: anticis utrinque ocello, punctisque subtus duobus albis: posticis supra immaculatis, subtus albo fuscoque marmoratis, fasciâ crenatâ concolore albo marginatâ venisque albis.

			Species.	Icon.
E.	42.	H.	*Epiphron*, Fab.	Hübn. Pap. Tab. 44. f. 202.
	43.	—	*Pharte*, Hübn.	Hübn. Pap. Tab. 97. f. 491. 492. (mas.) 493. 494. (fœm.)
	44.	—	*Melampus*, Esp.	Ernst, I. Pl. LXXXI. Suppl. II. Pl. 2. f. 41. a. b. bis.
	45.	—	*Cassiope*, Fab.	Ernst, I. Pl. XXIV. f. 45. a. b.
	46.	—	*Arete*, Fab. ...	Hübn. Pap. Tab. 50. f. 231. 232. (fœm.)
	47.	—	*Mnestra*, Hübn.	Hübn. Pap. Tab. 106. f. 540. 541. (mas.) 542. 543. (fœm.)
	48.		*Pyrrha*, Fab. .	Ernst, I. Pl. XXIII. f. 41. a—d.
	49.		*Oeme*, Hübn. .	Hübn. Pap. Tab. 104. f. 530. 531. (mas.) 532. 533. (fœm.)
	50.	—	*Psodea*, Hübn.	Hübn. Pap. Tab. 98. f. 497. (mas.) 498. 499. (fœm.)
	51.	—	*Afer*, Fab.	Hübn. Pap. Tab. 98. f. 500. 501. (mas.)
			Ceto, Hübn....	Hübn. Pap. Tab. 112. f. 578. 579. (mas.)
	53.	—	*Medusa*, Fab.	Ernst, I. Pl. XXIV. f. 44. a. b.
F.	54.	—	*Stygne*, Hübn.	Hübn. Pap. Tab. 48. f. 223. 224. (mas.)
	55.	—	*Melas*, Herbst.	Hübn. Pap. Tab. 45. f. 105. 106. (mas.)
	56.	—	*Alecto*, Hübn.	Hübn. Pap. Tab. 104. f. 528. 529. (mas.) Tab. 101. f. 515. 516. (fœm.)
	57.	—	*Medea*, Fab. .	Ernst, I. Pl. XXIV. f. 43. a. b. e. f. g.
	58.	—	*Ligea*, Linn. .	Ernst, I. Pl. XXIII. f. 42. a. b.
	59.	—	*Euryale*, Esp.	Esp. Schm. I. Th. Tab. 118. Cont. 73. f. 2. (mas.) f. 3. (fœm.)
	60.	—	*Embla*, Fab. . .	Hübn. Pap. Tab. 109. f. 561. 562. (mas.) Tab. 49. f. 228. 229. (fœm.)
	61.	—	*Pronöe*, Fab. .	Ernst, I. Pl. LXIV. Suppl. X. f. 42. a—e. bis.
	62.	—	*Goante*, Esp. .	Hübn. Pap. Tab. 50. f. 233, 234. (fœm.)
	63.	—	*Gorge*, Hübn.	Hübn. Pap. Tab. 99. f. 502. 503. (mas.) 504. 505. (fœm.)
	64.	—	*Manto*, Fab....	Ernst, I. Pl. LXV. Suppl. XI. f. 42. a. b. tert.
	65.	—	*Tyndarus*, Fab.	Ernst, I. Pl. LXV. Suppl. XI. f. 42. a. b. quart.

	Species.	Icon.
G. 66.	H. *Davus*, Linn.	Ernst, I. Pl. XXIX. f. 58. a. b.
67.	— *Pamphilus*, Linn.	Ernst, I. Pl. XXIX. f. 56. a. b.
68.	— *Lyllus*, Esp.	Hübn. Pap. Tab. 109. f. 557. 558. (fœm.)
69.	— *Iphis*, Fab.	Hübn. Pap. Tab. 53. f. 249. (mas.) 250. 251. (fœm.)
70.	— *Hero*, Linn.	Ernst, I. Pl. XXIX. f. 59. a. b.
71.	— *Œdipus*, Fab.	Hübn. Pap. Tab. 52. f. 245. 246. (mas.)
72.	— *Arcania*, Linn.	Ernst, I. Pl. XXIX. f. 57. a—d.
73.	— *Dorus*, Esp.	Ernst, I. Pl. LXVIII. Suppl. XIV. f. 57. a. b. bis.
74.	— *Satyrion*, Esp.	Hübn. Pap. Tab. 53. f. 254. 255. (mas.)
75.	— *Corinna*, Hübn.	Hübn. Pap. Tab. 105. f. 534. 537. (fœm.)
76.	— *Leander*, Fab.	Hübn. Pap. Tab. 103. f. 526. 527. (fœm.)
77.	— *Phryne*, Fab.	Ernst, I. Pl. VIII. Suppl. III. f. 58. a. b. bis.

[To be continued.]

L. *On the Crystalline Forms and Composition of the Sulphates of Nickel.* By R. Phillips, *F.R.S. &c.*

THE *Annales de Chimie et de Physique* for May last, contains a memoir by Mons. E. Mitscherlich, "On the crystalline forms and composition of some sulphates": his statements, with respect to sulphate of nickel, appear to require some notice.

In the present paper the author has given only one of the forms of sulphate of nickel, stating that in a memoir which will shortly appear he shall describe another. The primary form of the crystal now under examination, Mons. M. considers as an acute octahedron with a square base; but it may be regarded, as shown by Mr. Brooke in the Annals of Philosophy, vol. 6. N.S., p. 437, as a square prism, parallel to the planes of which it may be cleaved. The composition of sulphate of nickel M. Mitscherlich states to be:

Sulphuric acid	28·51
Oxide of nickel	26·71
Water	44·78
	100·00

M. Mitscherlich then makes the following statement: "In another

other memoir which will soon appear, I shall describe another crystalline form of sulphate of nickel and of sulphate of zinc, which is entirely different from that which I have now described; the production of these different forms depends upon the temperature at which the crystals are formed. The seleniate of zinc, which at a temperature of 50° Fahr. gives prismatic crystals, changes its form when the prismatic crystals are exposed on paper to the heat of the sun. This phænomenon may be also extremely well observed in sulphate of nickel. A temperature of 59° of Fahr., still produces prismatic crystals. If these crystals, of a certain size, be exposed to the sun in a close vessel, it frequently happens that they retain their external form, so that the angles at which the planes meet may be measured; but if they be broken, they are found to consist of crystals frequently several lines in length, which are octahedrons with a square base, the angles of which I have been able to measure. This change requires two or three days.

"I have determined by a very complete analysis the quantity of water contained in this compound. The octahedrons with a square base, into which the prismatic crystals were converted, by several days' exposure to the sun in an uncovered [covered?] vessel, gave me 30·14 per cent. of sulphuric acid; some other octahedrons with a square base derived from the crystallization of a hot solution yielded 29·88. If we take the mean of these two results, we must admit that the octahedron of sulphate of nickel with a square base contains:

Sulphuric acid	30·02
Oxide of nickel	28·13
Water	41·85
	100·00

"It follows from this phænomenon," concludes M. Mitscherlich, "as well as from several others which I have before announced, that the isolated particles of matter in solid bodies are moveable with respect to each other, and that they may assume different relative positions to those which they originally had, without its being necessary to render the bodies fluid.

Now without asserting it to be the case, I do most certainly think that Mons. M. has attributed the difference in the form of these crystals to a wrong cause; and at any rate I am quite sure that the crystals of sulphate of nickel of both kinds may be procured at pleasure, and totally independently of the temperature at which the crystallization occurs.

I have already stated that M. Mitscherlich considers as an octahedron that which Mr. Brooke regards as a square prism;

either of which forms, from the relation which they bear to each other, may be assumed as the fundamental crystal: while he has not given at all the precise form of the prismatic variety, and which Mr. Brooke in a paper already quoted, has described as a rhombic prism so nearly approaching that of sulphate of zinc, that he is inclined to doubt whether there is any real difference between them.

In this memoir the difference between the crystalline forms is clearly traced by Mr. Brooke; and I have endeavoured to prove that the difference is dependent not upon the proportion of water, as Mons. M. appears to suppose, but owing to one of the crystals containing more sulphuric acid than the other, although not in atomic proportion.

The analyses which I have given in the Annals of Philosophy, N.S. vol. vi. p. 439, show that the quantity of acid in 100 of the square, is to that in the rhombic prisms as 30 to 28·16,—a difference of nearly 2 per cent.; and the quantities of water are respectively 45·54 and 43·8, a difference of almost 1¾ per cent. instead of nearly 3, as stated by M. Mitscherlich.

That excess of acid without any variation of temperature is capable of producing variation of form, is proved by the following experiment: I dissolved 200 grs. of rhombic prisms in water, and added to the solution about half its weight of sulphuric acid, and put the solution to crystallize in a room, the temperature of which varied from about 60° to 64°. The crystals first obtained were similar to those dissolved, viz. rhombic prisms; afterwards I procured a mixture of rhombic and square prisms; and lastly, square prisms only, and this without any greater variation in the temperature than that which I have already noticed.

From this experiment it is evident, that owing to the formation of rhombic crystals in the first instance without excess of acid, the relative proportion of sulphuric acid to the oxide of nickel was subsequently so much increased, that square prisms were formed, which from the analysis already stated contain a larger proportion of acid.

In corroboration of the inference that the difference of form is dependent upon that of the quantity of acid, I shall merely add that a solution of 200 grs. of rhombic prisms, to which no sulphuric acid was added, and crystallized in the same room already mentioned, yielded merely rhombic without any admixture of the square prisms.

To the foregoing statements I may add, that rhombic crystals of sulphate of nickel, when exposed to the air, effloresce, which is not the case with the octahedral variety; and it appears to me probable, that when rhombic prisms which have

been

been deposited from a solution containing excess of acid, are exposed to the sun, the supposed formation of octahedral crystals is merely a removal of the enveloping rhombic crystals by efflorescence, and the consequent development of the inclosed octahedral crystals; for rhombic crystals formed in the requisite mode frequently contain minute octahedrons, which may be observed by merely breaking the crystal, which will explain their occurrence, even without any external change in the enveloping rhombic prism.

LI. *Chemical Examination of some of the Substances connected with an Egyptian Mummy. By* E. S. George, *F.L.S. Secretary to the Leeds Philosophical and Literary Society**.

I.—A PORTION of the pounded wood found about the throat and breast, was digested in boiling alcohol; a deep brown solution was thus obtained, which, after being filtered, remained permanently transparent. The odour of myrrh was very sensible, and the alcoholic extract afforded with water the characteristic precipitate of solutions of myrrh. By a careful examination of the wood, it was separated into two parts; the one, and that the most abundant, was myrrh, and the other cassia. The odoriferous wood from the abdominal cavity, subjected to the same treatment, gave similar results.

II.—The folds of cloth with which the mummy was bandaged, presented, near the body, a much deeper colour than the external wrappings. A portion having a deep chesnut colour was digested in boiling water ten minutes; a very deep brown-coloured solution was thus obtained. The addition of a few drops of gelatine to this solution, gave an immediate precipitate, indicative of the presence of tannin: this result was rendered more striking by concentrating the solution, when large flakes of the tannate of gelatine were precipitated. A few drops of a solution of muriate of barytes were added to this aqueous extract: an immediate precipitate fell down; it was found to consist principally, of carbonate of barytes, and by the requisite tests, the presence of the carbonate, muriate, and sulphate of soda was discovered, the former salt in the largest proportion.

III.—Analysis of the fleshy parts of the body.

1.—A piece of thick abdominal muscle, weighing 97 grains, was macerated one hour in water, at about 170°; the solution had a light yellow colour and a saline taste.

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* From Mr. Osburn's "Account of an Egyptian Mummy presented to the Museum of the Leeds Philosophical and Literary Society, by the late John Blayds, Esq."—A further notice of this work will appear in our next Number.

2.—The portion remaining was repeatedly digested in boiling alcohol (·835 rectified spirit of wine), until the spirit ceased to acquire any colour: the alcoholic solutions deposited a yellow-coloured substance upon cooling. The whole of the solutions were mixed together and diluted by about twice their amount of water; an immediate yellow-coloured precipitate fell down: on the application of heat the precipitate melted, and floated upon the surface of the water; it was ascertained that this substance became solid at 110° Fahrenheit: it was thus removed, and after being dried between the folds of filtering paper, weighed 23 grains.

3.—The remaining liquid, when cool, was opaque, but upon being heated to 212° became transparent. On evaporation to dryness, it weighed 12 grains, and proved to be almost entirely gelatine.

4.—The aqueous solution, (No. 1.) was evaporated to dryness; the residue, which was of a dark-brown colour, brittle, and covered with bright saline crystals, weighed 9 grains. The addition of a few drops of water converted the whole of it into a mucilage; the saline part consisted almost entirely of carbonate of soda, with some muriate and sulphate, and appeared identical with the salt found in the bandages. The mucilaginous portion was gelatine.

5.—The part undissolved by the action of both water and alcohol, weighed, when dry, 51 grains; it did not inflame readily, and gave out when burning the peculiar smell of burnt horn.

6.—The substance (2) had a deep yellow colour, and a greasy feel very much like that of cerate; it possessed little either of taste or smell when cold: when fused, it gave out the odour of the spices found about the body; it inflamed, and emitted a large quantity of light during its combustion. It was entirely dissolved in liquid ammonia, and the solution remained perfectly transparent on the addition of water; this alkaline solution exposed in an evaporating-dish to the air, deposited a soapy substance as the ammonia evaporated. Potash formed with this substance a soap soluble in water. Boiling nitric acid scarcely acted upon it. Subjected to destructive distillation, it presented the following appearances. Upon the application of heat it melted, and bubbles of air were rapidly disengaged; —after a short time, the liquid became quiescent, the retort was filled with dense white fumes, and a few drops of water were condensed. No trace of the formation of ammonia was perceptible, nor did the water taste acid; at a more elevated temperature a dark-coloured oily fluid trickled down the beak of the retort, and was condensed into a fatty substance, which increased and became more solid as the distillation advanced; at the same time a pungent and disagreeable vapour passed over, having very much the odour of candle-snuff. At the close of the experiment, a bright charcoal remained in the retort.

7.—A portion of muscle was digested in boiling spirit of turpentine: the solution, which was deep coloured, being evaporated to dryness, left a substance similar to that separated by alcohol.

8.—In order to determine whether the substance separated by alcohol and essential oil of turpentine was formed by their action upon the animal matter, a portion of muscle was digested one hour in boiling water; the surface of the water was covered by an oily substance, which, on cooling, became solid, and resembled in all its properties that separated 1. 2. and 7. The aqueous solution contained a considerable quantity of gelatine.

9.—A piece of thick muscle covered with skin was digested four days in cold alcohol (sp. gr. 835), the solution acquired a dark-brown colour; by spontaneous evaporation, a white substance in plates was deposited. The solution, when reduced to one-fourth of its original bulk, was filtered, and the solid part dried upon the filter: it had precisely the same properties as the substance obtained by the action of boiling alcohol, except being of a much lighter colour. Upon evaporating the solution which passed through the filter to dryness, a very small quantity of a body heavier than water, insoluble in that liquid, and which possessed all the characters of resin, remained.

IV.—A small fragment of the visceral substance, supposed to be the liver, was examined. It was covered with a thin coating of saline efflorescence, mixed with earth. The salts proved to be the same as those before examined,—the carbonate, muriate, and sulphate of soda; tests were carefully applied to detect, if present, the nitrate of potash, but without discovering any trace of that salt. The earthy substance effervesced with acids.

The liver was next repeatedly digested in alcohol and water. Gelatine was the only substance separated by these solvents—the aqueous solution contained a large quantity. I found in the course of this set of experiments, that although gelatine is insoluble in pure alcohol, yet the rectified spirit of wine (sp. gr. 835) dissolves it.

V.—The drops of a resinous substance from the cavity of the head, were found to be pure resin, having a very fine odour, which was not acertained to resemble that of any known resin.

In many of the substances discovered by this analysis, the characters are so unequivocal as to render their identification easy and certain;—such are the salts, the tannin, and colouring matter of the bandages; the gelatine obtained from both the muscular part and the viscera; the resins, and the pounded spices from the body. There is, however, some difficulty in arriving at a conclusion with the remaining, and indeed most important substance; for although its appearance, and many of its chemical properties closely resemble those of wax, some others approach very nearly to the properties of animal substances, as adipocire.

Like wax, this substance is soluble in alcohol, but differs in degree; cold alcohol, which scarcely acts upon wax, dissolving it readily. With wax, the alkalies form soaps almost insoluble in water: with this substance, the alkaline soaps are very soluble.—Nitric acid scarcely acts upon wax; boiling nitric acid exerts a very slight action upon this substance, for the loss of colour depends upon the removal of a small quantity of resin, which it was shown (9.) that the alcoholic solution from the muscular part contains*.

The results of the destructive distillation of both very closely agree. Comparative experiments with equal weights of wax were made; the only difference noticed was, that in the distillation of the wax the product was more acid and empyreumatic, and that the quantity of permanent gases liberated was larger. With adipocire this substance agrees in its solubility in cold alcohol, in forming alkaline soaps soluble in water, in its point of fusibility, which is lower than that of wax, and in the action of acids.

Whether this substance be an adipocirous body formed by the process of embalming, or wax introduced during that process into every part of the deepest muscle, I shall not determine. In the appearance of the mummy there is much to favour the former opinion; the bones of the most exposed parts, as the head, are not in the slightest degree penetrated by this waxy substance, in a fused solution of which, we must suppose the body to have been many days immersed; nor is the wax found in greater abundance upon and near the surface, than in the most deeply seated parts; the cuticle covers every part of the body, which scarcely would have been the case if exposed so long to an elevated temperature.

I am aware that Dr. Granville has, from a very elaborate and interesting examination of a mummy, concluded that wax was employed in the process. This mummy, in some respects, differs from his, in the perfect state of the viscera, and in the total absence of bitumen, or of any but the most expensive woods and resins.

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* The oil of cedar is one of the ingredients in the process of embalming. The resinous appearance may, probably, have arisen from the use of this condiment.—*Note by Mr. Osburn.*

LII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

June 20.—JOHN, Earl of Shrewsbury, of Great Stanhope Street, May Fair, and of Alton Abbey, Staffordshire; Robert Allan, Esq. of Charlotte Square, Edinburgh; W. S. Henwood, Esq. of Perran Wharf, Truro, Cornwall; and the Rev. John Ward, Vicar of Great Bedwin, Wilts,— were elected Fellows of this Society.

A paper was read "On the Geology of Bundelcund, Boghelcund, and the districts of Saugor and Jabalpoor in central India." By Captain James Franklin, of the Bengal Army, F.R.S. F.A.S.

The tract of country described by the author is a portion of the lowest northern steps of the Vindáya mountains, situated between the latitudes 22° 40″, and 25° 20″ N., and the longitudes 78° 30″, and 83° E.; having on its north-eastern extremity the towns of Mirzapoor and Allahabad, and near its southern limit, those of Tendukaira, Singpoor and Mundla.

In this extent of country the principal situations examined by Captain Franklin were, the pass of Tara in the first range of hills; the pass of Kattra in the second range; the cataracts of Billohi, Bauti, Kenti, Chachye, and of the Tonse river; the neighbourhood of the villages of Simmereah, Hathee, Birsingpur, Sohawel, Nagound, and Lohargaon; the bed of the Cané river near Tigra; the neighbourhood of Hatta, Narsing-hagarh, Patteriya, Saugor, Tendukaira; the valley of the Nermada river; Garha-kota, Great Deori; the Bandair and Kymur hills; Jabalpoor, and the waterfall of Beragurh.

The succession of formations observed by the author consisted, in a descending order:—1. Of diluvial deposits.—2. Of overlying rocks of the trap formation.—3. Of a compact limestone.—4. Of red-sandstone.—And, lastly, 5. Of primitive rocks, including granite, gneiss, &c. The paper is illustrated by a geological map and section of the country; and the author particularly wishes to direct the attention of geologists to the limestone of the second range of hills, which he is of opinion corresponds with the lias-limestone of England, a formation which has not hitherto been shown to exist in India.

Having commenced his route at Mirzapoor on the Ganges,—in a district covered with alluvium reposing in some places on beds of "Canker," in others on sandstone, the author ascended the first range of hills at the pass of Tara. These hills are composed of fine-grained sandstone horizontally stratified, and more or less coloured by red oxide of iron; the rock appears to be saliferous, and is in many places quarried for architectural purposes; and it seems to correspond with the central portion of the new-red-sandstone of England.

At the pass of Kattra, near the summit of the second range of hills, a friable variegated sandstone appears, in which thin laminæ of sandstone alternate with red clay, resembling the red marle of England, both reposing on slaty marle coloured by chlorite, which rests, apparently, on massive horizontal strata, resembling clay-slate or grauwacke.

At the bottom of the cataract of Billohi, 398 feet in height, argillaceous sandstone was found, tinged deeply by red oxide of iron, and containing disseminated mica,—on which reposed a siliceous sandstone of a more compact texture.

Greenish white arenaceous sandstone not quite so compact was found at the cataract of Bauti, 420 feet below the summit, varying in colour as it ascended: and twenty-four miles further westward, at the cataract of Kenti, and at a depth of 272 feet, as well as at the cataracts of Chachye and of the Tonse river, sandstone of the same general character was observed rising to the surface.

The sandstone of Simmereah is sometimes ferruginous, at others slaty, and interspersed with mica; in the neighbourhood of Hathee it is succeeded by what the author considers as the equivalent of the lias-limestone.

At Birsingpur, in the bed of a small river, is a stratum of red marle or sandstone, containing laminæ of calc-spar; at Sohawel the red marle underlies the limestone above mentioned; and at Nagound in the bed of the Omeron river, the lower and central beds of limestone are exposed to view, containing fragments of fossil wood, stems of ferns,—and, as the author states, the gryphite which is characteristic of this formation in Europe.

This limestone appears also at Hatta and Narsinghagarh reposing on red marle, and in the latter situation is tinged green by chlorite. At Patteriya, where the limestone comes into contact with trap, the strata assume in some places the form of chert.

The aspect of this limestone is dull and earthy; its stratification horizontal or nearly so, and always conformable to the red marle on which it reposes.

Between the pass of Patteriya and Saugor, the author met with no other rock than trap, generally in the form of boulders imbedded in friable wacken, and composed of concentric layers: beneath the boulders is a bed of indurated wacken and basalt; and under the latter a stratum of impure limestone, in some parts containing a large proportion of alumine; below the limestone is a stratum of amygdaloid, containing calc-spar and a few zeolites, which at Saugor reposes on sandstone.

The trap of Saugor continues without interruption to Tendukaira: it contains abundance of chalcedony, semiopal, mealy zeolite, cachalong, agates, jaspars and heliotrope.

At about the distance of three miles from the foot of the hills near Tendukaira, in the valley of the Nermada river, the older rocks are exposed to view, in strata which are highly inclined,—in some instances nearly vertical, and in all cases unconformable to those already noticed.

On his route from Tendukaira to Garha-Kota, captain Franklin was enabled to ascertain the eastern boundary of the trap formation, which is throughout intimately associated with earthy limestone; the whole series reposing on red marle and sandstone.

Trap in horizontal strata was also observed for an extent of three miles near Great Deori, previous to the appearance of the sandstone of the Bandair hills, which last-mentioned rock the author is of opinion corresponds with the new-red-sandstone of England; the same chain of hills is composed of sandstone opposite Nagound, Lohargaon, Tigra, and Gurreha. The Kymur range in some parts appears to be composed of quartz-rock, varying to siliceous grit, in strata nearly vertical; but to the S.W. near Hirapur, the rock becomes more compact; and still further west, opposite Googni, it is intermixed with clay-slate and schistose limestone.

A broad valley covered with diluvium, intervenes between the Kymur range of hills and Jabalpoor; and near that town another range is situated, composed of granite containing flesh-coloured felspar, smoky quartz, black mica and hornblende;—and in which, also almost every rock commonly associated with granite is to be found.

Snow-white dolomite, traversed occasionally by chlorite schist, is to be seen near the waterfall of Beragurh, intimately associated with quartz; it is here capable of taking a fine polish, and scarcely effervesces with acids; but a few miles further west, near Bograi, it is exceedingly friable, and effervesces freely: it moreover contains crystals of Tremolite.

Captain Franklin observes that a part of the southern barrier of the valley of the Nermada river, like the northern barrier opposite Tendukaira, is composed of trap-rocks, the contour of which, to the extent of 80 miles, he has laid down on his map. The eastern deposit of overlying rocks extends southwards as far as Chuparah, and thence eastward towards Mandela, Omercuntuc, and Sohagpoor; but whether it is united with the great central mass, he was unable to ascertain.

The paper concludes with some inferences from the observations; and after stating the opinion of the late Dr. Voysey, that "the basis of the whole peninsula of India is granite," (Asiatic Researches, Vol. XV. page 123.) the author observes,—1. That although granite is very near the surface in many parts of the tract which fell under his examination, yet there is here, as in other countries, a series of primary stratified rocks intervening between the granite and secondary formations; which series however, there is reason to conclude, is thin and often wanting.

2. The sandstone formation has a visible thickness of 420 feet at the cataract of Bouti, and is considerably thicker no doubt near Chachye and the Bandair hills, &c. The limestone formation on the contrary, which in other countries sometimes forms mountain tracts, and occupies extensive portions of the earth's surface, is in India a mere plastering, as it were, over the red marle or sandstone; and Captain

Franklin doubts whether it ever attains a thickness of 100 feet; 50 feet being perhaps a fair average. He never met with it in any other situation than on the summit of the second range of hills.

3. The overlying trap-rocks are not only the most extensive, but, considering them in a geological view, the most important formation in this part of India. The thickness of this formation is variable: it reposes on every rock indiscriminately, from granite upwards; and at Saugor it may be seen on sandstone, where its inferior boundary is about 1350 feet above the sea. In the centre of India it occupies the summits of the highest mountains; and at Bombay it descends to the level of the sea.

There are two kinds of basaltic rock in the district of Jabalpoor, clearly of distinct formations; the older variety penetrates the grauwacke stratum, in the bed of Nermada river, near Lamaita; the younger is an overlying rock like that at Saugor,—but reposing on granite, and containing a greater proportion of augite and olivine.

Captain Franklin also describes a calcareous conglomerate, found in the beds of most of the rivers whose sources or channels are in the trap, and of sufficient cohesion for architectural purposes: its stratification is always horizontal, and in point of age he thinks it must be classed with the tufas and concretionary formations so prevalent in India.

An appendix to this paper contains the results of barometrical and thermometrical observations made between Nov. 1826, and Feb. 1827, on the route from Mirzapoor to Saugor, and thence to Tendukaira and Jabalpoor; with the heights of fifty-four places above the sea, and the latitudes and longitudes of the respective stations.

An extract was read of a letter from Samuel Hobson, Esq. to Dr. Roget, F.G.S. Sec. R.S. &c. (dated at New Orleans, 6th April, 1827,) and enclosing an account of some gigantic bones,—by Samuel W. Logan, M.D.

The place where these bones had been found is not mentioned; but at the date of the letter, they were exhibited publicly at New Orleans. Dr. Logan describes them as consisting of one of the bones of the cranium, fifteen or twenty vertebræ, two entire ribs and a part of a third, one thigh-bone, two bones of the leg, and several large masses of a cancellated structure.

The cranial bone was twenty feet and some inches in its greatest length, about four feet in extreme width (for the bone tapers to a point), and it weighed twelve hundred pounds. Dr. Logan inclines to think that this is the temporal bone.

The vertebræ, consisting of a body, oblique transverse, and spinous processes, gave sixteen inches as the mean diameter, and twelve inches as the depth of the bodies; while the passage for the spinal cord measured nine inches by six. The spinous processes stand off backwards and downwards, fourteen inches in the dorsal, and somewhat less in the lumbar vertebræ, three of which latter are entire.

The ribs, well formed and in a perfect state of preservation, measured nine feet along the curve, and about three inches in thickness.

The thigh-bone, measured in length, gave only one foot six inches, but is very thick. The bones of the leg are of similar dimensions, but perhaps a little more slender.

It had been conjectured that the animal to which these remains belonged, was amphibious, and perhaps of the crocodile family; and the conjecture appeared to Dr. Logan to be justified by the great length and flatness of the head (judging from the single specimen of the cranial bone), and the shortness of the limbs. It was also supposed that the animal, when alive, must have measured five and twenty feet around the body, and about one hundred and thirty feet in length.

An Extract was read of a letter frem his Grace the Duke of Buckingham, to Professor Buckland, V.P.G.S. dated at Naples, 3rd April, 1828, giving an account of certain phænomena, which attended the late eruption of Vesuvius. The author states that the Solfaterra was in no degree affected by the eruption.

A Letter was read from Charles Stokes, Esq. F.G.S. F.R.S. to W. J. Broderip, Esq. Sec. G.S. explanatory of three drawings of Echini, representing,—1. A specimen of *Galeorites albo-galerus* (Lam.), from the chalk, in which the plates of the mouth, consisting of five pairs, are preserved *in situ*;—2. A *Cidaris,* also from the chalk, in which portions of the plates of the mouth and the teeth are visible: they are displaced, but exhibit a system quite analogous to that of the recent cidaris;—and, 3. A *Cidaris* from Stonesfield, in which the anal plates are in the best preservation.

At the close of this Meeting, which terminated the Session, the Society adjourned till Friday Evening, the 7th of November; when they will meet at their Apartments in Somerset House.

ASTRONOMICAL SOCIETY.

The reading of Mr. South's paper "On the Occultation of *δ Piscium* by the Moon, &c." commenced in April, was resumed and concluded.

Of all the phænomena which occupy the attention of the practical Astronomer, the Author of this paper considers, that no one admits of such accurate observation, as does the occultation of a fixed star by the moon: occasionally, however, a circumstance presents itself to his notice, which merits peculiar consideration, namely an apparent projection of the star, upon the lunar disk; the instances in which this anomaly has been observed in this country, are indeed rare; and has, he says, led many to consider it the attendant of a lively imagination. If, however, we dispassionately review the observations of Continental Astronomers, of unsullied reputation, there will, Mr. South says, be little reason to doubt the fact of apparent projection, although perhaps there may be considerable difficulty in arriving at its cause.

The only instance in which Mr. South has witnessed the phænomenon, was previous to the occultation of *δ Piscium,* on the 6th of February 1821. (Latitude of the Observatory 51° 30′ 2″,97 North. Longitude of the Observatory $21^{s},76$ West.) The night was beautifully fine, the moon's dark limb and unilluminated disk unusually distinct: the atmosphere peculiarly serene, and the moon's limb, as well as the star, remarkably steady. The observation was made with his five-feet equatorial, furnished with a power of 127; the observer was at the telescope 4 or 5 minutes before the immersion could happen: every thing went on as usual, till the moon's limb came in contact with the star; but the expected occultation did not occur. He noted the time when the apparent contact took place, which was at $3^h\ 20^m\ 54^s,0$ by the clock. The star, unshorn of any of its splendour, remained visible on the unillumined lunar disk, till $3^h\ 21^m\ 2^s,9$ by the clock, when it instantaneously disappeared. Not the slightest sensible deviation in the star's place occurred between the moments of apparent contact and subsequent disappearance; and it exhibited the same perfectly defined disk whilst on the moon's limb, as it was observed to have, previously to the contact.

The corrections for the clock's error being applied, the observations will stand thus:

	h	m	s
Apparent contact at	3	20	29,87
Instantaneous immersion ..	3	20	38,77
Emersion	4	14	32,88

The only corresponding observations of this occultation which have come to Mr. South's knowledge, were made by Mr. Littrow and Mr. Baily, and their results are given in the Memoirs of this Society. As Mr. Littrow has not narrated any peculiarity, it is probable none presented itself to him; and Mr. Baily has authorized Mr. South to state that he saw nothing anomalous; a circumstance certainly important, seeing that in Blackman Street the apparent projection of the star on the moon's disk, continued nearly nine seconds of time. On the same evening in Blackman Street, a star of the 8th or 9th magnitude suffered occultation by the moon's dark limb, nearly at the same part, at which *δ Piscium* entered on the disk; the star disappeared instantaneously at $5^h\ 2^m\ 6^s,0$ by Mr. South's clock; prior to occultation, however, this star was not seen projected upon the limb; but the low altitude of the moon rendered the observation less satisfactory than was the previous occultation of *δ Piscium.*

The recorded observations of other Astronomers are then quoted in the words of their respective authors, or in authentic abstracts; and are principally derived from the *Mémoires de l'Academie Royale des Sciences,* the *Histoire et Mémoires de l'Academie Royale des Sciences de Toulouse,* the *Histoire Céleste Française,* the *Connoissance des Tems,* and the *Observations Astronomiques faites à l'Observatoire Royal de Paris,* tom. I.: this done, they are arranged in a tabular form, presenting at one view, the name of the observer, the place of observation, the nature of the occultation, the age of the Moon, and her motion at the time, whether northerly or southerly; information of a nature not easily to be abridged, and far too voluminous, to have insertion in the Monthly notices of the Society's proceedings.

On perusing them, however, we find, that more than 20 stars have exhibited peculiarities at, or on, the moon's limb, prior to immersion behind it, or emersion from it; that the anomalies are not confined to stars of a certain magnitude or colour; nor are they dependent upon any particular age of the moon. Most of them have furnished but solitary instances of peculiarity; viz. *Spica Virginis, γ Libræ, 132 Tauri, α¹ Cancri, λ Aquarii, 249 Aquarii, 187 Sagittarii, γ Tauri, ρ Leonis, ρ Geminorum, δ Cancri,* and *δ Piscium.* One, *Regulus,* affords three, whilst to *Aldebaran* we are indebted for no less than twenty instances of anomaly.

On reference to the list, the anomaly alluded to, it will be seen, stands not upon the testimony of a single individual, but is supported frequently by the evidence of a second, and sometimes even of a third person; occasionally they are co-observers at the same station; at other times they are at different parts of the same city; whilst in some instances, they are separated by a very considerable distance. On the other hand, the conflicting testimonies, where we should least expect to find them, are perplexing; a circumstance which together with the vague manner in which the observations are frequently recorded, and the habit, which in many instances unfortunately prevailed, namely, of observing the immersion and emersion of the same star, on the same occasion, with different telescopes, and the almost constant omission to register, if the moon's dark limb, *was* or was *not* visible, enable us, Mr. South says, to do little more than to state, with some appearance of probability, what are *not* the causes of the phænomena.

The hypotheses advanced as explanatory of the phænomena in question, are then stated: viz. A lively imagination on the part of the observer:—A spurious disc given to the moon's image by the instrument of observation:—A lunar atmosphere:—Irradiation:—And lastly, different refrangibilities to which the rays from the moon and star are liable, arising from their differences of colour.

As unfavourable to the first hypothesis, which would refer the phænomenon to a lively imagination on the part of the observer, Mr. South advances the fact, that 'more than sixty instances of anomaly stand attested by such men as Messier, Troughton, Bouvard, Arago and Mathieu; and that it is rather too much to suppose, that all of them are liable to the imputation, which such an hypothesis would require.'

The second hypothesis, which supposes a spurious disk to be given to the moon's image, might, he observes, be entitled to some consideration, had refracting telescopes whose object-glasses were not achromatic, been solely employed for the observations; but seeing that refractors, long and short, achromatic and non-achromatic; reflectors, newtonian and gregorian, most of which were probably far above the rank of good instruments, and some of which certainly might be brought forward as the most perfect specimens of optical ingenuity,—have all exhibited the anomaly; there is considerable difficulty in receiving the hypothesis; unless indeed we could grant that a constant cause should not produce a constant effect.

The next hypothesis offers a lunar atmosphere as the occasion of the apparent projection of a star on the moon's disk. Were this the case, its effects would be similar upon all stars of similar colour; and should we not see evidence of its existence, in some shape or other, at every occultation which occurred? yet, how infinitely few are the instances, in which any thing whatever is observable of alteration in the star, on the moon's approach to it; indicated by derangement of its position, diminution of its splendour, or change of colour. Still it must be remembered, such changes do stand on record; but they are either so unsubstantiated, or are so referable to other causes than a lunar atmosphere, that we are scarcely warranted in lending ourselves to the hypothesis, to which they would conduct us.

The hypothesis next in order, suggests irradiation as the source from which the anomaly is derived. It seems, however, Mr. South thinks, incapable of answering the purpose for which it is brought forward; seeing that projections of stars upon the moon's *dark* limb, have been witnessed by Messier, by Maskelyne, by Arago, by Mathieu, and by himself.

That the last hypothesis,—namely, that which supposes the apparent projection to arise from the different refrangibilities of the rays issuing from the star and moon,—is not tenable, Mr. South advances the circumstance, that not only Aldebaran and the *red* stars, are liable to the anomaly; but that stars as remarkable for their *white* light, as is Aldebaran for its *red,* have exhibited the phænomenon of apparent projection. He also says that, as far as he knows, no instance of apparent projection of the planet Mars upon the moon's disk, is at present recorded amongst the list of lunar occultations of that planet; yet Mars is much more decidedly *red* than Aldebaran, or than any other star which has been observed on the lunar disk.

Having, as he thinks, shown that the above hypotheses are inadequate to the purpose for which they have been designed, Mr. South states that he should not be justified in advancing any hypothesis in addition to those which he has combated; but concludes his paper by stating that from the *Connoissance des Tems,* he finds that the moon's path will, during the years 1829 and 1830 furnish several occultations of Aldebaran; when it is to be hoped that a phænomenon, which has been so little observed in Great Britain, that if it rested solely on the authority of British Astronomers would be scarcely entitled to any notice, will not longer furnish an object for their reproach.

ROYAL ACADEMY OF SCIENCES AT PARIS.

Feb. 4th, 1828.—Dr. Panquy presented an Essay on a natural chemical method.—M. Moreau de Jonnès communicated some details relating to the late earthquakes in the Antilles.—M. Freycinet read a letter from MM. Quoy and Gaymard, dated from Tongatabou.—MM. Latreille and Duméril gave a favourable account of a memoir presented by Dr. Bretonneau, On the blistering properties of some insects of the Cantharides family.—M. Coquebert de Montbret reported respecting a memoir of M. Auguste Duvau, intitled: *A Statistical Essay on the department of Indre and Loire, or ancient Touraine.*—M. Gay-Lussac announced that M. Guimet, assistant-commissary in the Powder and Saltpetre Works, had succeeded in manufacturing ultramarine, by combining the principles which chemical analysis had discovered to exist in it. This new product is richer in colour and more splendid than the natural one.—M. Ozenne continued the reading of his paper on the study of delivery. —The Academy elected a Commission by ballot, for adjudging the prize founded by M. de Monthyon, in favour of him who should render an art or trade less unwholesome.—The Commissioners are: MM. Thenard, Gay-Lussac, Darcet, Chevreul, and Dulong.

Feb. 11.—M. Lermier sent Researches concerning the influence which the will of man exercises upon inanimate bodies.—M. Julia Fontenelle, a letter On the phænomena of the incandescence of strontian and barytes.—M. Baehr of Kœnigsberg, a memoir intitled, *De Ovi Mammalium et Hominis Generi.*—M. Beudant gave a favourable account of M. Rozet's geognostic description of the Bas-Boulonnais.—M. Cuvier, on behalf of a Commission, presented an analysis of all the specimens which MM. Quoy and Gaymard had sent to the Museum of Natural History since the sailing of the Astrolabe: these indefatigable observers render themselves increasingly worthy of the protection of authority.—M. Geoffroy Saint-Hilaire reported respecting the memoir of M. Lisfranc, relating to the Rhinoplastic; the opinions are very favourable.—M. Civiale began the reading of a new memoir on Lithotrity.

Feb. 18.—The Minister of the Interior invited the Academy to examine the weighing machines made by M. Paret.—M. Tournal, Jun. sent a memoir On the geognostic constitution of the basin in the environs of Narbonne.—M. Lassaigne sent, by request of the Director of the School at Alfort, a molar tooth of an elephant, found fifteen feet below the surface, in a mass of sand and flints worked near the village of Maison.—M. Levret presented a memoir, intitled *Des Courbes et des Surfaces semblables.*—M. Donné read a memoir On the employment of iodine and bromine, as tests of the vegetable alkalies.—MM. Saint-Hilaire and Martin read a memoir On the anatomy of some parts of the female tortoise.—MM. Legendre, Poinsot and Cauchy gave a favourable account of an extremely curious memoir very long since presented to the Academy by M. Poncelet: this memoir was intitled, *Théorie générale des polaires réciproques.*—M. Lassis read the first part of a memoir On the yellow fever.

Feb. 25.—The Minister of War invited the Academy to examine M. Longchamp's Theory of Nitrification. M. Longchamp wrote to request that the examination might be confined to the inquiry, whether it was advisable to construct artificial nitre-works according to his theory.—M. Jomard communicated some details respecting Captain Clapperton and Major Laing, whose death had been announced.—M. Saint-Hilaire made a favourable report on MM. Audouin's and Edwards's memoir On the nervous system of the Crustaceæ.—M. Desfontaines gave a verbal account of M. Chevalier's new Flora of the environs of Paris.—M. Lassis concluded the reading of his memoir On the yellow fever.—M. Comte read anatomico-physiological researches on the causes of the superiority of the right hand.

March 3.—M. Leymeries sent several treatises respecting the causes of the yellow fever; M. Arago presented from M. Fiedler, several vitrified tubes produced by lightning; one of them was eighteen feet long.—M. Chevreul requested not to be one of the Commission appointed to examine the memoir of M. Longchamp on nitrification.—M. Latreille, on behalf of a Commission, gave a very favourable account of M. Edwards's memoir On the four undescribed species of Crustaceæ.—M. Savart, in the name of a Commission requested by the Minister of the Interior, announced that there would be no inconvenience in punching M. Paret's weighing machines.—M. Coquebert-Montbret gave a verbal account of several statistical researches by M. César Moreau, vice-consul of France in London, relating to the finances of Great Britain.

March 10.—M. Vauquelin presented a memoir by M. Farro On the copper extracted from vegetables—M. Raspail presented several plates relating to a memoir which he read in the month of September last.—MM. Duméril and Magendie, named by the Academy, at the request of M. Malbouche, to take cognizance of the processes received from America, and which, according to M. Malbouche, form a certain method of curing stammering, announced that the method succeeds in the greater number of cases.—M. Ampère gave an unfavourable verbal account of a publication by M. Opoix, respecting the sensations of sound and light.—There were afterwards read,—a memoir by M. Peclet, On the passage of hot air through pipes;—a memoir by M. Nicollet, On the latitudes of Barcelona and Montjouy, ascertained by M. Méchain;—a memoir by M. Raspail, On the granules of pollen;—and a memoir On the mechanism of the voice, by M. Begin.

March 17.—M. Deleau gave a written account of the progress of four deaf and dumb children, which had been put under his care.—M. Roche presented a memoir relating to the laws according to which the elastic force of vapour increases with the temperature.—M. Gendrin announced that he had obtained very good results in the employment of iodine in the gout.—An anonymous correspondent announced that he had discovered an infallible plan for stopping the leakages in the Tunnel under the Thames.—M. Brongniart read a letter from M. Acosta, respecting the earthquake which had destroyed a great part of the city of Popayan.—M. Warden communicated a note respecting two islands recently discovered in the Pacific Ocean, by Captain Joshua Coffin.—M. Arago replied to some doubts which had been expressed respecting M. Fiedler's vitrified tubes; and afterwards gave an analysis of an English memoir, which the President had sent him, respecting two auroræ boreales, observed in America.—M. Dumeril gave a very favourable account of the anatomical researches which MM. Martin and Isidore Saint-Hilaire presented respecting the anatomy of the tortoise and the crocodile.—M. Fourier presented a memoir On the conducting power of bodies.—M. Héricart de Thury read a notice respecting an overflowing spring, lately obtained by boring, in the park belonging to Madame Groslier at Epinay.—M. Dutrochet read additional researches on endosmosis and exosmosis.

March 24.—M. Poinsot presented a note on the formulæ, by the aid of which the invariable plan of our system is determined, regard being had to the rotary motion of the sun.—M. Cuvier exhibited a portion of a fossil jaw recently discovered in the gypsum of Montmartre, analogous only to that of an animal of Van Dieman's Land.—Dr. Foville presented his researches On the anatomy of the brain.—The Academy afterwards heard a verbal report by M. Damoiseau On the chronological researches of M. Eustache Oliveri;—a memoir On vision, by M. Vallée;—a memoir by M. Becquerel, On the effects of heat upon the tourmaline and bad conductors of electricity.

March 31.—The Academy received a sealed packet from M. Caventou, containing the results of some experiments on vegetable chemistry;—a letter from M. Coulier, On the means of preventing the falsification of writings;—a note by M. Sérullas, On the sweet oil of wine, oxalic æther, and bicarburetted hydrogen;—an extract of a memoir by M. Gaudin On colours;—an analysis of M. de Fermont's work On the circulation and on respiration;—a memoir by M. Vallot, On some ancient descriptions and drawings of the giraffe;—a notice by M. Thirria, On the grottos of Echenoz and Fouvent (Haute-Saône), and the fossil bones which they contain.—M. Mathieu, in the name of a Commission, reported on the memoir of M. Alexandre Roger, concerning the height of Mont-Blanc.—M. Beudant began the reading of a memoir On the chemical composition of mineral substances.

April 7.—The Academy received a memoir On the equilibrium of solid bodies, by MM. Lamé and Clapeyron;—a memoir by M. Duhamel, On the mathematical theory of heat;—a note by M. Braun, On the possibility of directing air-balloons;—a letter from M. Corencez, who offered himself as a candidate for the vacant place of member in the section of geometry;—a memoir by M. Farreau, On the presence of copper in vegetables, and the blood.—M. Geoffroy Saint-Hilaire stated that the anatomical facts discovered by his son and M. Martin, had been completely illustrated by examining yesterday a dead tortoise at the Menagerie.—M. Chevreul read a memoir On the influence which two colours may exert upon each other when seen together.

April 14.—The Academy received a sealed packet from MM. Chevalier and Langlumé, containing an account of some improvements in lithographic processes;—the results of meteorological observations made at Alais, in 1827, by M. d'Hombres-Firmas;—another letter from M. de Coulier, On the means of preventing the falsification of writings;—a memoir on the Euripode by M. Guerin;—a memoir by M. Corancez, On the integration of equations, &c.;—a note from M. Tournal, On the sulphur which had been found in the gypseous fresh-water formation at Narbonne.—M. Beudant read a notice on vitrified tubes.—M. Coquebert announced that, according to M. Pentland, there are in America several higher mountains than Chimborazo.—M. Geoffroy Saint-Hilaire gave an unfavourable account of M. Vallot's memoir On the giraffe.—M. Maisonabe exhibited a boy of twelve years old, who had club feet; one of them had already been subjected to treatment: M. Maisonabe will present him again when cured.—M. Arago, in the name of a Commission, reported on M. Bunten's modification of the barometer.—M. Beudant continued the reading of his paper On the analyses of minerals.—M. Héron de Villefosse communicated the results of statistical researches on iron.

April 21.—The Academy received: Considerations on light and colours, by Baron Blein;—a memoir by M. J. Cambessedes, On the families of the *Ternstromiaceæ* and *Guttiferæ;*—a memoir by M. Warden, On the civilization of the Cherokees;—Researches on the harvests of France formerly and at present, by M. Benoiston de Châteauneuf.— MM. Arago and Savart announced that the memoir of M. Braun On the means of directing air-balloons, contained nothing worthy of serious criticism.—M. Poisson read a memoir On the equilibrium and motion of elastic bodies.—M. Latreille gave a very favourable account of M. Guerin's memoir On a new genus of Crustacea, called *Euripode.*

April 28.—The reading of the minutes of the last sitting occasioned various explanations between MM. Poisson, Navier, and Cauchy, on the subject of differential equations proper to represent the internal motions of elastic bodies.—The Academy received A claim from M. Meller on the subject of M. Maisonabe's communication respecting the cure of club feet;—A sealed packet from M. Deleau, marked: Theory of stammering;—A letter from M. Despretz, relating to some fusible white crystals, volatile at a low temperature, which he had noticed during the decomposition of bicarburetted hydrogen subjected to a strong heat; and on the diminished density of iron, copper and platina, during the decomposition of ammonia by these metals. After the reading of this letter, M. Savart stated that he had himself long since found, in concert with M. Persoz, the last results obtained by M. Despretz. Several members of the Academy present at the sitting, asserted that M. Savart had mentioned it to them.—M. Delpech communicated several facts relating to *rhinoplasty*, to the disease known by the name of *trichyasis*, &c. &c.—Baron Blein read the memoir on light and colours, which he presented at the last sitting.—M. Warden communicated some information respecting the American colony established at Liberia, on the coast of Africa.—A Commission was appointed to propose a mathematical prize for 1830; it was composed of MM. Legendre, Fourier, Poisson, Lacroix, and Poinsot.—M. Longchamp read an additional notice on his theory of nitrification. A member, M. Arago, observed that M. Longchamp's memoir contained statements totally devoid of science; that he imagined he had even heard offensive personalities against a very distinguished foreign philosopher. He invited the President to listen attentively to the remainder of the memoir, and to decide, whether he ought not to stop the reading of it, in conformity with an article of the regulations.

Prizes adjudged by the Royal Academy of Sciences for the year 1828.

On examining the essays, it was found that no one of them sufficiently answered to the terms of the question, to be entitled to the grand mathematical prize.

Astronomical Prize, founded by M. Lalande.—The medal was adjudged to MM. Carlini of Milan, and Plana of Turin, authors of a work On geodesical and astronomical observations, &c.

Prize for experimental Physiology, founded by M. Montyon.—A gold medal was adjudged to M. Dutrochet, for his discovery of the phænomena which he has detailed under the name of *Endosmosis.*—Another medal was given to MM. Andouin and Edwards, for their experiments and observations upon the circulation and respiration of the Crustacea.

Prize for discovering the means of rendering an art or trade less unhealthy, founded by M. Montyon.—This prize was not awarded.

Prize founded by M. Montyon, for improving the healing art.—To M. Chervin, for his work on the yellow fever, 10,000 francs were awarded. 5000 francs to Baron Heurteloup, for his important improvements and ingenious instruments introduced this year in lithotrity. To Dr. Gruethuisen, for his works on the same subject, a gold medal of the value of 1000 francs.

Statistical Prize founded by M. Montyon.—This was awarded to M. Thomas, for his statistical account of the Isle of Bourbon.

LIII. *Intelligence and Miscellaneous Articles.*

CHLORINE IN BLACK OXIDE OF MANGANESE.

IN a former number of the Phil. Mag. and Annals I have noticed a paper published by Mr. MacMullen in the Institution Journal, the object of which was to prove that the native black oxide of manganese contains chloric acid. In my remarks I supposed I had proved that the source of chlorine was chloride of lime, which I found in all the specimens of peroxide of manganese submitted to examination. Mr. MacMullen has replied to my observations, and contends for the accuracy of his experiments and the inferences deduced from them. The only answer I think it necessary to give, is the observation of Dr. Turner printed in the Phil. Mag. for August last. "It is the accidental presence of the muriates which gives rise to the disengagement of chlorine when sulphuric acid is added to some of the native oxides of manganese, and which induced Mr. MacMullen to regard chloric acid as a constituent of these ores. For the correction of this error we are indebted to Mr. Richard Phillips*, with whose observation my own experiments correspond;—none of the native oxides yield a trace of chlorine on the addition of sulphuric acid, provided the muriates have been previously removed by washing."

In the Institution Journal for April last, Mr. James F. W. Johnston has advanced opinions respecting some compounds of manganese, which are almost as extraordinary and quite as groundless as those of Mr. MacMullen. I shall not notice all Mr. Johnston's statements; the correctness of the first two I admit; the third and fourth are as destitute of accuracy as the fifth, which is as follows:—"I threw down a *pure* carbonate from a pure muriate of manganese, obtained by Faraday's process. This was dried and partially decomposed by heating in an oven; with diluted sulphuric acid it gave also the smell of chlorine."

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* Phil. Mag. and Annals, N.S. vol. i. p. 313.

"From these experiments," continues Mr. Johnston, "we may legitimately conclude, first, that Mr. MacMullen was correct as to the *fact* of the emission of chlorine from the native oxide, which Mr. Phillips has called in question, for it is given off by *artificial* oxides, into which no trace of a muriate could possibly enter."

If Mr. Johnston had read my remarks upon Mr. MacMullen's paper, he would have found *not* that I called in question the fact of the emission of chlorine from the native oxide, but on the contrary that I admitted and explained it; nor can I discover the accuracy of the reasoning by which it is attempted to be proved, that the native peroxide of manganese must yield chlorine, because it is given off by the artificial peroxide, even admitting this to be a fact.

I assert, however, and every chemist will readily admit the correctness of my statement, that pure carbonate of manganese does not yield chlorine by the action of acids. I poured muriatic acid upon perfectly white and moist carbonate of manganese; no smell of chlorine was perceptible, and litmus paper was reddened instead of having its colour destroyed.

When, however, carbonate of manganese is dried, a portion of it is decomposed, oxygen being absorbed and carbonic acid evolved; and if muriatic acid is added to this mixture, chlorine is readily obtained, mingled with carbonic acid: this, however, will not account for the evolution of chlorine when sulphuric acid is employed to decompose the carbonate. But there are two modes of accounting for its production: first, if the precipitate be not sufficiently washed it will contain chloride of potassium or sodium, derived from the union of the base of the precipitating alkali with the chlorine of the muriate of manganese; or, which I have repeatedly found to be the case, a submuriate of manganese is formed; and sulphuric acid acting upon a mixture of carbonate, per- or deut-oxide and submuriate of manganese would readily occasion the evolution of chlorine for the disengagement of which Mr. Johnston finds it so difficult to account.

R. P.

BROWN OXIDE OF CHROMIUM.

This compound may be formed by mixing a solution of chromate of potash with protochloride of chromium, or by boiling chromic acid with protoxide of chrome; when the brown oxide obtained is digested with acetate of lead, chromate of lead and acetate of the protoxide of chrome are formed. Potash also converts it into chromic acid, and green oxide of chrome. Arsenic acid, carefully added, produces arseniate of chrome and chromic acid.

The brown precipitate produced by mixing chromate of potash and chloride of chromium, is decomposed by being repeatedly washed with water, especially if hot; chromic acid is removed, and green oxide of chrome remains. Chromate of chromium is decomposed in the same manner.

If chromate of ammonia be heated gradually to the point of decomposition, the salt is decomposed suddenly, pure deutoxide remains, which dissolves readily in concentrated acid. This oxide has been mistaken for a combination of protoxide and chromic acid. If at the moment of decomposition the temperature be suddenly raised, a luminous appearance is produced. Chromic acid dissolves the hydrate or the carbonate of chrome readily, and a dark brown solution is produced, which when evaporated leaves a brittle resinous-looking mass, which is deliquescent and soluble in alcohol. —M. Mans. *Annales de Chimie*, xxxvi. 216.

MASSES OF NATIVE PLATINA.

One by Humboldt from Peru, now in the Berlin museum, weight 1083 grains. Another from America in 1822, weighing 11,640 grains, now in the Madrid museum. A third within a few months from the Uralian mountains, deposited in the museum at St. Petersburgh, weighing above 81,000 grains.—*Jameson's Journal.*

PREPARATION OF TITANIC ACID.

The following is the method recommended by M. H. Rose:—Pulverize and wash titaniferous iron, expose it in a porcelain tube to the action of dry sulphuretted hydrogen gas, at a very high temperature. The oxide of iron is reduced and converted into sulphuret, whilst the titanic acid suffers no change: the product when cold is to be digested in concentrated muriatic acid; much sulphuretted hydrogen is given out, and sulphur is deposited; this mixing with the titanic acid, which the heat has rendered insoluble in the muriatic acid, the acid becomes grey. The acid is to be washed, dried and ignited, to expel the sulphur.

If the operation were to terminate here, the titanic acid would still contain some iron, and become red by calcination. The reason of this is, that the sulphuret of iron formed being in considerable quantity, agglutinates by heat with the titanic acid, and prevents the centre of the mass from being perfectly attacked by the sulphuretted hydrogen. On this account the operation must not be continued until water ceases to be disengaged, which would render it very long, but it must be stopped at the moment the water begins sensibly to diminish. The titanic acid is therefore to be exposed a second time in the porcelain tube to the action of sulphuretted hydrogen, and after having been treated with muriatic acid, well washed and ignited, it becomes perfectly white and pure. All varieties of titanic acid, which contain but little iron, and even rutile, may be treated by this process.

This process appears to me shorter and less expensive than that which I have before published, and which consists in dissolving titaniferous iron in muriatic acid, adding tartaric acid to the solution, and precipitating the iron by hydrosulphuret of ammonia. Not only is the newer process less complicated and expensive, but I have never yet found tartaric acid free from lime, and this base remains combined with the titanic acid. If during the treatment of the titaniferous iron by the sulphuretted hydrogen the heat is not very strong, the titanic acid obtained will render the washings milky, and will partly pass through the filter; but this will not occur if the heat be sufficiently great.

Hydrogen gas does not succeed so well as sulphuretted hydrogen: the oxide is indeed reduced, but the titanic acid obtained by this process is always ferruginous. Muriatic acid does not give a better result.

The time occupied in the operation now described may be shortened by fusing the titaniferous iron with sulphur in an earthen crucible. The mass is to be treated with concentrated muriatic acid, which removes much of the iron; but some remains with the titanic acid, and nearly as much as in rutile; by treating this impure acid with sulphuretted hydrogen as above described, it is obtained pure at one operation.—*Annales de Chimie*, xxxviii. 133.

ARTIFICIAL FORMATION OF UREA. BY M. F. WOHLER.

M. Wohler has already shown, that when cyanogen is made to act upon solution of ammonia, there are obtained, besides several other products, oxalic acid, and a white crystalline substance, which occurs also whenever the attempt is made to combine cyanic acid with ammonia by double decomposition. On prosecuting his inquiries, M. W. found that by the combination of cyanic acid with ammonia, urea was formed; this is a remarkable fact, as offering the artificial formation of organic matter, and even animal matter, by means of inorganic principles.

The white crystalline substance is most readily obtained by decomposing cyanate of silver by a solution of muriate of ammonia, or cyanate of lead by liquid ammonia; it is colourless, transparent, and crystallizes in the form of small rectangular quadrilateral prisms without any distinguishable pyramids. Neither potash nor lime evolves any trace of ammonia from this substance. Acids do not, as with the cyanates, disengage either carbonic or cyanic acid: it does not, like the cyanates, precipitate the salts of lead and silver; it is therefore evident that it contains neither ammonia nor cyanic acid. Most acids have no marked action on this substance, but the nitric acid when added to a concentrated solution gives a precipitate in the form of brilliant scales. These crystals are extremely acid, and were at first supposed to be a peculiar acid, but when decomposed by bases, nitrates of those bases were obtained; and by alcohol, the white crystalline matter was obtained unchanged in its properties: these properties, when compared with those of pure urea obtained from urine, indicated that this substance, or cyanate of ammonia, is absolutely identical with urea; a conclusion which is strengthened by the properties assigned to urea in the writings of Proust, Prout, and others. M. Wohler states some facts with respect to urea (and also with regard to this artificial substance,) which he says have not been previously noticed. When natural or artificial urea is decomposed by heat, there is produced, besides a large quantity of carbonate of ammonia, towards the end of the operation a smell of cyanic acid resembling that of acetic acid, precisely as occurs during the distillation of cyanate of mercury or uric acid, and especially urate of mercury. By the distillation of urea, a white substance is also obtained, the properties of which are under examination.

If cyanate of ammonia be similar to urea, then the composition of the former as obtained by calculation should resemble that of the latter; assuming one atom of water in cyanate of ammonia, as in all ammoniacal salts which contain any, and adopting Prout's analysis of urea as the most correct, it consists of

Azote	46·650	4 atoms.
Carbon	19·975	2
Hydrogen	6·670	8
Oxygen	26·650	
	99·945	

Cyanate of ammonia should consist of 56·92 cyanic acid, 28·14 ammonia, and 14·74 water, which give as its elements:

Azote	46·78	4 atoms.
Carbon	20·19	2
Hydrogen	6·59	8
Oxygen	26·64	

By the combustion of cyanic acid by means of oxide of copper, two volumes carbonic acid gas, and one volume of azote are obtained; but by the combustion of cyanate of ammonia, there should be procured equal volumes of these gases, which is what Prout actually found in the combustion of urea.—*Annales de Chimie*, April 1828.

NATIVE IRON IN THE UNITED STATES.

In the second volume of the Phil. Mag. and Annals, at p. 71, will be found an account of a variety of native iron found on Canaan mountain in Connecticut, extracted from Silliman's Journal. In the last Number of that Journal, which we have lately received, are the following particulars of the situation in which the iron was found, and of the probable existence of a mass of native iron at that spot. They are contained in Prof. E. Hitchcock's "Miscellaneous Notices of Mineral Localities."

"Sept. 6th, *Canaan*, *Connecticut*.—This is an interesting region, both to the geologist and mineralogist. We were attracted thither, principally by the hope of discovering the spot from which the native iron was obtained, that was recently announced in this Journal. We called upon Major Burrall, who, in search of the iron which he formerly obtained from this mountain, had recently visited it again, in company with his son, Mr. Wm. Burrall, a graduate of Yale-College, and Dr. Reed. Major B. not being able to go with us to the spot, the two other gentlemen just named, conducted us. About two miles north of the meeting-house, in the south parish in Canaan, is a precipitous mountain, nearly a thousand feet in height; and it was on its top, and near the western edge, that the native iron was found,—not three years ago as stated in this Journal, but as Major Burrall informed us, sixteen or seventeen years since. At the base of the mountain is limestone, succeeded by an aggregate of quartz and mica, which appears to be one of the varieties of Dr. Macculloch's quartz-rock. The top of the mountain, however, is well characterized mica-slate, containing small imperfect crystals of magnetic iron-ore, sparinglydisseminated. On the top of the mountain we came to a pond, perhaps sixty or eighty rods across; and on the southwest margin of this pond is the spot where, as well as Major Burrall can recollect, he obtained the specimens in question. At this spot he found his compass liable to so great a variation that it was useless; and on examining the rocks for the cause, he found the specimens that have excited so much interest. Mr. Burrall, junior, took his father's compass with him, on our present excursion, and attempted to run over the same line which his father pointed out to him, as the one upon which he experienced so much difficulty. This line runs nearly east and west, just upon the southern margin of the pond; and we found that where it approaches the nearest to the pond, there was a variation of 30°, as shown by back objects. On setting the compass only two or three rods backwards or forwards on the line, however, the variation almost entirely vanished. This showed us that the magnetic mass, that produced the variation, could not be far removed from the line, either north or south; for had it been at a considerable distance, the removal of the compass a few rods either east or west, could not materially have affected the variation; since the radius of a large circle for a considerable number of degrees, differs so little from the secant. We removed the compass one or two rods to the north, and ran a line parallel to that above named, so as even to enter a little distance into the pond, where the water is highest. Here the variation was even greater than upon the first line; so that the attracting mass must lie north of that first line. Probably it lies just in the edge of the pond; and I have no hesitation in saying, that a circle, described with a radius of two rods, upon the point where the greatest variation was noticed, would embrace the ferruginous mass that here disturbs the needle; nor is there much reason to doubt but that mass is native iron. And whoever has observed how large a mass of iron it requires to turn aside the needle of a compass, at the distance of one or two rods, will presume that the mass here deposited must be a large one. The spot I have been describing is covered with trees and thick underbrush, and the moss and rubbish almost entirely hide the rocks underneath. The bottom of the pond is sphagnous; and perhaps it might be necessary partially to drain it, which is not difficult. Whoever will be at the trouble and expense of removing the brush, moss and soil, at this spot, under the direction of Mr. Burrall, or Dr. Reed, will, I have little doubt, be abundantly rewarded by the discovery of a mass of native iron."

"On seeing this pond, and considering this locality of native iron on its margin, the inquiry forces itself on the mind,—may it not be the crater of an extinct volcano? But I could perceive not the least indication of any igneous action."

"Major Burrall presented me with a small specimen of the native iron, whose characters correspond exactly to those given in the 12th volume of this Journal; but it furnishes no additional information."—*Silliman's Journal*, vol. xiv. p. 223.

In the review of Prof. Olmsted's official "Report on the Geology of North Carolina," given in the same Number, p. 235, occurs the subjoined notice of specimens of native iron from that state.

"One of the specimens of iron-ore sent to Prof. Olmsted, from the slate-formation, or gold-region, proved to be native iron. Another was afterwards discovered that weighed twenty-seven pounds, and a part of it was wrought by the blacksmiths."—p. 31 and 108.

An account of the slightly arseniuretted native iron of Bedford county, Connecticut, (also extracted from Silliman's Journal,) will be found in the present volume of the Phil. Mag. and Annals, p. 73.

FOSSIL REPTILES.

M. Jæger, in his work *Uber die Fossile Reptilien, welche in Würtemberg aufgefunden worden sind*, Stuttgart, 1828, gives the following list of fossil reptiles found in the Wirtemberg rocks:

In the Lias	*Crocodilus Bollensis.*
	Geosaurus Bollensis.
	Ichthyosaurus platyodon.
	Ichthyosaurus communis.
	Ichthyosaurus intermedius.
	Ichthyosaurus tenuirostris.
	Plesiosaurus?
In the variegated Marl. (*Keuper.*)	*Cylindricodon.*
	Cubicodon.
In Aluminous Shale.	*Massodonsaurus.*
	Salamandroides giganteus.
In Muschelkalk	*Plesiosaurus.*
	Ichthyosaurus.
	A third Reptile.

H. T. D. B.

METHOD OF PRESERVING FUNGUSES.

Mr. Cooke, surgeon (Trinity Square, Tower Hill), having been very successful in his endeavours to preserve anatomical preparations in salt and water, was requested to try to preserve in the same way a specimen of *Clavaria muscoides* (Sowerby's English Fungi), supposing that it might answer for funguses of some kinds.

Mr. Cooke in a written account says: "I put it into brine a little below saturation, suspending it by a delicate thread of silk, and closing the bottle by means of glass. Since that time it has remained in the solution, and with the exception of having become a little deeper in colour it is unchanged. As spirits are not only expensive,

pensive, but usually deprive plants of all colour, the discovery of a cheap and effectual solution for the preservation of plants is a desideratum."

The specimen was gathered at the latter end of October 1826, and was presented to the Linnæan Society in May last, with an account of the process. As many species of funguses may be expected to appear at the latter end of this month and in the next, persons who are desirous of trying the before-mentioned method of preserving such vegetables, will no doubt have an opportunity of so doing.

Sept. 17, 1828. B. M. Forster.

DIFFERENCE OF LONGITUDE BETWEEN PARIS AND GREENWICH.

Captain Kater in his account of trigonometrical operations for determining this difference, (published in Part I. of the Phil. Trans. for the present year,) observes, p. 193, that the quantity $2^\circ 30' 17''\cdot73$, obtained by those operations, "converted into time is $9^m 21^s\cdot18$, differing only $0\cdot28$ in defect from the admirable results obtained by the operations with fire-signals, reported in the Phil. Trans. for 1826, by Mr. Herschel."

It may possibly save trouble to some future inquirers, to state, that Captain Kater here refers to the results of Mr. Herschel's operations, *as corrected* by Mr. Henderson in the Phil. Trans. for 1827, p. 295 (see also Phil. Mag. and Annals, N. S., vol. ii. p. 142), which give $9^m 21^s\cdot46$, or $9^m 21^s\cdot5$, to the nearest tenth of a second, instead of $9^m 21^s\cdot568$, and $9^m 21^s\cdot6$, as given in Mr. Herschel's paper.

Mr. Ivory also, in his paper On the measurement of degrees perpendicular to the meridian, in the last Number of the Phil. Mag. and Annals, refers to the same determination as corrected by Mr. Henderson; though he, like Captain Kater, refers only to the original paper in the Phil. Trans. for 1826, without mentioning Mr. Henderson's recomputation in the same work for 1827.

FIGURE OF THE CELLS OF THE HONEYCOMB.

To the Editors of the Philosophical Magazine and Annals.

Messieurs,

En faisant des récherches *sur les alvéoles des abeilles*[*], j'ai recueilli les détails suivans, qui peuvent servir à l'histoire du problème proposé par Reaumur.

1°. Le professeur Cramer, de Genève, envoya à Kœnig, en 1739, une solution qui ne différait pas de la sienne, étant appuyée comme elle sur le calcul de l'infini. Toutes les deux sont perdues.

2°. Le Père Boscowich, sans avoir connaissance de la méthode de Maclaurin, résolut comme lui le problème par la considération des *maxima et minima*. On trouve cette solution dans ses *remarques sur le poëme de Stay*.

3°. Enfin, Lhuilier, de Genève, a résolu aussi le problème, par un procédé plus simple encore que celui de Maclaurin, puisqu'il arrive au même résultat, sans employer la considération des *maxima et minima*: ce n'est plus qu'une question de géométrie commune.

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[*] See p. 20 and p. 233 of the present volume of the Phil. Mag. and Annals.—Edit.

Vos lecteurs, pour s'en assurer, peuvent recourir aux Mémoires de *l'Académie de Berlin*, année 1781. FAYOLLE.

ON VARIGNON'S METHOD OF SOLVING EQUATIONS OF THE SECOND DEGREE. BY M. FAYOLLE.

To the Editors of the Philosophical Magazine and Annals.

Messieurs,

La méthode que vous avez insérée dans votre dernier numéro, pour la résolution des équations du second degré, m'en a rappellé une de Varignon qui est encore plus simple, et qui s'applique aux équations du troisième degré. Elle est si facile (disait Fontenelle), qu'on est tout surpris que Varignon l'ait trouvée le premier.

La voici en peu de mots:

Soit $z^2+pz+q=0$.

Faisons $z=x-y$; alors

$$\begin{aligned} z^2 = x^2-2xy+y^2 &= x^2-2xy+2y^2-y^2 \\ &= x^2-2yz-y^2 \end{aligned}$$

Et par conséquent, $\left.\begin{matrix} z^2+2yz+y^2 \\ -x^2 \end{matrix}\right\} = 0$.

Laquelle comparée terme à terme avec la proposée, donnera

1°. $pz=2yz$, ou $y=\frac{1}{2}p$,

2°. $q=y^2-x^2$, ou $q=\frac{1}{4}p^2-x^2$,

D'où résulte $x=\pm\sqrt{\frac{1}{4}p^2-q}$

Donc $z=x-y=-\frac{1}{2}p\pm\sqrt{\frac{1}{4}p^2-q}$.

Si l'on avait $z^2-pz+q=0$,

Il faudrait prendre $z=x+y$.

Cette méthode s'applique avec la même facilité à l'équation du troisième degré $z^3+pz+q=0$,

Laquelle est dégagée du second terme.

Ce 16 Aug. 1828. FAYOLLE.

MINERALOGICAL LITERATURE.

1. Dr. Naumann, professor in the Mining Academy of Freiberg, has published *Lehrbuch der Mineralogie* (*Treatise on Mineralogy*), Berlin, 1828, by A. Rucker, in 8vo. This treatise, by a scholar of the celebrated Professor Mohs of Vienna, is one of the best on that science. The crystallographic method of Professor Naumann is eclectic in reference to those of Mohs and Weiss, and is very good; the system is established according to the physical and chemical characters of minerals. He describes a multitude of varieties of crystals with the assistance of 556 figures. In general the work is very classical, and deserves to be recommended to mineralogists.

2. Dr. Charles Hartmann, Mining-officer in the service of his Highness the Duke of Brunswick, has published *Worterbuch der Mineralogie und Geognosie*, (*Dictionary of Mineralogy and Geology*). Leipsic, by Brockhaus, 8vo. This work gives a description of all known minerals and rocks in alphabetical order, and contains an

introduction to mineralogy and geology, with the history and literature of the sciences in systematical arrangement. In reference to the crystallography, Dr. Hartmann pursues the methods of Professor Mohs and of Professor Weiss. This work merits the particular notice of all mineralogists, and also travellers, because the size of the book is not great, and the type very small. A German, an English, a French, and an Italian index facilitate the use of the book, and 312 figures illustrate the forms of the crystals.

3. Dr. Hartmann has also published *Vorlesungen über Mineralogie, &c.*,—(*Lectures on Mineralogy, particularly for Schools,*) &c. Ilmenau, by Voigt, 8vo. This elementary treatise is strongly recommended to young men who study the natural history of minerals, and to lecturers in schools. As in the elementary introduction of Mr. Phillips, the crystalline forms of minerals are illustrated with wood-cuts printed along with the text.

SCIENTIFIC BOOKS.

Just published.

No. I. of Zoological Researches and Illustrations; or Natural History of Nondescript or imperfectly known animals, in a series of Memoirs: illustrated by numerous figures. By John V. Thompson, Esq., F.L.S., Surgeon to the Forces; author of A memoir on *Pentacrinus Europæus.*

This first Number contains the following memoirs: 1. On the metamorphoses of the *Crustacea,* and on the animals forming the genus *Zoea*, exposing their singular structure, and demonstrating that they are not, as has been supposed, a peculiar genus, but the larvæ of *Crustacea:* with two Plates. 2. On the genus *Mysis,* or Opossum Shrimp; also with two Plates.

The author of this novel contribution to the stores of zoological science, has we understand devoted much time and exertion to the collection and preparation of materials for the work. They are wholly original, and the results of observation on animals of every class, but more especially on the marine *Invertebrata,* in both hemispheres of the world.

No. II. will be published in January; and the succeeding Numbers at intervals of three or four months.

Elements of Algebra. By Robert Wallace, A.M., late Andersonian Professor of Mathematics, Glasgow.

A Circular, explanatory of Skene's patent as applicable to steam-navigation, and undershot water-mills.

No. II., for the year 1829, of the "Enigmatical Entertainer and Mathematical Associate."

In the Press.

An American reprint of Mr. Bakewell's new and enlarged edition of his "Introduction to Geology" is announced, under the superintendence of Professor Silliman.

A second edition, considerably enlarged, of Mr. De la Beche's Tabular and Proportional View of the Tertiary and Secondary Rocks.

LIST OF NEW PATENTS.

To G. Stratton, of Frederick-place, Hampstead Road, for an improvement in warming and ventilating buildings.—Dated the 28th of August 1828.—6 months allowed to enrol specification.

To Granville Sharp Pattison, of Old Burlington-street, esquire, for a method of applying iron in the sheathing of ships, and of applying iron bolts, spikes, nails, and other fastenings in ships and other vessels.—4th of September.—6 months.

To J. Seaward and S. Seaward, of the Canal Iron-Works, Poplar, for a new method for propelling carriages on roads, and also ships and other vessels on water.—4th of September.—6 months.

To C. Sanderson, of Park Gate Iron-Works, near Rotherham, Yorkshire, for a new method of making sheer steel.—4th of September.—2 months.

To Admiral S. Brooking, of Plymouth, for his new mode of making sails of ships and other vessels.—4th of September.—6 months.

To J. Robertson, of Limehouse Hole, Poplar, for improvements in the manufacture of hempen rope or cordage.—4th of September.—6 months.

To W. Bell, of Lucas-street, Commercial Road, Middlesex, for his improved methods for filtrating water and other liquids.—4th of September.—6 months.

To W. Farish, Jacksonian Professor in the University of Cambridge, for his improved method of clearing out water-courses.—4th of September.—6 months.

To T. R. Williams, of Norfolk-street, Strand, for improvements in the making of hats, &c., and in the covering of them with silk and other materials with the assistance of machinery.—11th of September.—6 months.

To T. Minikew, of Berwick-street, for an improvement in the making of chairs, sofas, beds and other articles of furniture for similar purposes, and also of travelling and other carriages for personal use.—11th of September.—2 months.

To J. B. Neilson, of Glasgow, for the improved application of air to produce heat in fire-forges and furnaces where bellows or other blowing apparatus are required.—11th of September.—6 months.

To L. W. Wright, of Mansfield-street, Borough Road, Surrey, for improvements in machinery for making screws.—18th of September.—6 months.

To W. Losh, of Benton House, Northamptonshire, esquire, for improvements in iron-rails for rail-roads, and of the chains or pedestals in or upon which the rails may be placed or fixed.—18th of September.—2 months.

To J. Rhodes, junior, of Alverthorp, Wakefield, for improvements in machinery for spinning and twisting worsted yarn and other fibrous substances.—18th of September.—6 months.

METEOR.

On Sunday evening last, about half-past eight o'clock, I observed a most splendid meteor in the north-east, at an altitude of about 45°, and having a diameter of about 20′ of a degree. Its course (eastward) was rapid, inclined to the horizon at an angle of about 50°, and described in three or four seconds an arch of about 15°, when it disappeared quite abruptly. During its appearance the light was of a splendour equal to that of the mid-day sun.

At Newhouses, about a mile north of this place, it appeared at the commencement of a size not exceeding that of a shooting-star, but increased almost instantaneously to a diameter equal to that of the sun. On the point of disappearing it separated into numerous *scintillant* fragments.

Horton in Ribblesdale, Sept. 10, 1828. J. NIXON.

AURORA BOREALIS?

A remarkable light resembling the aurora borealis was lately observed at Boreham in Essex, by our correspondent Dr. Forster; it occupied the northern hemisphere, to which so late even as midnight it gave a fine clear *orange* tint, at times so clear that one might have read a large print by it. As it declined, a storm gathered in the same quarter, which is rather an unusual circumstance; and by about two o'clock thunder began to be heard. The storm increased before three, and became one of the most violent ever remembered in the county: an almost uninterrupted succession of flashes, with sharp and rolling thunder, continued till past four o'clock, when the storm gradually passed off.

SOLAR SPOTS.

The two solar spots described under our last Meteorological Report, p. 236, came on the sun's eastern limb again on the 6th instant, but from the interposition of clouds the time could not be ascertained. The lower spot, which had before divided into four, returned waning, and without any perceptible umbra: by the 9th they had increased to eight very small black specks, and on the 12th were evanescent, making the period on the sun's disk from the appearance of the original spot, sixty days. The upper one returned with additional spots near it, which we shall not notice at present, although one of them from its proximity has been found useful to a continuance of the observations. On the 9th its umbra was conspicuous, but the nucleus had decreased to a small size. On the 13th it was again nearest to the sun's centre, and on the 16th it had lost the eastern side of its umbra. On the 19th at sunset it was on the sun's western limb, having a large facula on the eastern side of it (in which it is probable another spot will soon appear), and must have gone round on his posterior side between 9 and 10 o'clock that evening. As it did not return again, we shall now close our remarks on it. Up to the 19th instant it had been on the sun's disk 91 days, a longer period perhaps than any solar spot on record; during which time it presented a variety of changes in appearance and magnitude, varying from 4000 to 14,000 miles in diameter, and afforded good opportunities from day to day in fine weather of ascertaining satisfactorily the *apparent time* of each revolution, namely, 27 days, 7 hours, and a few minutes, which latter it would be difficult to determine with any degree of accuracy; however, the minutes cannot at the furthest exceed twenty, which we will allow. From this time, 27 days, 7 hours, 20 minutes, it is proper to subtract 1 day, 21 hours, 40 minutes for the angular distance in time that the earth made in the ecliptic during each revolution of the spot; hence the *real time* of each respective revolution, and also the real time of the revolution of the sun on its axis, which is the cause of the apparent motion of the solar spots, is 25 days, 9 hours, and 40 minutes. Our daily observations on other solar spots in the interim, after they had made one revolution, corroborate the accuracy of this time.

If any malign influence on the weather exist from the appearance of solar spots, it has been verified in the summers of 1816, 1823, and 1828, by the great number which then appeared on the sun's disk, and the wet and stormy state of the earth's atmosphere at these periods. But to demonstrate this hypothesis of a great man, which has recently been alluded to in the provincial newspapers, would require great labour and anxiety at the telescope in the day; yet we think the task is by no means insurmountable, where time is no object to an accurate observer.

METEOROLOGICAL OBSERVATIONS FOR AUGUST 1828.

Gosport.—Numerical Results for the Month.

Barom. Max. 30·29 Aug. 26. Wind S.E.—Min. 29·36 Aug. 6. Wind S.W.
Range of the index 0·93.
Mean barometrical pressure for the month .... 29·860
Spaces described by the rising and falling of the mercury............ 4·300
Greatest variation in 24 hours 0·430.—Number of changes 22.
Therm. Max. 76° Aug. 24 & 25. Wind W.—Min. 47° Aug. 15. Wind N.E.
Range 29°.—Mean temp. of exter. air 63°·18. For 31 days with ☉ in ♌ 62·60
Max. var. in 24 hours 25°·00—Mean temp. of spring water at 8 A.M. 55°·20

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 13th .... 94°
Greatest dryness of the air in the afternoon of the 15th............... 45
Range of the index.. 49
Mean at 2 P.M. 59°·5—Mean at 8 A.M. 66°·4—Mean at 8 P.M. 71·5
—— of three observations each day at 8, 2, and 8 o'clock......... 65·8
Evaporation for the month 3·05 inches.
Rain near ground 2·585 inches.
Prevailing wind, S.W.

Summary of the Weather.

A clear sky, 4½; fine, with various modifications of clouds, 12; an overcast sky without rain, 9; rain, 5½.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
23	15	30	0	24	26	21

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
1	4½	2½	3	1	12	3	4	31

General

General Observations.—To the 14th of this month the atmosphere constantly wore a humid aspect, and more or less rain fell daily (accompanied with high winds), which from its frequently interrupting the operations of the harvest, excited much alarm among the agriculturists in this and the adjoining counties for the fate of the outstanding part of their corn crops. The remainder of the period being fine and dry, has certainly proved a blessing to the country in general, and enabled the farmers to get in their wheat and barley in tolerable condition; and it is said from undoubted authority that the crops will yield at least an average quantity, and in many places much more.

In several of the northern districts, it would appear, from their reports, that the thunder-storms had been more frequent, and the rain more copious than with us, which beat down and spoiled much of the corn; but the weather having changed favourably, they say that all will be well with them, and that they shall have no necessity to advance the prices of their corn. On the morning of the 5th instant, thunder-storms with heavy rain were experienced both on Portsdown Hill and at Southampton: the storm at Portsdown was seen from this place, and the thunder repeatedly heard, yet only a few drops of rain fell here. On the 9th, 10th, and 11th, four strata of clouds one above another frequently prevailed, with a hard gale of wind from the S.W., which literally rooted up several trees in this town and neighbourhood, and was much felt at Plymouth. It was succeeded on the 13th and 14th by a gale equally as strong from the opposite quarter, N.E., which blew back the dense black clouds that had been carried thither by the S.W. gale. The latter day was remarkably cold, the mean temperature of the external air being only equal to that in the middle of May.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are three parhelia; two solar halos, forty-four meteors; thunder on three different days; lightning in the evening of the 10th; and twelve gales of wind, or days on which they have prevailed; namely, four from the North-east, seven from the South-west, and one from the North-west.

REMARKS.

London. — August 1. Very fine. 2. Slight rain in morning; showery. 3. Rainy. 4, 5. Very fine. 6. Heavy rain with thunder in evening. 7. Cloudy with showers. 8. Very fine. 9. Stormy with showers. 10. Fine. 11. Cloudy, with rain at night. 12. Showery. 13. Heavy rain. 14. Rainy. 15. Fine. 16. Foggy morning: very fine. 17. Cloudy. 18—20. Very fine. 21, 22. Showery. 23, 24. Very fine. 25. Sultry and warm. 26. Foggy morning: very fine. 27—30. Very fine. 31. Cloudy.

Boston.—August 1. Cloudy. 2. Rain. 3. Fine: rain, P.M. 4. Fine: rain P.M., distant thunder P.M. 5. Fine: heavy rain P.M. 6. Cloudy: rain P.M. 7. Cloudy. 8. Fine: storm of wind: rain, thunder, and lightning, 5 P.M. 9. Fine: stormy day, rain P.M. 10. Stormy. 11. Cloudy: storm of wind, rain, hail, thunder and lightning, 4 P.M. 12. Stormy. 13. Fine. 14. Rain. 15. Cloudy. 16. Fine. 17. Cloudy: rain early in the morning and P.M. 18—20. Fine. 21. Fine: rain A.M. 22. Cloudy: rain A.M. and P.M. 23. Fine. 24, 25. Cloudy. 26. Misty. 27. Fine: 5 P.M. Therm. 74°. 28. Misty. 29. Fine. 30, 31. Cloudy.

Penzance.—August 1. Rain. 2, 3. Showers. 4, 5. Fair. 6. Rain: fair. 7. Rain: clear. 8. Clear: rain at night, stormy. 9. Cloudy: showers. 10. Clear: showers. 11. Clear: heavy showers. 12, 13. Fair: showers. 14. Heavy rain: fair. 15. Showers. 16. Rain: misty. 17. Rain: fair. 18, 19. Clear. 20—22. Fair. 23. Clear. 24. Fair. 25—31. Clear.

Meteorological Observations made at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Giddy at Penzance, Dr. Burney at Gosport, and Mr. Veall at Boston.

Days of Month, 1828.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost. 8½ A.M.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.										
Aug. 1	29·959	29·895	29·95	29·75	29·95	29·88	29·47	74	55	64	54	71	59	60	w.	sw.	s.	calm	...	...	...	0·135	...
☾ 2	29·691	29·630	29·70	29·68	29·71	29·64	29·23	69	53	67	57	69	56	60	s.	w.	sw.	calm	...	0·65	0·780	·260	...
3	29·584	29·543	29·65	29·65	29·60	29·56	29·05	68	52	66	57	69	56	60	sw.	w.	sw.	w.	0·15	·50	·200	·010	0·12
4	29·584	29·567	29·65	29·65	29·60	29·58	29·00	70	53	67	55	72	55	61·5	sw.	w.	w.	w.	...	·12	·045	·345	·03
5	29·644	29·616	29·72	29·70	29·65	29·60	29·03	72	50	65	54	74	56	62	sw.	w.	sw.	w.	...	...	...	·110	·03
6	29·477	29·420	29·45	29·45	29·43	29·36	29·02	70	56	67	55	71	60	61	s.	sw.	sw.	w.	·30	·42	·360	·150	·35
7	29·527	29·454	29·55	29·45	29·48	29·42	28·95	72	58	66	55	72	56	61·5	s.	w.	sw.	r.	...	·57	·385	·025	·19
8	29·742	29·614	29·75	29·62	29·65	29·58	29·10	72	55	68	58	73	59	63	sw.	sw.	sw.	calm	...	·13	...	·100	·04
9	29·502	29·406	29·55	29·15	29·51	29·42	28·90	68	56	65	56	70	58	64	sw.	sw.	s.	s.	·30	·04	·435	·090	·55
● 10	29·725	29·634	29·70	29·60	29·70	29·62	29·00	69	53	66	54	69	55	62	sw.	w.	sw.	w.	...	...	·060	·210	·04
11	29·804	29·707	29·75	29·70	29·70	29·67	29·16	69	55	64	54	69	56	61	sw.	sw.	sw.	w.	...	·36	·360	·270	...
12	29·860	29·794	29·85	29·80	29·83	29·80	29·21	69	46	64	54	70	58	60	w.	w.	sw.	w.	·40	·11	·050	·065	·17
13	29·836	29·698	29·78	29·68	29·78	29·70	29·35	62	54	62	51	70	56	60·5	SE.	NE.	E.	w.	...	1·14	·205	·785	...
14	29·724	29·552	29·80	29·65	29·62	29·47	29·21	58	47	61	54	60	48	57	E.	N.	NE.	NE.	...	0·12	1·190	...	·21
15	29·934	29·863	29·95	29·85	29·90	29·84	29·30	68	45	64	51	69	47	56·5	NW.	w.	N.	N.	·20	...	0·105	...	...
16	29·966	29·954	29·90	29·85	29·94	29·90	29·50	72	58	66	55	65	60	58	w.	sw.	SE.	w.	...	...	·130	...	...
17	29·905	29·838	29·87	29·85	29·88	29·86	29·30	68	51	64	58	73	55	64	sw.	sw.	sw.	w.	...	·04	·230	·005	·02
☽ 18	29·968	29·934	30·05	30·00	30·02	29·93	29·27	69	49	66	56	72	51	62	w.	NW.	w.	w.	·30	...	·040	·010	·17
19	30·147	30·127	30·10	30·08	30·14	30·12	29·51	74	46	67	55	68	53	60·5	sw.	w.	NW.	w.	...	...	...	...	...
20	30·081	29·959	30·05	30·00	30·08	29·97	29·50	73	52	67	54	70	58	62	s.	s.	sw.	w.	...	...	...	...	...
21	29·856	29·772	29·98	29·95	29·87	29·80	29·30	67	50	64	55	72	53	61	s.	NW.	NW.	calm	·40	·10	...	·010	...
22	29·834	29·804	29·96	29·94	29·88	29·77	29·06	65	49	64	54	68	51	57	NW.	NW.	NW.	w.	...	·05	...	·005	·11
23	30·137	30·025	30·20	30·15	30·13	30·03	29·47	67	44	66	54	71	51	59	w.	NW.	NW.	NW.	...	...	...	...	·23
24	30·201	30·187	30·24	30·23	30·23	30·17	29·64	76	61	70	58	76	58	62	w.	NW.	w.	w.	·35	...	...	...	...
○ 25	30·338	30·296	30·25	30·24	30·28	30·26	29·70	78	59	68	60	76	60	66·5	NW.	s.	NW.	N.	...	...	...	...	...
26	30·340	30·323	30·20	30·18	30·29	30·25	29·75	78	48	68	57	73	56	64	E.	SE.	SE.	calm	...	...	...	...	...
27	30·297	30·231	30·12	30·10	30·23	30·17	29·72	74	52	68	58	67	57	62	E.	SE.	E.	calm	·25	...	...	...	...
28	30·218	30·190	30·05	30·04	30·16	30·13	29·65	75	54	68	58	70	58	61·5	NE.	SE.	SE.	calm	...	...	...	...	...
29	30·198	30·192	30·05	30·03	30·15	30·13	29·65	74	58	69	59	74	57	63·5	E.	SE.	SE.	E.	...	...	...	...	...
30	30·193	30·140	30·08	30·03	30·12	30·10	29·65	72	55	68	58	72	57	62	NE.	SE.	NE.	E.	...	...	...	...	...
31	30·123	30·065	30·08	30·05	30·06	30·00	29·62	62	56	67	57	69	61	59	SE.	NE.	NE.	NE.	·40	...	...	...	...
Aver.:	30·340	29·406	30·25	29·15	30·29	29·36	29·33	78	44	70	51	76	47	61					3·05	4·35	4·575	2·585	2·26

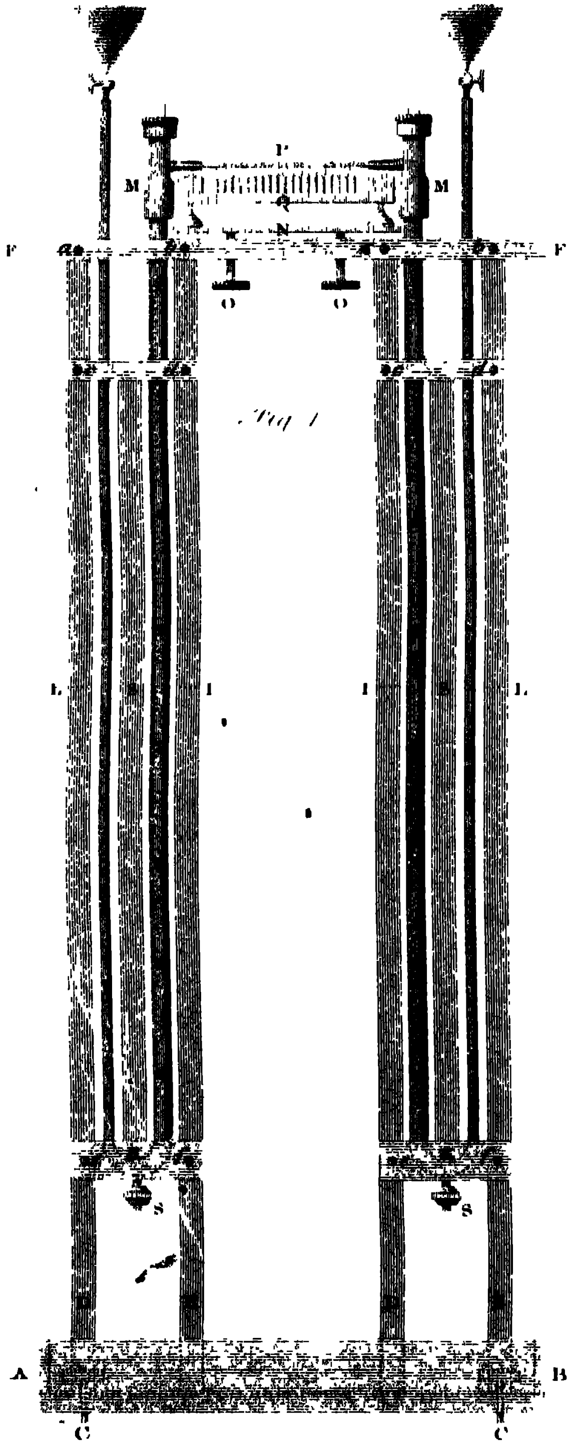

Fig 1

THE
PHILOSOPHICAL MAGAZINE
AND
ANNALS OF PHILOSOPHY.

[NEW SERIES.]

NOVEMBER 1828.

LIV. *Answer to an Article by* Mr. Henry Meikle, *published in No.* VII. *of the Quarterly Journal of Science. By* J. Ivory, *Esq. M.A. F.R.S. &c.*[*]

In the last Number of the Quarterly Journal of Science, there is an article of some length by Mr. Henry Meikle, in which he animadverts in a style which is hardly called for, on the analytical theory of sound, and on the variation of temperature which air undergoes when it changes its bulk. In his strictures on the first of these subjects he does little more than enlarge on the observations which Professor Leslie has more briefly made relative to the same matter in the article *Acoustics*, in the Supplement to the *Encyclopædia Britannica.* Whatever purpose such discussions may serve, one is at a loss to find out how they can benefit science. Great fault is found; no remedy is proposed; the subject is ultimately left just where the author found it; and the whole ends in a vain display of learning and fine writing. Whoever will attend to Mr. Meikle's remarks will soon be convinced that any attempt to answer them is not a very likely way to elucidate, or to improve, the theory he attacks. Besides, this theory has been long known; I have only made use of the most simple case of it for a particular purpose; and therefore I do not think myself indispensably called upon to defend it against such arguments as are contained in the article under consideration. Leaving, then, the analytical theory of the propagation of sound to stand on its own merits, I must, however, express my regret that Mr. Meikle has made so little progress towards that thorough reform which, he thinks, is so necessary.

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[*] Communicated by the Author.

I proceed next to Mr. Meikle's strictures on the explanation, I have given, of the variation of temperature when air changes its bulk. I have not seen any of the articles he cites in the Edin. Phil. Journ. What I have written on this subject is no more than an easy deduction from the usual theory of the thermometer; and the best way to enable the reader to judge of the justness of Mr. Meikle's animadversions, is briefly to explain the principles I proceed upon, disengaging them, as far as perspicuity will permit, from all purely mathematical calculations.

In an air-thermometer, or, which is the same thing, in a mass of air which changes its bulk by the application of heat, the pressure being constant, I take it for granted that the capacity of the air for heat, within certain limits, remains invariable; that is, I suppose that equal quantities of absolute heat, produce equal variations of volume and temperature. It will be admitted that this postulate is true, so long as two thermometers, one of air and the other of mercury, both exposed to the same flow of heat, continue to indicate the same temperatures. To speak more particularly, but without aiming at great precision, the capacity of air for heat may be supposed constant between -40° and $+300^\circ$ on the centigrade scale, comprising a range of temperature between 300° and 400°. The reasoning which follows is entirely founded on this experimental fact, and it must be considered as applying only within the limits mentioned.

Suppose that the given mass of air has undergone a certain variation of bulk; and let us put h for the whole quantity of heat which has produced this effect; and τ, for the change of temperature in passing from the one volume to the other; then we shall have $\frac{h}{\tau} = k$, k standing for some number not yet known, which however is invariably the same so long as the temperature τ is contained within the assigned limits. All this is manifest from what is said above; for the equation merely expresses that the absolute heat which causes any *change of volume is proportional* to the variation of temperature, according to the postulate laid down, and as must be the case if the thermometer be an exact measurer of heat. Now h, whether it is heat added to, or abstracted from, the mass of air, is always greater than τ, the rise or depression of temperature. For when heat is added, the volume of the air is enlarged, and some heat is absorbed and disappears without acting on the thermometer; on the other hand when heat is abstracted, the volume is diminished, and some heat is evolved which is instantaneously dissipated and produces no effect on the fall of temperature. We may therefore put $h = i + \tau$; and, on account of the foregoing equation, we shall have,

$$\frac{i}{\tau} = k - 1. \qquad \text{(a)}$$

The new symbol i stands for the difference between the absolute heat, and the heat of temperature, answering to any change of the bulk; or it is the heat which enters into the air when its volume increases, and is again extricated when the volume decreases, without affecting the thermometer in either case. I shall call latent heat, that portion of the absolute heat which in ordinary circumstances is not indicated by the thermometer, when a mass of air under a constant pressure changes its bulk. This term was introduced by Dr. Black; but it has been objected to and proscribed by some chemists, who express the same thing by a phraseology certainly not more perspicuous. There can be no impropriety nor inconvenience in using the term, since it is no more than the unambiguous expression of a fact, the existence of which is certain. It remains now to discover the value of the number k; for when this is done, the equation (a), taken always within the assigned limits, contains the whole of the doctrine under consideration.

In order to find the number k, or $k-1$, we must have recourse to an experiment contrived by MM. Clement and Desormes. These celebrated chemists have invented a very ingenious process by which we can ascertain the proportion of the latent heat to the heat of temperature, that is, the value of $\frac{i}{\tau}$, when a given mass of air under a constant pressure suffers a small variation of bulk. Taking an average of many experiments, it has been found that $\frac{h}{\tau}$ and $\frac{i}{\tau}$, that is, k and $k-1$, are nearly equal to $\frac{11}{8}$ and $\frac{3}{8}$. Now, making $k-1 = \frac{3}{8}$, the equation (a) will coincide with the conclusion which I have stated at p. 94 of this Journal for February 1827, and which enables us to compute the latent heat for a given variation of bulk. MM. Gay-Lussac and Welter repeated the experiment alluded to on air under a great variety of pressures, and in a range of temperature reaching from $-20°$ to $+40°$ on the centigrade scale; and the resulting proportion of the latent heat to the heat of temperature came out in every instance very nearly the same. By this means, not only are the numbers k and $k-1$ ascertained to a considerable degree of precision, but it is likewise demonstrated that, within certain limits, they are independent of the state of the air. Now this is a practical proof in favour of the theory we have been explaining; since, as far as experiments have been extended, the ratios $\frac{h}{\tau}$ and $\frac{i}{\tau}$ are found to remain constant, as the theory requires.

But in applying the foregoing doctrine to the velocity of sound, I made use of algebraic formulas, which are things held in abhorrence by a certain class, although it appears that others know very well how to turn them to their own purposes. Taking a mass of air at a given temperature, suppose zero of the centigrade scale, let α denote the dilatation under a constant pressure for 1° of temperature, and β the dilatation requisite for the absorption of 1° of latent heat; then it is obvious that shall we have,

$$k - 1 = \frac{\alpha}{\beta} = \frac{3}{8}.$$

I now take one of the formulas marked (C) at p. 252 of this Journal for April 1827, viz.

$$\frac{\varrho}{\varrho'} = \frac{1 + \alpha\theta}{1 + \alpha\theta + \beta i};$$

whence I get, $$i = \frac{1}{\beta}(1 + \alpha\theta)\left(\frac{\varrho'}{\varrho} - 1\right).$$

It will perhaps contribute in some degree to perspicuity, if, instead of the proportion of the densities $\frac{\varrho'}{\varrho}$, we substitute the inverse proportion of the volumes, viz. $\frac{\text{V}}{\text{V}'}$; then

$$i = \frac{1}{\beta}(1 + \alpha\theta)\left(\frac{\text{V}}{\text{V}'} - 1\right).$$

The formula will be still more simplified if we make the initial temperature θ equal to zero, in which case V′ will be the volume of the air at the beginning of the thermometrical scale; then

$$i = \frac{1}{\beta}\left(\frac{\text{V}}{\text{V}'} - 1\right).$$

And, if τ be the variation of temperature, we obtain, by the theory of the thermometer,

$$\tau = \frac{1}{\alpha}\left(\frac{\text{V}}{\text{V}'} - 1\right).$$

These formulas show clearly the relation that subsists between the temperature and the latent heat, and in what manner both these quantities are derived from the volume. Both the expressions are significant, and represent what actually takes place in nature, so long as the postulate on which they are founded holds good; or so long as the capacity of the air for heat remains constant, and the absolute heat which changes the bulk of a mass of air is proportional to the variation of temperature. Beyond this limit on either side, the formulas become insignificant; they are mere abstract expressions which denote nothing that is true in nature. It is therefore absurd to contend from the numerical results obtained in some extreme cases, that the formulas are consonant to fact in no case whatever; I say, the formulas, for the argument applies equally to both. Suppose that the original volume is reduced to a very small space, or to a point; then, according to the centigrade scale,

$$\tau = -266^\circ{\cdot}7, \quad i = -100^\circ.$$

Now these numbers are either both true, or neither of them is so; and the latter may be affirmed, since it is very improbable, that a law, which has been verified only for a small part of the thermometrical scale, will continue to hold good without limit; it is even certain that it will not. Next let us suppose that the volume of air has decreased to half its original quantity; then, $\tau = -133^\circ, \quad i = -50^\circ.$
There is good reason to think that these numbers are not far from the truth; but it would be rash to affirm that they agree exactly with the phænomenon; for there is no proof, at least I am not aware of any, that a mass of air at the temperature zero, being cooled down to half its bulk, will still preserve the same capacity for heat. If we suppose a greater diminution of volume than one half, we are entirely ignorant of the manner in which the temperature and the volume vary in regard to the absolute heat; and, as the principle of the investigation now ceases to be exact, the conclusions obtained, whether expressed in algebraic language or otherwise, must no longer be applied.

All Mr. Meikle's objections to my doctrine are derived from the extreme cases just mentioned. His arguments have no force; since I have always confined my speculations to the limits within which the thermometer can be reckoned an exact measurer of heat. He finds many inconsistencies, and he descants on this topic with so much politeness, that he seems seriously to think, his remarks have some foundation. If such be the case, it will be allowed that the same ability does not attend the same person on all subjects and on all occasions; for we can here recognise very little of that acuteness and sagacity from which we expect the *thorough reform.*

M. Poisson has treated this subject in an able memoir in the *Conn. des Temps* 1826. His equations agree with the doctrine here delivered as far as the formula (7), p. 264, which is derived from an integral to which I have objected. On the preceding page he arrives at this equation, $\frac{\omega}{n} = k - 1$, k standing for the same value as in this article, and ω and n being the variations of latent heat and temperature arising from a small condensation γ. The equation may therefore be thus written, $\frac{di}{d\tau} = k - 1$; and as it is true of any number of continuous variations of density or volume, the pressure being constant, it agrees with the equation (a) investigated above. Observing also that $\gamma = \frac{d\varrho}{\varrho}$, and $k - 1 = \frac{\alpha}{\beta}$, his two equations (5) and (6) will become,

$$\frac{\alpha d\tau}{1 + \alpha\theta} = \frac{d\varrho}{\varrho},$$

$$\frac{\beta di}{1 + \alpha\theta} = \frac{d\varrho}{\varrho},$$

θ being the actual temperature of the air. In the first of these equations, $d\varrho$ is derived from $d\tau$; and in the second, $d\varrho$ determines di. The two equations are therefore intimately connected, and no just conclusion can be deduced from the second, if the first be overlooked. Now M. Poisson has integrated the second equation apart, and as if it were in no respect modified by the first. This is the ground of my objection. In reality the integral he obtains satisfies the second equation, but it does not satisfy the first, as it ought to do according to his own calculations. If we reason fairly, and fulfil all the relations of the differential quantities, we shall be necessarily led, even when we follow M. Poisson's train of investigation, to the same theory explained in the present and in former articles of this Journal*.

Oct. 13, 1828. J. IVORY.

LV. *Remarks on the Influence of Terrestrial Radiation in determining the Site of Malaria. By* WM. ADDISON.

[Concluded from p. 278.]

ONE of the chief arguments in favour of the important influence exerted by terrestrial radiation in the production of that state of the atmosphere favourable to the attacks of disease, and known by the name of malaria, is drawn from the fact that in almost, nay I might say every case, where the violence of the symptoms induced by it will permit us to observe *the first impressions* which it causes, we find that its baneful influence is exerted during the night-season, while in the day-time it is comparatively, if not quite, inert. It would be needless to reiterate here the numerous proofs of this, distributed among the writings of those many accurate observers who have been at the pains of noticing the habitudes of malaria.

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* See Phil. Mag. and Annals for October 1827, pp. 245, 246, 247.

laria. I shall content myself, therefore, with quoting only Dr. Ferguson, who observes in his History of the Marsh Poison, "that the rarifying heat of the sun dispels the miasms which create fevers and violent diseases, and that it is only during the cooler temperature of the night, that they acquire body, concentration, and power."

Now surely any miasmatous effluvia liberated from exposed vegeto-animal or other matters by the rays of the sun, must exist in the atmosphere as much if not more during the day-season than in the night; for it is more than probable that nothing is *given up by the ground* after sun-set. How is it then, we may ask, that the great potency of malaria at night, and its comparative harmlessness during the day, have so constantly forced themselves upon our notice? Is it not because the air during the former period is cooled by radiation and rendered incapable of retaining those matters which the warmer air of the day-time held in perfect solution? A still atmosphere containing miasmatous matters, therefore, becomes dangerous to health in proportion as it reaches, by a gradual reduction of temperature, such as ensues from radiation, its dew point: for during the period when its temperature is elevated above this point, the malarious matter is without any, or of but little injurious agency; while the nearer it approaches the point at which moisture will be liberated from it, the more those extraneous matters it may contain become developed, as is fully shown by the much greater potency of odours at that period, of which numerous instances might here be mentioned; but it will be sufficient to recall what every one must have observed during the summer months: after a hot day, if the air at night remains still, or is favourable to the process of radiation, it is truly astonishing how far odours will diffuse themselves, and how powerful they generally are: a few hours after sun-set, on evenings favourable to the deposition of dew, many effluvia become very perceptible, and are potent and concentrated in proportion to the stillness of the air and its approach to the *dew point.* Winds, although they very often cause considerable reductions of temperature, are not so prejudicial, or so frequently productive of ill effects upon the human body, as those abstractions of caloric resulting from radiation; and for this reason,—because in the former instance the morbific particles are dispersed, and so diluted by the aërial currents as to be rendered incapable of exercising any injurious influence upon the body, or only upon such as are rendered extremely sensible to the exciting causes of disease; whereas in the latter instance they become often greatly accumulated, and so highly prejudicial, that few escape.

In this country the pernicious nature of the morning and evening mists formed over low grounds has been observed, and in hotter climates I need scarcely say that their influence in generating fever is as notorious as any of the best established facts on this subject; and the progress of the sun upwards being a remedy for the morning mists, and the day altogether for those of the night, seems to confirm the opinion, that a watery and moist atmosphere is the active conductor or repository of malaria; and that when the former is dissipated, the latter is checked in its progress; when the one is entirely dispersed, the other may be destroyed: so that the matter of malaria seems to be defined as to its place and extent by vapour and mist*.

That the diseases arising from miasmata in the air do sometimes cease in a definite and sudden line, and terminate also at particular altitudes, has often been observed and recorded; and these remarkable instances cannot be satisfactorily explained upon any other supposition than that afforded by the *radiation of caloric.* To explain their cessation in the former instance, we may remark, that that depression of temperature which ensues at night over a good radiating surface, may be sufficient to render active the miasms existing in the air; while over others, less powerful in the dissipation of caloric, the depression of temperature may not be sufficient; and it is probable that in many cases an atmosphere rendered prejudicial by the one, is again made innoxious by passing over the other. With respect to altitude, I have before shown that slight elevations are frequently a protection against the heavy miasmatous air which subsides to the lowest situations.

But to place this important subject in the clearest possible light, let me endeavour (by an appeal to some well-known chemical facts) to set forth the nature of the connection existing between free caloric and the matter of malaria. Let us suppose that the former exerts over the latter an influence analogous to that exercised by an *acid* over an *alkali* (neutralizing its qualities and destroying its effects), and we shall immediately perceive that the mere presence of malarious matters in the air may not be sufficient to excite in the human body a state of disorder or disease: carry the reasoning a little further, and then we can fully understand the way in which *radiation* proves injurious. Are we not warranted in concluding, from those facts which observation and experience have discovered to us, that similar phænomena are exhibited in the relations subsisting between the matter of heat and miasmatous effluvia, as we witness among the various combinations of the chemical world? Withdraw one of the elements of a binary compound, and the other becomes immediately apparent, and is developed with all those potent qualities which had been destroyed or neutralized whilst in union. So miasmatous matters are inert while fixed to the ground, from which they can arise only in conjunction with caloric; and as long as they continue together no ill effects ensue: but diminish the temperature, or, in other words, take away the caloric, and the injurious qualities of the miasms immediately become apparent. It may be objected, that if the injurious agency of miasmata in the air results from the mere abstraction of heat, no reduction of temperature could ensue without the production of malaria.—But this is not true; for we may justly suppose that in a great majority of cases there is not sufficient noxious matter on the ground to saturate—if I may be allowed the term—the caloric existing in the air, and therefore that in these instances great reductions of temperature may take place without any appearance of malaria, in the same manner as (to carry on the analogy drawn from chemical combinations) we can detach a portion of the acid from a supersaturated salt, without developing the existence or qualities of the alkali. On the other hand, the miasmatous source may sometimes afford a supply amply sufficient to satisfy even a very high temperature; and then any trifling escape of caloric will be accompanied with an injurious precipitation: and if the cooling process continues, a highly noxious malaria will result.

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* *Vide* Macculloch's Essay, pp. 259 and 274.

It has been observed, that very often the diseases arising from malaria ensue upon the temperature of a place reaching a certain point; that they increase in frequency and violence as the heat increases, but diminish as the mean temperature falls. These facts are not at all irreconcileable with the phænomena of *radiation;* for in these cases we may justly suppose, that at the higher temperatures malarious matter is liberated from the soil, the quantity of which is greater in proportion to the thermometric rise, while the lower temperatures are not sufficient to liberate any quantity of the noxious effluvium and diffuse it through the air: in the former case the radiation of caloric will be attended with disease, in the latter it will not.

I might here relate many facts tending to show the intimate connection which subsists between caloric and miasmatous effluvia, but I conceive that what has been here stated will be ample to establish this point, as well as the fact that the latter become virulent in proportion to the abstraction of the former by the process of radiation.

In conclusion, I shall briefly point out the importance of the foregoing observations, if they shall be found correct, towards the attainment of that desirable end, *the protection of mankind against the injurious impregnations of the air.*

As regards the prevention of the rise of miasms from the ground, I fear we have too little controul over the powerful agency of the rays of the sun to adopt successfully any plan with reference to this head. The solar influence is too great and too general to enable us to obstruct the emanation of various effluvia from the soil: nevertheless, much may be done by removing as far as possible from the surface of the ground any thing likely to afford them; and although our endeavours on this point must be very inefficient, they may be more successful and beneficial if directed to obviate those conditions which, as we have seen, have such a considerable effect in rendering active the noxious properties of malaria; viz. 1st, by preventing the dissipation of caloric through a still atmosphere; and, 2ndly, by promoting those aërial currents which tend so much to dilute and carry off any deposition which may ensue from that process.

In order to accomplish the former of these indications, we must use every means in our power to diminish the radiation of heat from the ground after sun-set, or to remove as far as possible from the circle of its operation, by attaining during the night-season some moderate elevation, interspersed here and there with lofty trees, and hedges or inclosures, and placed to windward of the more rapidly radiating surfaces which may be near: for although we speak of a calm and still atmosphere as being highly favourable to the development of malaria, still it must be understood that in almost every instance there are gentle, although perhaps imperceptible currents in the air, fully sufficient to waft to a considerable distance the miasms liberated by the dissipation of caloric; and any increase of temperature which such currents may acquire in their passage over less perfect radiators, will not always be enough to disarm them of their injurious influence. In situations therefore more particularly, where we are likely to be subjected to miasmatous products, and where the air at night is generally still, or where the gentle breezes are found to sweep over tracts favourable to radiation, it behoves us to endeavour,—by exciting artificially aërial currents, and by raising or keeping up the temperature of the air of the place where we may be by circumstances constrained to remain,—to prevent the deposition and development of malaria. This may be accomplished by lighting *large fires* to windward of the place of our nightly sojourn.—This is not a new idea: fires have already been observed to be beneficial in warding off the nocent power of malaria, though the principle upon which they act has not been properly understood, and consequently they have never been employed to the best advantage for this purpose. Dr. Macculloch relates a very important case, where a superintendant engaged in directing the cutting of wood in Africa, erected thirty earthen furnaces on the spot where his men were employed, lighting them every day. Before this, he had always from forty to forty-eight of his workmen sick; when in a short time they were reduced to twelve, then to four, and finally to one. Napoleon adopted the same expedient very largely, and with success, when his armies were occupied in the very worst district of Italy*. Knowing the principle of their operation, I should recommend them to be lighted at sun-set, and to be allowed to burn until sun-rise, having a regard to their position as pointed out in the foregoing remarks. Where large numbers of human beings are congregated together, as in armies, camps, &c., and where their situation at night is too often determined by other circumstances than salubrity, the value of these observations, with the knowledge of the principles which should direct their application, cannot but be very apparent.

It will be easily seen, from what has already been said, that fires as defences against malaria will be much more necessary during the nocturnal period than at any other; and even at this season, when the wind is blowing strongly and the night is overcast, they will not be so much required as when the air is clear and still.—It is not my intention to speak here of those various extraneous circumstances which render the body more susceptible of injurious influences at night than during the day,—such as bodily and mental exhaustion, sleep and diminished temperature; nevertheless they are well worthy of our serious regard, as cooperating powerfully with noxious miasmata in producing a state of disease.

Malvern, July 1, 1828. WILLIAM ADDISON.

LVI. *An Account of the Formation of Alcoates, Definite Compounds of Salts and Alcohol analogous to the Hydrates.* By THOMAS GRAHAM, *Esq. M.A. F.R.S.E.*

[Concluded from page 272.]

II. *Alcoate of Nitrate of Magnesia.*

IT is difficult to expel the whole of the water with which nitrate of magnesia is combined, without driving off a portion of the acid, and decomposing the salt. For this salt may be wholly reduced [to magnesia] in a glass-tube by the heat of a spirit-lamp, and yet a sand-bath heat of 600° or 700° is not sufficient to drive off all its water of crystallization. But a partial decomposition of this salt is of no great consequence, as alcohol dissolves the undecomposed portion of the salt, while the magnesia resulting from the decomposition precipitates, and may be separated by decanting the solution, or by filtering.

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* Macculloch's Essay, p. 286.

Four parts alcohol at 60° dissolve one part nitrate of magnesia, and boiling alcohol dissolves more than half its weight of this salt. From the great difference between the solubility of this salt at high and low temperatures, the alcoate is obtained with facility. A hot solution, containing a greater proportion of nitrate than one part to three parts alcohol, became, upon cooling, an irregular dry mass, which could be indented by the point of a glass-rod, but was much harder than the alcoate of chloride of calcium. In solutions considerably weaker crystals were deposited on cooling, which sometimes resembled the crystals of the former alcoate, but were much smaller, and less distinct; but more frequently, the crystals were exceedingly minute, and detached, without any regular form which could be discerned. But the great mass of crystalline matter precipitated in scales of a pearly lustre and whiteness, but apparently made up of the small crystals.

Dried by pressure, in blotting paper, this alcoate much resembled the alcoate of chloride of calcium in external characters. It sank in water, but floated on the surface of a saline solution of the specific gravity 1·1. Heated, it melted readily; boiled, and much alcohol was given off. When boiled violently, red fumes rise with the alcohōl-vapour; but when dried slowly, no loss of acid takes place.

Upon cautiously heating 13·4 grains alcoate of nitrate of magnesia to dryness, there remained 3·56 grains nitrate of magnesia. This gives 9·84 alcohol to 3·56 nitrate of magnesia. But the atomic weight of anhydrous nitrate of magnesia is 9·25. Now, 3·56 : 9·84 : : 9·25 : 25·57.

In another case, 16 grains alcoate were reduced to 4·2 grains. This gives 11·8 grains alcohol to 4·2 grains nitrate of magnesia.

4·2 : 11·8 : : 9·25 : 25·99.

On the supposition that this alcoate consists of one atom nitrate of magnesia united with nine atoms alcohol, the alcohol should amount to 25·875, a number intermediate between the two results. This alcoate will be thus represented:

One atom nitrate of magnesia	9·25
Nine atoms alcohol	25·875
	35·125

III. *Alcoate*

III. *Alcoate of the Nitrate of Lime.*

Nitrate of lime may be obtained anhydrous with much greater facility than nitrate of magnesia, as, after being dried on the sand-bath, it may be heated in a glass-capsule by the spirit-lamp without decomposition, although it partially fuses. Boiling alcohol saturated with this salt formed a solution, which became very viscid on cooling, and remained without crystallizing for a whole day. But during a frosty night it was resolved into an amorphous solid, slightly moist, but without any appearance of crystallization. This substance was carefully dried in the usual way.

14·8 grains were reduced by heat to 8·8 grains. This gives 6 grains alcohol to 8·8 grains nitrate of lime. The atomic weight of anhydrous nitrate of lime is 10·25. Now,

8·8 : 6 : : 10·25 : 6·98.

In another case, 15·6 grains were reduced to 9·2, which gives 6·4 alcohol to 9·2 nitrate of lime. But,

9·2 : 6·4 : : 10·25 : 7·13.

This approaches 7·1875, or two and a half equivalent proportions of alcohol. The composition of the alcoate of nitrate of lime would be represented on this view, by

Two atoms nitrate of lime............	20·5
Five atoms alcohol....................	14·375
	34·875

In another strong alcoholic solution of nitrate of lime, a few irregular crystals were deposited; but the quantity was not sufficient to admit of examination, although they proved that this alcoate is capable of crystallizing.

IV. *Alcoate of Protochloride of Manganese.*

The protochloride of manganese, dried in a glass-tube, at a red heat, was light, friable, and of a reddish colour. Alcohol dissolved a very large quantity of it. When the solution was made at a high temperature, the alcoate crystallized readily upon cooling in plates with ragged edges. 14·6 grains of this alcoate, carefully dried by pressure in blotting paper, were reduced by heat to 7 grains. The alcoate, therefore, consisted of 7 grains protochloride of manganese, and 7·6 grains alcohol. The atomic weight of protochloride of manganese is 8. Now,

7 : 7·6 : : 8 : 8·686.

This slightly exceeds three atoms alcohol = 8·625, but the approximation to the theoretical number is as close as could be expected. The composition of this alcoate may therefore be expressed by

One atom protochloride of manganese	8·
Three atoms alcohol	8·625
	16·625.

V. *Alcoate of Chloride of Zinc.*

Alcohol dissolves chloride of zinc with great facility, and the solution when filtered is of a light amber colour. This solution may be concentrated to a very great extent without injury, and becomes so viscid when cold, that it may be inverted without flowing perceptibly. It is not till so concentrated that it begins to deposit crystals, which are small and independent, but apparently of no regular shape. A viscid solution, in which crystals formed, was found to be composed of 20 parts chloride of zinc, and 7 parts alcohol. The small proportion of alcohol is astonishing; yet no more alcohol was given out when the chloride was heated nearly to redness, and began to volatilize; nor did a portion of the chloride thus heated take fire when exposed directly to the flame of a candle.

The crystalline matter was dried with difficulty by pressure in blotting paper. When dry, it possessed the usual waxy softness of the alcoates, and was of a yellowish colour. Heated, it entered into a state of semifusion, and gave off its alcohol. Nine grains alcoate were reduced by the application of sufficient heat to 7·65 grains. Hence the alcoate consisted of 7·65 chloride of zinc, and 1·35 alcohol. But the atomic weight of chloride of zinc is 8·75.

7·65 : 1·35 : : 8·75 : 1·544.

1·544 slightly exceeds 1·4375, or half an atomic proportion of alcohol. It is probable that the excess was owing to the difficulty of freeing the alcoate completely from the viscid solution. According to this view, the alcoate of zinc consists of

Two atoms chloride of zinc.........	17·5
One atom alcohol	2·875
	20·375

Besides these alcoates, similar compounds of chloride of magnesium and of protochloride of iron and alcohol were formed, although in quantities too minute to enable me to ascertain their proportions. Alcohol is retained with great force by chloride of iron, and is partially decomposed when heated, as is the case with many metallic chlorides.

As I had it only in my power to present the fixed alkalies to absolute alcohol in the state of hydrates, no alcoate appeared to be formed. The same was the case with the vegetable acids soluble in alcohol.

It is probable that many more alcoates of salts may be formed, particularly of the metallic chlorides. The great obstacle to their formation is the difficulty, and frequently the impossibility, of rendering the salts perfectly anhydrous, before their solution in alcohol is attempted.

I am not aware of any other compounds in the solid form of the same class as the hydrates and alcoates. But there is an oxide, classed by Dr. Thomson in his System of Chemistry, with water and other neutral and unsalifiable oxides, the habitudes of which with certain salts are exceedingly remarkable, and have been looked upon as anomalous, but on which the established properties of hydrates and alcoates appear to me to throw some light. I refer to the deutoxide of azote or nitrous gas. 100 volumes pure water are capable of absorbing only 5 volumes of this gas, according to the experiments of Dr. Henry. But Dr. Priestley and Sir H. Davy ascertained that certain metallic salts, particularly the protosalts of iron, are capable of absorbing this gas in large quantities; and again emit the greater part of it unaltered, on being heated. That the absorption of deutoxide of azote by these salts, is not dependent upon the oxygen of their bases, or the water which they contain, I have proved in two ways, in the case of protomuriate of iron. By heating this salt to redness in a glass-tube, it is reduced to the state of protochloride of iron. Now, I find that this chloride in the dry state absorbs deutoxide of azote, although in a comparatively small proportion. And the alcoholic solution of the chloride, where neither oxygen nor water interferes, appears to exceed the aqueous solution of the protomuriate in its capacity for deutoxide of azote.

Deutoxide of azote, formed by the action of dilute nitric acid on copper, was conducted into a globular receiver surrounded by cold water, and thence through a glass-tube of two feet in length, filled with small fragments of chloride of calcium.* Thus dried, the deutoxide of azote was passed slowly over carefully prepared protochloride of iron in the state of powder, and contained in a glass-tube of small diameter. The protochloride immediately became darker in colour; and upon being withdrawn, after exposure to the current of gas for some time, was found to retain the smell of nitrous gas, and to have increased in weight. In one case, 30 grains chloride had increased to 31·1 grains; and in another case, 25 grains chloride to 25·5 grains. On being gently heated, the deutoxide of azote was evolved, and the chloride restored to its former colour.

The solution of protochloride of iron in absolute alcohol, absorbed a much greater quantity of deutoxide of azote, and became nearly black. A solution saturated with gas began to boil at 100°, evolving gas in great abundance, which, being collected in the pneumatic trough, proved to be pure deutoxide of azote. The greater part of the gas was expelled before the alcohol rose to its boiling point, and after the solution was in the state of ebullition for a few seconds gas ceased to rise, and the alcoholic solution recovered its original colour, which was generally a chocolate-brown, from the presence of a little bichloride of iron. The quantity of gas evolved from a solution of one part protochloride of iron in five parts absolute alcohol, amounted to 23 times the volume of the alcohol.

I think it probable that the absorption of deutoxide of azote by protochloride of iron, is analogous to the absorption of alcoholic and aqueous vapours by the same body. For I find that protochloride of iron absorbs alcohol-vapour as well as the vapour of water. The absorption of deutoxide of azote may depend upon a tendency of chloride of iron to *deliquesce* in like manner, in an atmosphere of that neutral oxide. At a very low temperature, which it is perhaps out of our power to reach, protochloride of iron would probably absorb this gas in sufficient quantity to exhibit the appearance of deliquescence, and might form with it a neutral compound similar to its alcoate or hydrate.

A reason can also be given for the superiority of the aqueous and alcoholic solutions of this chloride over the dry chloride itself, in absorbing deutoxide of azote. We formerly saw that the alcohol of the alcoate of chloride of calcium was completely expelled by a heat of 250°, when no water was present, but that, when a considerable quantity of water was present, alcohol was retained by that chloride at the temperature of 400° or 500°. Now, chloride of iron might be enabled to retain deutoxide of azote more powerfully, by the assistance of alcohol or water, in the same manner. But the *retaining* power we have formerly found in a similar case to be an index of the *absorbing* power. Hence solutions of protochloride of iron might absorb deutoxide of azote more powerfully than the chloride itself.

LVII. *On the Luminous Zone observed in the Heavens on the 29th of September last. By Capt.* H. Kater, *V.P.R.S.*

To Mr. Taylor.

Sir,

THE substance of the following communication was inserted in the Times newspaper of the 4th of October; but as it seems desirable that phænomena of this kind should be permanently recorded, I shall feel obliged by your giving it a place in the Philosophical Magazine.

On the 29th of September last, Professor Moll and myself, being at Chesfield Lodge near Stevenage, observed at 8^h^ 35^m^ mean time, a zone or luminous belt extending itself in the heavens from the eastern to the western horizon. The light of the zone was white, uniform or nearly so, and surpassing much in intensity that of the milky-way. Its breadth (nearly equal throughout) was about three-fourths of the distance from β to γ Aquilæ, or 3° 45′. The edges of the belt appeared perfectly well defined and equally luminous with the middle, and its transparency was such that the stars were distinctly seen through it.

The observations made at the moment were, that the belt covered the Pleïades, and appeared to be equally distant from α Arietis and γ Andromedæ. It passed between α Aquilæ and α Lyræ, at the distance from α Aquilæ of one-third or two-fifths of the interval between these stars. Professor Moll observed that its edges were upon β and γ Ophiuchi. Lower down, near the western horizon, this luminou szone suffered a very remarkable inflexion towards the north, and soon after was lost in the clouds at a little distance above the horizon. On tracing the course of this phænomenon upon a celestial globe, its path appears to have been nearly that of a great circle, meeting the horizon about the E.N.E. and W. by S. points. The altitude of the centre of the most elevated part appears to have been about 72°, so that it must have been nearly in the plane of the dipping-needle, and nearly at right angles to the magnetic meridian.

At 8^h^ 42^m^ mean time, the belt began to fade slowly from the east towards the west, and at 9^h^ 22^m^ no trace of it was perceptible. Its light during the whole time appeared perfectly steady and without any coruscations.

There was much wind from the S.E. The stars were unusually bright. The height of the barometer was 29·12 inches, and the thermometer 59°.

It may not be uninteresting to add, that a gentleman remarked that the setting sun had a very unusual appearance, being, as he described it, of a pale dirty yellow: and a letter which I have received from Cromer in Norfolk, mentions on that evening "a very beautiful setting sun, quite out of the common way;" but it is not stated in what its peculiarity consisted.

The latitude of Chesfield Lodge is 51° 56′ 15″, and its longitude about 43 seconds in time west from Greenwich.

I am, Sir, your obedient servant,

Chesfield Lodge, Oct. 14, 1828. HENRY KATER.

LVIII. *Expression for the time of Vibration of a simple Pendulum in a Circular Arc.* *By* A CORRESPONDENT.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

I TAKE the liberty of inclosing you an expression for the time of vibration in a circular arc, which does not appear to have been noticed by any writer on Dynamics.

I am, Gentlemen, your humble servant,

J. W. L.

Let ψ be the angle which the pendulum makes with the vertical, α the arc through which it moves, r the radius of the circle, g the force of gravity, t the time; then

$$dt = \sqrt{\frac{r}{2g}}\,\frac{d\psi}{\sqrt{\cos\psi - \cos a}},$$

Let $1 - \cos\psi = (1 - \cos\alpha)\sin^2\sigma$

$$dt = \sqrt{\frac{r}{g}}\,\frac{d\sigma}{\sqrt{1 - \frac{(1-\cos\alpha)}{2}\sin^2\sigma}} = \sqrt{\frac{r}{g}}\,\frac{d\sigma}{\sqrt{1 - \sin^2\frac{\alpha}{2}\sin^2\sigma}}$$

$$= \sqrt{\frac{r}{g}}\,\frac{d\sigma}{\sqrt{\sin^4\frac{\alpha}{4} + \cos^4\frac{\alpha}{4} + 2\sin^2\frac{\alpha}{4}\cos^2\frac{\alpha}{4}\cos 2\sigma}}$$

$$= \frac{1}{\cos^2\frac{\alpha}{4}}\sqrt{\frac{r}{g}}\,\frac{d\sigma}{\sqrt{1 + \tan^4\frac{\alpha}{4} + 2\tan^2\frac{\alpha}{4}\cos 2\sigma}}$$

$$\text{or,}\quad \frac{1}{\sin^2\frac{\alpha}{4}}\sqrt{\frac{r}{g}}\,\frac{d\sigma}{\sqrt{1 + \cot^4\frac{\alpha}{4} + 2\cot^2\frac{\alpha}{4}\cos 2\sigma}}$$

Expanding these series by the known methods, and integrating from $\sigma = -\frac{\pi}{2}$ to $\sigma = +\frac{\pi}{2}$, the time of an oscillation is given by either of the following series:—

$$\frac{\pi}{\cos^2\frac{\alpha}{4}}\sqrt{\frac{r}{g}}\left\{1+\left(\frac{1}{2}\right)^2\text{tang}^4\frac{\alpha}{4}+\left(\frac{1}{2}\cdot\frac{3}{4}\right)^2\tan^8\frac{\alpha}{4}\right.$$
$$\left.+\left(\frac{1.3.5}{2.4.6}\right)^2\tan^{16}\frac{\alpha}{4}+\&c.\right.$$

$$\text{or, }\frac{\pi}{\sin^2\frac{\alpha}{4}}\sqrt{\frac{r}{g}}\left\{1+\left(\frac{1}{2}\right)^2\cot^4\frac{\alpha}{4}+\left(\frac{1}{2}\cdot\frac{3}{4}\right)^2\cot^8\frac{\alpha}{4}\right.$$
$$\left.+\left(\frac{1.3.5}{2.4.6}\right)^2\cot^{16}\frac{\alpha}{4}+\&c.\right.$$

LIX. *A Letter to* William Morgan, *Esq. F.R.S. on the Experience of the Equitable Society. By* A CORRESPONDENT.

Dear Sir,

HAVING unfortunately failed, on some former occasions, of fully comprehending the meaning of your expressions, I earnestly intreat your attention to a few remarks on your late statement of the Experience of the Equitable Society, in order that you may correct, if possible, the exaggerated conclusions which appear to me to be fairly deducible from the numbers that you have published; for I have no doubt that you will unite with me in sincerely deprecating the dangerous consequences, that would result from the hasty adoption of these conclusions in the practice of life assurance, although they may still be very useful as cautions deserving the attention of the granters of annuities.

Your table, lately published, stands thus:

Age.	Number.	Died [annually].	That is, One in *
20 to 30	4720	29	163 = 188 − 25
30 to 40	15951	106	150 = 185 − 35
40 to 50	27072	201	135 = 180 − 45
50 to 60	23307	339	69 = 124 − 55
60 to 70	14705	436	34 = 99 − 65
70 to 80	5056	219	17 = 92 − 75
80 to 95	701	99	7 = 94 − 87

[View of the Rise and Progress of the Equitable Society, 8vo 1828. p. 42.]

1. I have first to observe, that the numbers of the column marked with an asterisc, which vary from 188 to 92, ought all, according to Halley's earliest hypothesis, to be 100; and according to Demoivre's correction of that hypothesis, to be

86 only. The mean of the numbers here computed is 137: and nearly in the proportion of 86 or 87 to 137 does the expectation of life, as exhibited by this table, exceed the estimate of the Northampton table: a result not materially differing from the proportion of 2 to 3, which you assign.

2. But a more remarkable peculiarity of the decrements, or rather the decremental quotients, derived from your table, is the regularity which is observable in their progress after the period of middle life; each of the numbers, which express them, being precisely or very nearly the half of the preceding number. Thus, disregarding fractions, we have $\frac{135}{2} = 67$ for 69, $\frac{69}{2} = 34$, $\frac{34}{2} = 17$, and $\frac{17}{2} = 8$ for 85, which is equivalent to 7 at 87½.

3. We may therefore continue this series with perfect confidence, until the whole number of lives is exhausted, taking the annual decrement at 95,¼, at 105,½, and at 115,1, which may be supposed to be a sufficient age for the termination of our computations.

4. The decremental quotient in your table, $-\frac{\Delta z}{z}$, is very nearly $\frac{1}{2^y}$; y being $\frac{115-x}{10}$, x the age, and z the number living: for this expression gives us, from 25 to 85, (512, 256,) 128, 64, 32, 16, 8: and if we wish to modify the formula, we may make it more generally $\frac{-\Delta z}{z} = a^y$, and $y = b - cx$; so that the computation might be adapted to the earlier ages, if we had sufficient documents for the purpose.

5. We might at once form a table of mortality from the quotients thus computed, proceeding downwards from a single life at the age of 115: but it will be much more convenient, and perhaps equally accurate, to employ the method of fluxions.

6. Since $\frac{-\Delta z}{z} = a^y \Delta x$, Δx being $= 1$, $y = b - cx$, and $\Delta y = -c\Delta x$, we have $-\frac{\Delta z}{z} = a^y . \frac{\Delta y}{-c}$; whence, substituting, as usual, $\frac{\mathrm{d}z}{\mathrm{d}y}$ for $\frac{\Delta z}{\Delta y}$, we have the equation $\frac{\mathrm{d}z}{z} = a^y . \frac{\mathrm{d}y}{c}$, of which the fluent is $\mathrm{hl}\,z = a^y . \frac{1}{c\,\mathrm{hl}\,a} + f$; which becomes, for the values $a = \frac{1}{2}$ and $c = \frac{1}{10}$, $f - \frac{10}{2^y . \mathrm{hl}\,z}$, or $\mathrm{hl}\,z = f - \frac{14\cdot42695}{2^y}$: which, when y is 11·5, becomes $f -$ ·0049926; and if we suppose the number born to be 100,000, 11·5129254 $= f -$ ·0049926,

and $f = 11{\cdot}5179180$, and $\mathrm{hl}\,z = 11{\cdot}517918 - \frac{14{\cdot}42695}{z^y}$, y being $= \frac{115-h}{10}$. When $y = 0$ and $z^y = 1$, $\mathrm{hl}\,z = -2{\cdot}90903 = \mathrm{hl}\,\frac{1}{11}$, so that about one in a million only would survive at 115.

7. It is obvious that, according to this formula, the value of z can never become wholly extinct, and that a population may be imagined great enough to have an individual living at any given age: but notwithstanding Mr. Gompertz's ingenious speculations on patriarchal longevity, it can scarcely be admitted that the analogy is sufficiently strong to justify such a conclusion respecting more modern times; to say nothing of the population of the whole world as a limit which would require to be considered.

8. The results of the formula are exhibited in the following table, in which they are compared with Mr. Babbage's table of the Equitable Experience, with the Carlisle table, and with the table published in the Philosophical Transactions for 1826.

Age.	Living.	B × 2	C × 2	Y × $\frac{3}{10}$	N × [illegible]
(0	10000)				
(15	9900)				
(25	9761)				
(35	9490)				
45	8970	9612	9454	10535	10827
50	8561	8910	8794	9263	9523
55	8014	8060	8146	7928	8160
60	7299	7084	7286	6543	6793
65	6396	6048	6036	5130	5440
70	5310	4974	4802	3733	4107
75	4075	3876	3350	2432	2773
80	2808	2714	1906	1336	1563
85	1654	1302	890	571	620
90	785	340	284	177	153
95	26	40	60	31	13
100	6·1	0	18	7	0
105	·7		0	1	
110	·04			·3	
115	·01			0	

9. I am at a loss to understand how you will be able to reconcile the numbers of the first column of this table, with the opinion that "the experience of the Equitable Office confirms the accuracy of the Northampton table," which is represented by the fourth column, on the supposition that a given number of individuals about 55 is to be compared. From this age, and as far as 85, the first column certainly represents

the numbers of your table, if I have not mistaken their import; but the formula may readily be made to extend with equal accuracy to ages somewhat above this limit, as well as to an earlier period. We may take, for example, 105 for the age at which the decremental quotient, indicating the *rate* of mortality, becomes $= 1$, and make it 150 at 35; it must then be $\sqrt{150}$ at the intermediate age of 70; and supposing $a^{70} = \frac{1}{150}$, $\frac{1}{a} = 1{\cdot}0742$, b being $= 105$, and $c = 1$, and the fluent becomes $= f - \frac{1}{hl\,1{\cdot}0742} \cdot \frac{1}{a^y}$, y being $105 - x$, or $f - \frac{13{\cdot}974}{a^y}$: and $\log a = {\cdot}031087$, and for 10000 at birth, $f = 9{\cdot}218$; which gives, at 95, 11, instead of 26, and at 55, 7207, a number nearer to the truth than the former.

10. If, happily for the welfare of mankind, it should hereafter appear that any firm reliance ought to be placed on these conclusions, or if the formula could be any otherwise modified so as to serve for the purposes of calculation, it might be made to afford essential assistance in determining the values of two or more joint lives; and by means of a proper table of fluents, the labour would be little greater for combinations of lives than for single ones, since the sums of the fluents would represent the products of the quantities to be combined; and a single table might be computed, which would render the integration of the fluent of $e^{a^y}\, \dot{y}$ a matter of little difficulty. But such an improvement would at present be premature.

While results like these, however, are fairly deducible from the face of the evidence that you have laid before the public, you must allow, my dear sir, that any government granting annuities would be highly culpable in reckoning on values of human life like those which are represented by the Northampton tables; and that any private office has a right to expect, beyond such a valuation, a fair percentage for the payment of their unavoidable expenses. On the other hand, I do not see how it is possible for any assurance office, not returning a large share of their profits, to satisfy the public that their terms are reasonable, without acting most improvidently for their own interests. Such offices as the Equitable are exempt from these objections; and I have not the least doubt of the judgement and integrity with which you have long conducted the business of that Society, nor of the impropriety of calling on a private body to adopt any other regulations than those which are approved by its members. But, as a man of science, it is natural to hope that you will be ready to allow other men of science to partake in the fruits of your researches, and that you will be desirous of vindicating yourself from all possible suspicion of ambiguity and of inconsistency.

I am, dear sir, with great respect, yours, &c.

Waterloo Place, Oct. 13, 1828. * * * *.

LX. *An Abstract of the Characters of* Ochsenheimer's *Genera of the Lepidoptera of Europe; with a List of the Species of each Genus, and Reference to one or more of their respective Icones.* *By* J. G. CHILDREN, *F.R.S. L. & E. F.L.S. &c.*

[Continued from p. 287.]

Genus 9. LYCÆNA, *Fab.*

THECLA et HESPERIA, Fab. POLYOMMATUS, Latr.
ARGYREUS, ARGUS, PTEROURUS, BATTUS et GRAPHIUM, Scop.
CUPIDO, Schrank. RUSTICI, Hübn.

FAM. A.—*Legs*, first pair shorter than the rest.

Wings, upper surface generally blue, especially in the males; in the females often brown, with a row of reddish-yellow spots near the exterior margin; under surface almost constantly grayish, with numerous ocelli with black pupils surrounded by white irides*.

Antennæ filiform, terminated by an elongated, compressed club.

Larva onisciform; head black, and, as well as the feet, very small, and scarcely perceptible; the body laciniate as usual, the back elevated, and generally beautifully coloured.

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* Latreille at first subdivided his genus *Polyommatus* into three great groups,—*les petits-porte-queue, les argus*, and *les bronzés*; and subsequently he established several smaller sections in each of those subdivisions. Mr. Stephens has arranged the British *Lycænidæ* in the three genera *Thecla, Lycæna*, and *Polyommatus*, including in the first, those insects "distinguished by the sombre tints of the upper surface of their wings, and the pale streaks with which they are adorned below; by the pubescence of the eyes, the abbreviated, triangular anterior wings, and the ovate-triangular posterior ones, which are usually furnished near the anal angle with one or more short linear tails, or are strongly denticulated on that part."—"The indigenous *Lycænæ* are known by the brilliant coppery tints which adorn the greater portion of the disc of the upper surface of the wings;" and though considerable diversity of habit and form prevails amongst the *Polyommati*, they are in general distinguished by the rich blue tints with which their wings are enlivened.

The *Lycænæ*, he adds, are further discriminated from the *Theclæ* by their naked eyes, and by the want of the tail-like appendages to their posterior wings; and from the *Polyommati*, by the abrupt obtuse club of the antennæ, the more evidently denticulated posterior wings, and the superior size of the pulvilli, or foot-cushions.—The antennæ of Stephens's *Polyommati* have an abrupt compressed club, terminating in a lateral point; those of the *Theclæ* have the club elongate, cylindric-oval.—See Illustr. of Brit. Entom. vol. i. (HAUSTELLATA), p. 75—83.

Pupa rather long, naked; colour whitish, with some dusky spots on the back and side.

Metamorphosis usually on the stem of a plant; rarely under the surface of the ground.

	Species.	Icon.
a.	No transverse reddish-yellow fascia on the under surface of the posterior wings.	
1.	L. *Arion*, Linn.	Ernst, I. Pl. XLI. f. 36. d. e. f.
2.	— *Alcon*, Fab.	Ernst, I. Pl. XLI. f. 86. i. k. Pl. LXXXIII. Suppl. II. Pl. 4. f. 80. a.—d. tert.
3.	— *Iolas*, Ochs.*	
4.	— *Euphemus*, Hübn.	Ernst, I. Pl. XLI. f. 36. g. h.
5.	— *Erebus*, Fab.......	Ernst, I. Pl. XL. f. 86. a—c.
6.	*Cyllarus*, Fab.....	Ernst, I. Pl. XLI. f. 86. o.
7.	*Acis*, Ochs.	Ernst, I. Pl. XLII. f. 88. a—d.
8.	*Argiolus*, Linn....	Ernst, I. Pl. XLI. f. 86. l. m.
9.	*Damon*, Fab......	Ernst, I. Pl. XLII. f. 87. a—d.
10.	*Alsus*, Fab.	Ernst, I. Pl. XLII. f. 88. e. f.
11.	*Lysimon*, Hübn.	Hübn. Pap. Tab. 105. f. 534. 535. (mas.)
12.	— *Pheretes*, Hübn.	Hübn. Pap. Tab. 97. f. 495. 496. (mas.) Tab. 107. f. 548. 549. (fœm.)
13.	— *Daphnis*, Hübn.	Ernst, I. Pl. XXXVIII. f. 81. a. b.
b.	A transverse fascia of orange-coloured spots on the under side of the posterior wings, near the outer margin.	
14.	— *Corydon*, Fab.....	Ernst, I. Pl. XXXIX. f. 83. a—d.
15.	*Dorylas*, Hübn...	Ernst, I. Pl. LXXXIII. Suppl. II. Pl. IV. f. 82. a—d. bis.
16.	— *Adonis*, Fab.......	Ernst, I. Pl. XXXIX. f. 82. a—e.
17.	— *Icarius*, Esp.......	Hübn. Pap. Tab. LIX. f. 283. (mas.) 284. 285. (fœm.)
18.	— *Alexis*, Hübn.....	Ernst, I. Pl. XXXVIII. f. 80. g. h.
19.	— *Eros*, Ochs. ...... (*Tithonus*, Hübn.)	Hübn. Pap. Tab. 108. f. 555. 556. (mas.)
20.	— *Orbitulus*, Esp....	Hübn. Pap. Tab. 103. f. 522. 523. (mas.) 524. 525. (fœm.)
21.	*Agestis*, Hübn....	Hübn. Pap. Tab. 62. f. 303. 304. (mas.) 305. 306. (fœm.)

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* Sp. n.—L. alis integris cœruleis, maris immaculatis, fœminæ fuscis, disco cœruleo, subtùs cinereis, lunula media strigaque punctorum nigrorum ocellarium.

	Species.	Icon.
22.	L. *Eumedon*, Hübn..	Hübn. Pap. Tab. 62. f. 301. 302. (mas.) 138. f. 700. 701. (fœm.)
23.	— *Admetus*, Hübn...	Ernst, I. Pl. VI. Suppl. III. f. 80. a—d. quart.
24.	— *Optilete*, Hübn...	Ernst, I. Pl. LXXXIV. Suppl. II. Pl. 5. f. 85. a—c. tert.
25.	— *Argus*, Linn....... (var. *Acreon*, Fab.)	Hübn. Pap. Tab. 64. f. 316. (mas.) 317. 318. (fœm.)
26.	— *Aegon*, Hübn......	Hübn. Pap. Tab. 64. f. 313. (mas.) 314. 315. (fœm.)*
27.	— *Amyntas*, Fab. ...	Ernst, I. Pl. XXXVII. f. 78. a—d.
28.	— *Polysperchon*, Bergstraes.	Ernst, I. Pl. XXXVII. f. 79. a. b.
29.	— *Hylas*, Fab.	Ernst, I. Pl. XL. f. 85. e. f.
30.	— *Battus*, Fab.......	Ernst, I. Pl. LXXXIV. Suppl. II. Pl. 5. f. 85. a—c. bis.

Fam. B.—The upper surface of the wings usually of a reddish-gold, or copper colour, often with black maculæ; the under surface always spotted; the posterior wings with an orange-coloured plain fascia, or composed of a series of maculæ, near the posterior margin; anal extremity usually distinctly angular.

Larva, generally longer than those of the preceding family; usually pale green, and villose; hairs reddish; head light brown, or brownish-white.

Pupa brownish, usually obtuse at each end; suspended horizontally by threads attached to the neck and posterior extremity.

	Species.	Icon.
31.	L. *Helle*, Fab.	Ernst, I. Pl. LXXI. Suppl. XVII. f. 89. a—c. bis.

* Ochsenheimer also quotes, *inter alia*, (though with a note of doubt,) Lewin's Ins. pl. 39. f. 8. 9, as icons of his *L. Ægon*, which, according to Haworth, represent *Papilio* (*Lycæna*) *Artaxerxes*. Through the kindness of James Wilson, Esq. of Woodville, Canaan, near Edinburgh, and author of the beautiful Illustrations of Zoology now in course of publication, my cabinet is rich in specimens of that singularly local and rare insect, by comparing which with Ochsenheimer's specific characters of *L. Ægon*, it is obvious that he never saw the true *L. Artaxerxes*. I subjoin his sp. ch. of *L. Ægon*, and the very accurate one of *P. Artaxerxes*, as given by Mr. Haworth.

L. Aegon. Alis integris cœruleis margine lato nigro; subtùs cœrulescenti-albidis, punctis ocellaribus: posticis fascia ferruginea ocellisque cœruleo argenteis marginalibus.—*Ochs. Schm. von. Eur.* I. part 2. p. 57.

P. Artaxerxes. Alis nigris, anticis puncto medio utrinque albo, posticis lunulis rufis, subtus margine albo rufo punctato.—*Haw. Lep. Brit.* p. 47. No. 62.

	Species.	Icon.
32.	L. *Circe*, Hübn.......	Ernst, I. Pl. XLIII. f. 89. a—d.
33.	— *Thersamon*, Fab..	Hübn. Pap. Tab. 69. f. 346. (mas.) 347. 348. (fœm.)
34.	— *Gordius*, Hübn. .	Ernst, I. Pl. LXXII. Suppl. XVIII. f. 91. a. b. bis. Pl. LXXIII. Suppl. XIX. f. 91. c. d. bis.
35.	— *Hipponoë*, Esp...	Ernst, I. Pl. XLIV. f. 92. a. b. Pl. LXXII. Suppl. XVIII. f. 92. f. g.
36.	— *Chryseis*, Fab.....	Ernst, I. Pl. LXXIII. Suppl. XIX. .—g. bi
37.	*Eurybia*, Ochs....	Hübn. Pap. Tab. 68. f. 339. 340. (mas.) 341. 342. (fœm.)
38.	*Hippothoë*, Linn..	Ernst, I. Pl. XLIII. f. 91. c. d. Pl. XLIV. f. 93. a—c.†
39.	*Virgaureæ*, Linn.	Ernst, I. Pl. XLIV. f. 92. c—e.
40.	*Phleas*, Linn......	Ernst, I. Pl. XLIII. f. 91. a. b. Pl. LXXII. Suppl. XVIII. f. 91. e. g. h.
41.	*Ballus*, Fab.......	Hübn. Pap. Tab. 107. f. 550. (mas.) Tab. 72. f. 360. 361. (fœm.)
42.	— *Rubi*, Linn.	Ernst, I. Pl. XLIII. f. 90. a. b.

Fam. C.—The posterior wings subcaudate, with generally one or more reddish-yellow maculæ above the short tail; a white transverse fascia (more or less distinct) either simple, or composed of minute maculæ on the under surface of both wings.

Larva similar to those of Fam. A., but less elevated, and rather broad at the fore-part; back hairy; hairs very fine and short.

Pupa flat beneath; back very convex; generally attached to a leaf by a web, and filaments across the back.

	Species.	Icon.
43.	L. *Roboris*, Esp......	Hübn. Pap. Tab. 73. f. 366. 367. (fœm.)
44.	*Quercus*, Linn.	Ernst, I. Pl. XXXV. f. 71. a—c.
45.	*Bœticus*, Linn.	Ernst, I. Pl. XXXVII. f. 76. a. b. Pl. LXXI. Suppl. XVII. f. 76. c.
46.	— *Telicanus*, Hübn.	Hübn. Pap. Tab. 74. f. 371. 372. (mas.) Tab. 108. f. 553. 554. (fœm.)

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† 38*. L. *Dispar*, Haw. .... Curtis, Brit. Ent. I. Pl. 12. ♂ & ♀.

Mr. Stephens observes, that this species may eventually prove to be the same as *L. Hippothoe*. Ochsenheimer has omitted it altogether.

Species.	Icon.
47. L. *Spini*, Fab.........	Ernst, I. Pl. XXXVI. f. 74. a. b.
48. — *Ilicis*, Hübn......	Ernst. I. Pl. XXXV. f. 72. a. b.? Pl. XXXVI. f. 75. a. b.
49. — *Æsculi*, Ochs.....	Hübn. Pap. Tab. 109. f. 559. 560. (mas.)
50. — *Acaciæ*, Fab.......	Herbst, Schm. Tab. 308. f. 3. 4.
51. — *W. album*, Knoch.	Ernst, I. Pl. LXXXII. Suppl. II. Pl. 3. f. 72. a—c. bis.
52. — *Pruni*, Linn.......	Ernst, I. Pl. XXXVI. f. 73. a—f.
53. — *Betulæ*, Linn......	Ernst, I. Pl. XXXV. f. 70. a—f.

Genus 10. PAPILIO, *Fab.*, *Lat.*

Pterourus, Scop. Pieris, Schrank. Principes, Hübn.

Legs six, perfect (formed for walking).

Wings, exterior margin of the anterior wings longer than the interior; posterior wings caudate, and excised to allow freedom of motion to the abdomen, or grooved to receive it.

Antennæ filiform, terminated by an oval obtuse club.

Larva fleshy; head obtuse, small; neck furnished with a furcate, retractile organ.

Pupa angular, anteriorly bifurcate, fastened by a transverse thread.

Metamorphosis in the air.

Species.	Icon.
1. P. *Ajax*, Linn........	Esper, Schm. I. Th. Tab. LI. Cont. I. f. 1.
2. — *Podalirius*, Linn.	Ernst, I. Pl. XXXIV. f. 69. a—d.
3. — *Machaon*, Linn...	Ernst, I. Pl. XXXIV. f. 68. a—e.

Genus 11. ZERYNTHIA, *Ochs.*

Thais, Fab., Latr. Argyreus, Scop. Pieris, Schrank.

Legs six, perfect (formed for walking).

Wings, posterior elongated, dentate, ecaudate.

Antennæ short; knob oval; apex slightly pointed.

Larva similar to those of the preceding genus in form, with the segments of the body furnished with rows of stiff hairs.

Species.	Icon.
1. Z. *Polyxena*, Hübn..	Ernst, I. Pl. LII. f. 109. a. b.
2. — *Medesicaste*, Illig.	Ernst, I. Pl. LXXVIII. Suppl. XXIV. f. 109. a—d. bis.
3. — *Rumina*, Linn. ...	Hübn. Pap. Tab. 124. f. 633. 634. (mas.)

Genus 12. DORITIS, *Fab.*

Parnassius, Latr. Pieris, Schrank.
Argus et Battus, Scop.

Legs six, perfect.

Wings rather long, partially diaphanous; posterior excised, not enveloping the body.

Body very short, thick, and hairy; the females with a strong, carinated, concave membrane on the posterior segment of the abdomen.

Antennæ short; club elongated oval, straight.

Larva with tentacula, and nearly of equal thickness through its whole length, hairy, hairs short.

Pupa, oval, folliculated, inclosed in a thin web.

Species.	Icon.
1. D. *Apollinus*, Herbst.	Ernst, I. Pl. LXXVI. Suppl. XXII. f. 99. a—d. quart.
2. — *Apollo*, Linn. ……	Ernst, I. Pl. XLVII. f. 99. a—h. Pl. LXXV. Suppl. XXI. f. 99. a. b. bis.
3. — *Delius*, Esp. ……..	Hübn. Pap. Tab. 110. f. 567. 568. (mas.)
4. — *Mnemosyne*, Linn.	Ernst, I. Pl. XLVIII. f. 100. a—c.

Genus 13. PONTIA, *Fab.* (Steph.)

Pieris, Latr., Schrank. Battus et Ascia, Scop.
Mancipia, Hübn.

Legs six, alike in both sexes.

Wings entire, opaque; anterior somewhat triangular, sometimes rounded at the tip, generally white, with some black spots; posterior rounded, with a groove on the inner margin to receive the abdomen, beneath often coloured yellowish or greenish.

Antennæ with an abrupt, obconic, compressed club.

Larva with a small, round head; body slender, tapering at each end, downy.

Pupa angular, acuminated in front, supported by transverse threads on the middle and posterior portion of the body *.

1. P. *Cra-*

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* Mr. Stephens, in his Illustrations, observes that the insects of this Genus, "from the simplicity of their colouring, and their common appearance, have been unworthily neglected in this country by collectors; and in consequence we still remain unacquainted with the history and metamorphosis of some of the species, which evidently are far from uncommon." Mr. Stephens has examined this group with considerable attention, and has been induced in consequence to introduce as distinct species, certain individuals which have hitherto

Species.	Icon.
1. Po. *Cratægi*, Linn....	Ernst, I. Pl. XLVIII. f. 101. a—f.
2. — *Brassicæ*, Linn...	Ernst, I. Pl. XLIX. f. 102. a—c.
3. — *Rapæ*, Linn.......	Ernst, I. Pl. XLIX. f. 103. a—d.
4. — *Napi*, Linn.	Ernst, I. Pl. L. f. 104. a. b.
5. — *Callidice*, Hübn...	Hübn. Pap. Tab. 81. f. 408. 409. (mas.) Tab. 108. f. 551. 552. (fœm.)
6. — *Raphani*, Fab. ...	Esper, Schm. I. Th. Tab. LXXXIV. Cont. XXXIV. f. 3. (mas.) Tab. CXXIII. Cont. 78. f. 3. (mas.) 4. (fœm.)

hitherto been considered merely as varieties of long established species:—for instance, the smaller variety of *Po. Brassicæ* constitutes his species *Chariclea.* Now it has been generally considered, that the chief difference between the larger and smaller varieties of *Po. Brassicæ* consists in size and colour; to explain which, it is observed that the larger are the æstival, and the smaller the vernal brood; and that the paler colours and smaller size of the latter are owing, the one, to the solar rays not being sufficiently powerful, when the insect comes forth, to produce the intense hue so conspicuous in the supposed æstival brood of *Po. Brassicæ;* the other, to the diminution in bulk, which the animal is presumed to sustain in consequence of the longer period that it remains in the pupa state, namely, from September to April; whereas the æstival brood remains in that state a few days only. To these explanations Mr. Stephens objects, that *Po. Brassicæ* also occurs early in the month of May, so that the difference of the sun's influence can, in those cases, amount to little. And as to the supposed alternating increase and diminution of size in the vernal and æstival broods, it is an anomaly in Zoology, "unless *Po. Rapæ* and *Metra* offer an example; but these insects, I presume, are distinct, upon similar grounds to those which appear to separate the insects that have promoted these observations."—*Stephens.* These grounds are, at least as to *Po. Brassicæ* and *Chariclea,* that the latter is considerably smaller than the former; *Po. Brassicæ* has the tip of the anterior wings above, black, and the patch on its inner edge indented, the points of the indentations following the direction of the nervures, and the extreme tip being slightly irrorated with white, with the cilia waved with black and yellowish; *Po. Chariclea* has the tip ash-coloured, without any internal indentations; the cilia with which it is fringed are pale, and the under surface of the posterior wings of a deeper yellow and more thickly irrorated with dusky, than those of *Pontia Brassicæ.* Stephens divides his genus into two sections,—the first containing "the true *Pontiæ;* the second, those insects which, if necessary to create (*them*) into a new genus, may, after Hübner, be termed *Mancipia.*"

The following are his characters of the two sections:

"A. With the terminal joint of the palpi longer than the second: the apex of the anterior wings obtusely angled: the posterior wings not variegated beneath: the pupa strongly angulated, with a distinct short process in front, and projecting lateral appendages in front of the wing-cases (Pontia)."

"B. With the terminal joint of the palpi shorter than the second: the anterior wings distinctly rounded at the tip: the posterior variegated beneath: the pupa angulated, with an elongated acute process or beak in front: lateral appendages wanting (Mancipium)."

7. P. *Chlo-*

Species.	Icon.
7. P. *Chloridice*, Hübn.	Hübn. Pap. Tab. 141. f. 712. 713. (mas.) 714. 715. (fœm.)
8. — *Daplidice*, Linn..	Ernst, I. Pl. L. f. 106. a—c. Curtis, Brit. Ent. I. Pl. 48. (figura optima.)
9. — *Glauce*, Hübn. ...	Hübn. Pap. Tab. 107. f. 546. 547. (mas.)
10. — *Belemia*, Hübn...	Hübn. Pap. Tab. 82. f. 412. 413. (fœm.)
11. — *Belia*, Fab.	Hübn. Pap. Tab. 83. f. 417. 418. (fœm.)
12. — *Ausonia*, Hübn...	Hübn. Pap. Tab. 113. f. 582. 583. (fœm.) Tab. 83. 416. (mas.)
13. — *Tagis*, Hübn.	Hübn. Pap. Tab. 110. f. 565. 566. (mas.)
14. — *Cardamines*, Linn.	Ernst, I. Pl. LI. f. 107. a—k.
15. — *Eupheno*, Linn....	Ernst, I. Pl. LII. f. 108. a. b. e. f. Pl. LXXVII. Suppl. XXIII. f. 108. g. h.
16. — *Sinapis**, Linn...	Ernst, I. Pl. L. f. 105. a—c.

Genus 14. COLIAS, *Fab.*, *Latr.*

ARGYREUS et BATTUS, Scop. PIERIS, Schrank.

Legs six, alike in both sexes, moderate, slender.

Wings, anterior somewhat triangular, posterior rounded, with a groove to receive the abdomen.

Antennæ short, rather slender, filiform at the base, towards the tip gradually thickening into an obconic club.

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* On this species Stephens has formed a new Genus, which he has called LEUCOPHASIA. Its characters are as follows:

"Genus 5. LEUCOPHASIA[a], *mihi.*

"*Antennæ* with an abrupt, obconic, compressed club; *palpi* very short, depressed, three-jointed, the basal joint large, conic, the second small, quadrate, the terminal one minute, globose: *wings* opaque, suborbicular, the discoidal cell small, basal; posterior wings slightly grooved: *legs* alike in both sexes, moderate; *claws* distinct, bifid. *Caterpillar* cylindric, downy. *Chrysalis* angulated, fusiform, supported by a transverse thread."—*Illust. Brit. Entom.* (HAUSTELLATA), vol. i. p. 24.

Stephens refers *Po. Cratægi* to the genus *Pieris*, which he adopts as distinct from *Pontia;* and in the latter genus he inserts as separate species *Napææ*, Hübn., and *Bryoniæ*, Wallner, both of which Ochsenheimer considers (though with a note of doubt) as varieties of *Napi;* and Stephens himself suspects also, that the former may possibly be nothing more. He has substituted Petiver's name of *Sabellicæ* for that of *Bryoniæ*, adopted by Wallner on the score of priority.

[a] Λευκος, *albus;* Φασις, *apparitio.*

Larva elongated, nearly cylindrical, hairy, but the hairs so short that they appear naked; back pale, or dark green, no central, longitudinal stripe.

Pupa acuminated in front, gibbous, subangulated, fastened by a transverse thread.

"The *Coliades* are particularly gay and showy insects; they are eminently distinguished by the brilliant tints of orange and yellow with which their wings are adorned; they are of moderate size, and usually appear in their final state towards the autumn."—*Stephens.*

FAM. A.—Wings rounded, margin generally dark-coloured.

Species.	Icon.
1. C. *Edusa*, Fab.	Ernst, I. Pl. LIV. f. 111. a—e.
2. — *Aurora*, Fab.	Ernst, I. Pl. VIII. Suppl. III. f. 111. quint.
3. — *Myrmidone*, Hübn.	Ernst, I. Pl. LXXVIII. Suppl. XXIV. f. 111. a. b. bis.
4. — *Chrysotheme*, Hübn.	Ernst, I. Pl. LXXVIII. Suppl. XXIV. f. 111. a. b. tert.
5. — *Phicomone*, Hübn.	Ernst, I. Pl. LXXIX. Suppl. XXV. f. 112. a—c. bis.
6. — *Hyale*, Linn.	Ernst, I. Pl. LIV. f. 112. a. b.
7. — *Palæno*, Linn.	Ernst, I. Pl. VI. Suppl. III. f. 111. a. b. quart.

FAM. B.—Wings somewhat angular*.

8. C. *Rhamni*, Linn.	Ernst, I. Pl. LIII. f. 110. a—e. Curtis, Brit. Ent. Pl. 173.
9. — *Cleopatra*, Linn. ...	Ernst, I. Pl. LIII. f. 110. f. g. (mas.)

Genus 15. HECAERGE, *Ochs.*

LIBYTHEA, Fab. NYMPHALIS, Latr.

Legs four, perfect.

Wings angular, dentate, dark coloured with lighter spots.

Antennæ short, rigid, fusiform.

Palpi very long, porrected, straight.

Species.	Icon.
1. H. *Celtis*, Fab.	Ernst, I. Pl. I. Suppl. III. f. 5. a—f. bis.

† Only one Europæan species.

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* Genus GONEPTERYX, *Leach.*

"*Antennæ* short, stout, very gradually thickening into an obconic club; *palpi* short, much compressed, the terminal joint very short; *wings* angulated, large, the *posterior* grooved to receive the abdomen: *legs* alike in both sexes, short, stout; *claws* minute, bifid. *Caterpillar* naked. *Chrysalis* angulated, acuminated in front; fastened with a loose thread round its middle."—*Stephens*, Illust. Brit. Entom. (HAUSTELLATA), vol. i. p. 8.

Genus

Genus 16. HESPERIA, *Latr.*

THYMELE, PAMPHILA, Fab. (Steph.)	BATTUS, Scop.
ERINNYS, Schrank.	URBANI, Hübn.

Legs six, perfect (formed for walking.)

Wings, *anterior* either short, broad, triangular, and rounded posteriorly (THYMELE, Steph.), or nearly triangular, and slightly elongate (PAMPHILA, Stephens); *posterior* broad, rounded, triangular, entire, or slightly denticulated (THYMELE, Steph.), or rather ovate-triangular, with an obsolete emargination on the hinder margin, and sometimes a rudiment of a tail at the anal angle (PAMPHILA, Steph.).

Antennæ short, a little elongate, with a curved, fusiform club, not terminating in an acute hook (THYMELE, Steph.), or not very long, with an abrupt, fusiform club, varying slightly in form, and terminated generally in a hook (PAMPHILA, Steph.).

Head large.

Body short, thick.

Larva, naked (THYMELE, Steph.), or pubescent (PAMPHILA, Steph.).

Pupa, with the head-case notched (THYMELE, Steph.), or with the front acuminated (PAMPHILA, Steph.).

	Species.	Icon.
1.	H. *Malvarum*, Hoffmannsegg. O. * ...	Ernst, I. Pl. XLVI. f. 98. a—c.
2.	— *Lavateræ*, Hübn.	Ernst, I. Pl. LXXV. Suppl. XXI. f. 98. d. e.
3.	— *Tessellum*, Hübn.	Hübn. Pap. Tab. 93. f. 469. 470. (mas.)
4.	— *Sidæ*, Fab.........	Ernst, I. Pl. VII. Suppl. III. f. 97. a. b. quart.
5.	— *Carthami*, Hübn.	Ernst, I. Pl. VII. Suppl. III. f. 97. quint.
6.	— *Alveus*, Hübn. ...	Hübn. Pap. Tab. 99. f. 506. (fœm.)
7.	— *Fritillum*, Hübn.	Hübn. Pap. Tab. 92. f. 461. (mas.) 462. 463. (fœm.)
8.	— *Alveolus*, Hübn. †	Hübn. Pap. Tab. 92. f. 466. 467. (fœm.)
9.	— *Proto*, Ochs.......	Esper, Schm. I. Th. Tab. CXXIII. Cont. 78. f. 5. (mas.) f. 6. (fœm.)
10.	— *Sertorius*, Illig. ...	Hübn. Pap. Tab. 95. f. 471. 472. (fœm.)

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* Pa. *Malvæ*, Fab.—THYMELE, Steph.

† THYMELE, Steph.

11. H. *Encrate*,

Species.	Icon.
11. H. *Eucrate*, Ochs. ...	Esper, Schm. I. Th. Tab. CXXIV. Cont. 79. f. 6.
12. — *Tages*, Linn.* ...	Ernst, I. Pl.LXXV.Suppl.XXI. f. 97. a. b. bis.
13. — *Pumilio*, Illig.	Hübn. Pap. Tab.91. f.458. (mas.) 459. 460. (fœm.)
14. — *Steropes*, Hübn....	Ernst, I. Pl. LXIV. f. 94. a. b.
15. — *Paniscus*, Fab.†	Ernst, I. Pl. XLV. f. 96. a. b.
16. — *Sylvius*, Fab.† ...	Ernst,I. Pl.LXXIV. Suppl.XX. f. 96. e. f.
17. — *Comma*, Linn.†...	Hübn. Pap.Tab. 95. f. 479. (mas.) 480. 481. (fœm.)
18. — *Sylvanus*, Fab.†...	Ernst,I. Pl.XLV.f.95.a—d.g.h.
19. — *Linea*, Fab.†......	Ernst, I. Pl. XLV. f. 95. c. f.
20. — *Lineola*, Ochs. ...	Hübn. Pap. Tab. 130. f. 660. 661. (mas.) 662. 663. (fœm.)
21. — *Actæon*, Hübn....	Hübn. Pap. Tab. 96. f. 488. 489. (mas.) 490. (fœm.)

Genus 17. CHIMÆRA, *Ochs.*

ATYCHIA, Latr. STYGIA, Godart.‡

Wings, anterior short, small, of nearly equal length throughout; posterior rounded.

Head small.

Antennæ bipectinate in the male, simple in the female (Latr.).§

Palpi, labial rising remarkably above the clypeus, anteriorly very hirsute. (Latr.)

Antlia very short, or wanting.

Abdomen posteriorly elongated.

Tibiæ, with elongated scales and calcaria. (Latr.)

Larva, unknown.

Species.	Icon.
1. Ch. *Pumila*, Ochs. ...	Hübn. Noctuæ, Tab. 86. f. 405.
2. — *Appendiculata*, Ochs. ‖	Ernst, III. Pl. CII. f. 149. a—c. (fœm.) VI. Pl. CCLXXIII. f. 438. a—c.

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* THYMELE, Steph. † PAMPHILA, Steph.

‡ *Histoire Naturelle des Lepidoptères, ou Papillons de France*, vol.iii. p.167. Although this volume is dated 1822, and the *fourth* of Ochsenheimer's work appeared six years before, Godart does not seem to have been aware that the German author had in this, his last volume, adopted Latreille's, or rather Draparnaud's Genus STYGIA, for the reception of Hübner's *Bombyx Terebellum*, which he accordingly transferred from its former place with the Chimeræ (vol. ii. p. 6. No. 4. *Ch. leucomelas*) to that Genus.

§ Ochsenheimer's generic characters in this and several other instances are so insufficient, that I have often found it necessary, as in the present case, to quote other naturalists of acknowledged authority.

‖ *Noct. Chimæra*, Hübn. *Pyral. Vahliana*, Fab.

Species.	Icon.
3. Ch. *Radiata*, Ochs....	— — —
4. — *Lugubris*, Ochs.[*]	Hübn. Bombyces, Tab. 51. f. 217. (mas.)

Genus 18. ATYCHIA, *Ochs.*

PROCRIS, Fab., Latr. AGLAOPE, Latr. GLAUCOPIS, Fab., Latr. (INO, Leach, Stephens.) CHRYSAORES, Hübn.

Wings oblong, ciliated; submarginal cell of the inferior closed behind by a very angular nervure, from which three branches proceed, and terminate at the posterior margin. (Godart.)

Antennæ bipectinate in the male; simple in the female. (Latr.)

Palpi short, scarcely or not at all rising above the clypeus, densely clothed with scales, not hirsute. (Latr.)

Tibiæ scaly; posterior with small calcaria, and the two upper, interior spines very minute, or obsolete. (Latr.)

Larva, short, thick, nearly naked; head small.

Pupa soft, with moderately long wing-cases.

Species.	Icon.
Fam. A. 1. A. *Infausta*, Linn.[†]	Ernst, III. Pl. CIII. f. 152. a. b.
Fam. B. 2. — *Pruni*, Fab.	Ernst, III. Pl. CIII. f. 151. a—e.
3. — *Globulariæ*, Hübn....	Hübn. Sphing. Tab. 1. f. 2. (mas.) 3. (fœm.)
4. — *Statices*, Linn.[‡]	Ernst, III. Pl. CIII. f. 150. a—d.

[To be continued.]

Note.—The reader is requested to attend to the following corrections of some of the preceding synonyms.

Gen. Argynnis	20.	*for* Fab.	*read* Hübn.	*Gen.* Hipparchia	38. 57.	*for* Fab.	*read* Hübn.
Hipparchia	1.	— Fab.	— Hübn.	———	60.	— Fab.	— Thunb.
———	3.	— Linn.	— Hübn.	———	61. 65.	— Fab.	— Esp.
———	7. 11.	— Fab.	— Herbst.	———	69.	— Fab.	— Hübn.
———	13. 20.	— Fab.	— Hübn.	———	77.	— Fab.	— Pallas.
———	23.	— Fab.	— Esper.				

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[*] *Bomb. lugubris*, Hübn.

[†] Genus AGLAOPE, Latr.

A. *lingua* nulla, aut obsoleta. *Palpi* minimi, articulo ultimo subgraciliore, minus squamato. *Tibiæ* posticæ calcaribus spinisque brevissimis, subobsoletis. *Anus* imberbis. *Latr. Gen. Crust. et Ins.* iv. 214. To which may be added: *Antennæ* sexu utroque bipectinatæ; *alæ* oblongæ, cellulâ marginali inferiorum posticè clausâ, ramisque duobus nervosis, ad lineam sepimenti sese invicem decussantibus, longitudinaliter divisâ. (Godart.)

[‡] This and the preceding species are placed by Stephens in the Genus INO, Leach, "established by Fabricius by the name of Procris; but that having been preoccupied, Dr. Leach changed its appellation to the one it now bears." The generic characters of Ino are given by Mr. Stephens as follows:

"INO,

LXI. *Notice of the Geological Features of a Route from Madras to Bellary, in April and May* 1822. *By Capt.* W. CULLEN, *of the East India Company's Artillery service*[*].

I BEG to submit to the Society an attempt to describe the geological features of a route which I lately passed over from Madras to Bellary. It accompanies a small collection of specimens of the prevalent rocks, and a barometrical section, which combined will, I hope, assist in affording some idea of the nature of the tracts in question.

The high road to Bellary was followed as far as Cuddapah; but from thence going north, by Chinnoor Nundialpett Poonnamila to Iddamacul, my route, from the last-mentioned village, lay nearly west by Giddeloor, over the Nulla Mulla range of hills by the Nundi Kunnuwi Ghaut[†], by Banaganapilly, Piaplee, and Gootty, to Bellary. A great proportion of this route must, in favourable weather, be as beautiful in point of scenery, as it is rich in geological interest; but at the period of my passing (the latter end of April and beginning of May), the excessive heat had checked all vegetation, and afforded but little inducement for excursions in quest of mineralogical specimens.

Referring the route to Arrowsmith's large map, which is sufficiently correct for the present purpose, it will be observed to offer an obvious distribution into five portions, each of them characterized by distinct geographical features.

First. The plain open tract from Madras to Naggery.

Second. The narrow mountainous belt extending from Naggery to the neighbourhood of Cummum.

"INO, Leach.—*Antennæ* gradually thickening from the base to near the apex, straight, bipectinated, or simple, with the interior edge subserrated: *palpi* short, not reaching beyond the clypeus, densely clothed with scales: *head, thorax, abdomen,* and *femora,* thickly covered with scales, rather elongate on the former. *Larva* scaly, depressed; head small: pupa with long wing-cases."

"The species are known from the Anthoceræ (Zygænæ) by the form of the Antennæ, which are not curved, but nearly straight, and become gradually thicker as they approach the tip, which is again slightly attenuated; the males have this part bipectinated, and the females simple, but serrated beneath; the species (of which there are several on the continent) are all of rich tints of light green, blue, or brownish, and immaculate."—*Illust. Brit. Ent.* (Haustellata), vol. i. p. 105. Stephens gives only one species, (*Statices,* Linn.) as decidedly British: that considered as *Globulariæ,* Hübn. having, on examination, proved to be referable to *Ino Statices,* var. β. He conceives, however, that it is extremely probable that *Ino Globulariæ* may occur in England.

[*] From the Transactions of the Literary Society of Madras. Part I.

[†] Kunnuwi is Kanarese for Ghat. Nundikunnuwi means, therefore, Nundi Ghat.

Third. The open level country from the Nulla Mulla hills to Banaganapilly.

Fourth. The tract of tabular land between that town and Gootty.

Fifth. The level country from thence to Bellary.

The geological characters of this tract are equally remarkable, and admit of a division corresponding perfectly with its geographical features.

In the first division the prevailing rocks are granite.

In the second, clay-slate and sandstone.

In the third, compact blue limestone.

In the fourth, clay-slate and sandstone.

In the fifth, granitic.

I have ventured to characterize each division by one or two rocks only, because in each of them the rocks specified were, in general, beyond all comparison the most abundant. In the several divisions, of course, were found many of those minerals by which the principal rocks are usually accompanied; but to enumerate the whole of these as they occurred may not be deemed necessary, since the specimens themselves are forwarded.

Before entering into a detail of the rocks prevailing in these tracts, it may be proper to notice, in a general way, their absolute heights above the sea.

The north-west side of Pootoor, at the distance of sixty-four miles from Madras, exclusive of windings, stands about 500 feet above the sea; exhibiting a rise of eight feet in the mile; and this proportion holds good throughout that part of the route, interrupted only by one undulation on the east side of Naggery, and by a second between Naggery and Pootoor.

These undulations, which rise 100 or 150 feet above the general level, mark the course of chains of hills, which in such places cross the road; and, in general, in all these sections of the *terrepleine* of a country, similar abrupt elevations may be considered as indications of the presence, and course of a chain of hills. There is a third rise a little beyond Pootoor, indicating like the former, the presence of a mountainous range.

The valley of Tripetty is, on a mean, about 360 feet above the sea, but the river which runs through its centre little above 300. The mean height of the valley from Baulpilly to Wuntimettah, an interval of about 52 miles, is about 550 feet, and the town of Cuddapah itself a little below 500.

Chinnoor on the Pennar river, five or six miles north of Cuddapah, is about 30 feet lower than that place; but the height of Jungumpilly, the next march, is 700 feet. There is then a fall of about 100 feet to the Saghilair river; after which it rises gradually to Alinuggar and Iddamacul, both of which places are on the same level, about 900 feet above the sea. I was much disappointed in the height of the Nulla Mulla range, which, at the point where I crossed, did not attain an elevation of 1800 feet above the sea, and of little more, therefore, than 800 feet above the plains on either side.

The route across the plain, between the Nulla Mulla range and the table land at Banaganapilly, is nearly level, and about 800 feet above the sea; but the general declination of this plain appears to be from the Kistnah to the Pennar.

From Banaganapilly to Jeldroogum the ascent along the valley is pretty considerable, being 400 feet in about twenty miles, or 20 feet per mile.

The table land, commencing two or three miles west of Jeldroogum, and extending to Piaplee, a distance of eight or ten miles, is between 1700 and 1800 feet above the sea *; and Colonel Lambton has already stated that to be the mean height of the country between Gootty and Bellary †.

Although granitic have been mentioned as the prevailing rocks in the first division, none of them were seen *in situ* till about the thirty-seventh mile, in the bed of the river at the village of Nellatoor. The whole of the previous flat being a loose sandy soil, entirely free from rocky masses, and even almost so of fragments, with the exception of some stony swells to the north of Cunkama Choultry. I should observe, however, that all the pagodas, facings of tanks, &c, were built either of granite or laterite.

The blocks forming these latter have a rolled appearance, are a kind of coarse sandstone conglomerate or breccia, and perhaps originate from, or are connected with, the mountain-chain running north from Naggery Nose. The granite, which first makes its appearance at Nellatoor, may be traced as far as Curcumbaddy, with no other interruption save that of those singular beds or courses of trap which are apparently so common in all the granitic tracts of this country. All these beds appear to run nearly east and west. In the present instance they were remarkably numerous, forming chains of low hills, and crossing the route so frequently, as to occupy a space which, taken in the aggregate, would nearly equal that of the granite itself. Granite, however, evidently composes the great mass of hills, which commence a few miles to the south-west of Naggery, and which continuing near to the left of the road as far as Woramallipett, then stretch off to the west, till they are lost in the prolongation of the Tripetty range. The peculiar features of the granite are very marked and conspicuous in the whole of this western mass of hills, exhibiting itself on their slope, in those great bare masses of rock, which are so familiar to most people in this country, and on their summits in enormous detached rugged piles and fragments. But what contributes most powerfully to the interest of this part of the route are these singular courses or dykes of trap rocks, which may be observed crossing the country, without experiencing the smallest deviation or interruption in their course from the granitic barriers, which seem to oppose themselves on all hands to their progress.

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* But there is a very rapid descent from Piaplee towards Gootty, of 400 or 500 feet in the first ten miles. The plains west of Gootty are about 1200 feet above the sea.

† This seems rather under the truth:—barometrical observations, which I have since had an opportunity of making, give from 1400 to 1500 feet for the mean altitude of the country between Gootty and the Hoggree river, eight miles east of Bellary.

Their deep black hue, and sharp, well-defined outline, contrasted with the light colour of the granite-masses, through and over which they seem to pass, forcibly arrest the attention. Granite appears also to a considerable distance on the right or north-east side of the road, and probably constitutes the greater portion of the very remarkable hill called Naggery Nose, as I have traced it nearly to the foot of that hill. The hill just mentioned, however, as well as those immediately to the north of it, and whose outlines are equally singular, are evidently capped with rock of a different nature.

The caps, which occupy about one-fifth or one-sixth of the whole height of the hills, are precipitous and mural on their south and east sides, to the north sloping gradually off, until they fall almost into the same level with the plains. I attempted, both from Potoor and Woramullipett, to reach these hills, with the view of ascertaining their composition, but the distance was too great, and I could only approach their bases.

Judging from the external appearance of the cap, it is composed of two distinct rocks arranged in horizontal beds or strata. The upper and lower portion of it appeared to be of the same nature, being alike in colour, and marked by similar numerous, but irregular vertical seams and fissures; the effect, probably, of decomposition. The aspect of the central stratum or bed, was, however, different from either of those between which it lay. It was marked most distinctly throughout its whole extent, by regularly parallel and horizontal seams, which appeared to be those of stratification; its colour also, which was darker than the others, strengthening the supposition of its being a rock of a different nature*.

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* I have since had an opportunity of examining the hills at Tripetty, where both the cap and slope of the hills appeared to consist of but one rock,

The western approach to these hills, for one mile and a half or two miles from their bases, was thickly strewed with nodules of several varieties of sandstone, the most common of which were of rather a close fine grain, sometimes so much so as hardly to be distinguished from quartz or hornstone. The finer-grained varieties were of different shades of red or brown, but generally of a light colour. There was also great abundance of a very coarse variety, composed of rounded pebbles, and fragments of quartz of all sizes, in the same specimen, from that of a pin's head to two or three inches in diameter, imbedded in a dark green basis. This variety was very remarkable. It was composed of rolled fragments and pebbles of quartz, which were generally of a white colour in a ground of dark green. The cement appears (on the march from Naggery to Potoor there were rolled masses of this variety twelve to eighteen inches diameter) to be hornblende, which communicating its tinge to the finer and transparent particles of quartz, affords a beautiful contrast to the large white pebbles imbedded in it. These nodules I should be disposed to trace from one or both of the two first-noticed portions of the cap, but I met with no fragments of any kind of schistus, owing perhaps to my not having approached sufficiently near. It has been noticed that the summits of this group of mountains, of which Naggery Nose forms the southernmost point, are mural and precipitous to the east and south, while to the north they fall gradually away, till they nearly coincide with the general level of the country. This latter appearance is very striking from Curcumbaddy, where the whole of that group is seen in reverse; Curcumbaddy itself being situated at the foot of one of these declivities, being a prolongation of the Tripetty range, which, from its outline and general aspect, I would infer to be of similar structure with that of Naggery.

The clay-slate, which occupies so great a portion of the subsequent route, first makes its appearance at Curcumbaddy; but the accumulation of sand and alluvial soil in the Tripetty valley, which is crossed on leaving Woramallipett, prevented my thus far tracing the continuity of the granite, although it is to be observed, with occasional beds of green-stone, in several parts of the road. The last rock I recollect to have passed before reaching Curcumbaddy was a bed of porphyritic greenstone, about one mile and a half or two miles from the village.

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rock, and that sandstone. From this, and other corroborative instances on the route between Cuddapah and Ryachootee, I have little doubt that the caps of the Naggery range, of the great mass of hills east of that line, and, in short, of all the ranges exhibiting the same remarkable outlines, consist of varieties of sandstone or conglomerates.

The granites of this division were generally of a light colour, shades of white and of a coarse texture; the darker varieties, however, inclining to brown or red, being, I think, the finer grained.

The quartz and felspar were by far the most abundant constituents, and gave the colour to the rock; the hornblende, which was of a dark green, being very irregularly and sparingly distributed. There seemed to be little or no mica.

The texture of the trap was very uniform, and of a fine grain, composed distinctly of hornblende, and greenish white felspar.

The porphyritic variety, alluded to near Curcumbaddy, contained irregular crystals of felspar, of from one-tenth to five-tenths of an inch in diameter, of the same colour as the felspar of the basis.

The transition of clay-slate is very sudden and complete. The low hills immediately at the back of Curcumbaddy consist of a compact quartzose sandstone, or hornstone, but the clay-slate may be observed in contact with it, within 100 yards of the north side of the village. From this spot clay-slate forms the grand and almost sole constituent; for, with the exception of occasional beds of calcareous schistus and flinty slate in the valleys and sandstone-caps on some of the hills, the great mass of the two singular mountain-chains which form the boundary of this interesting valley, on a line of upwards of 150 miles, appears to consist entirely of that rock. I must add, however, that should an actual personal examination of the strata be considered indispensable in subjects of this nature, these observations must of course, in such a case, be considered as only strictly applicable to the high road itself, or to a short distance on either hand.

The seams of stratification are, however, so entirely regular and distinct on the slope of the hills on either side, and in general so decidedly characteristic of these clay-slate tracts, that it is hardly possible to be mistaken in their nature, even at a distance of several miles. Towards the commencement, the hills are rather thickly clothed with wood; but on approaching Cuddapah, and all to the north of that place, the trees are stunted, and but thinly scattered over their sides, leaving the strata-seams, like so many artificial terraces or ploughed furrows, distinctly exposed to view. The internal structure and colour of the slate, in a tract of such extent, were of course very various. At Curcumbaddy, and for a stage or two afterwards, chiefly shades of red; about Wuntimettah, purple and gray. Shades of these two last prevailed, I think, generally, till within eight miles south of Poonnamilla, when it suddenly altered to green; and this colour subsequently seemed to be constant in all the plains and low grounds. The general direction of the strata of clay-slate corresponded with that of the ranges of mountains which they composed; viz. about north-north-west and south-south-east, with a very great dip to the north-east; all the associated rocks being conformable, unless the *sandstone*-caps should be an exception, which appeared to have a very slight dip, if the appearances noticed from Curcumbaddy and Nundialpett may be considered as indications of it. However, of the latter I had few favourable opportunities for examination.

The strata of clay-slate appeared sometimes to be nearly vertical; but the exact dip was never measured.

The same dip and direction of the strata were exhibited in the fourth division, of clay-slate.

Of the rocks associated with clay-slate, the more important and general were sandstone, hornstone, calcareous schistus, flinty slate, and quartz. Calc-tufa, and marls of infinite variety of colour and induration were also found nearly throughout, and in some places in extraordinary quantity.

The sandstone was usually found on the summits of the hills; the calcareous schistus and flinty slate in the valleys; the quartz forming veins and layers in the seams of the clay-slate, and appearing therefore only where the latter was not concealed by alluvial depositions.

These were sometimes found all together; but it may be more convenient to consider each of them separately.

The quartz was generally of a white colour, and the layers of all degrees of thickness, from one tenth of an inch to one foot and a half. It was extremely subject to disintegration, covering the ground frequently in such quantity with its nodules, as completely to whiten it. These appearances were particularly remarkable on the march from Curcumbaddy to Baulpilly, from the vicinity of the hills on both flanks. Afterwards the valley opens, and the strata are generally concealed by the soil; but whenever rocks appear to any extent, quartz, either in veins or layers, will almost invariably be found pervading them. It is very abundant in the clay-slate between Nundaloor and Wuntimettah, and here rather remarkable from containing numerous little nests of a kind of green earth; until, however, fifty miles north of Cuddapah, and clearing the hills beyond Jungumpilly, the individual masses of quartz are too inconsiderable in themselves, to serve in any other way than merely as a characteristic of the clay-slate, and other more important rocks.

The march from Jungumpilly to Poornamila, with the exception of the first five or six miles, is through an open level country, of perhaps fifteen miles square, as if it had been formed by the abstraction of a part of the central chain of hills which divide the southern and northern portions of this tract into two narrow valleys. The Saghilair river is crossed nearly in the centre of this open space; and it is immediately on reaching its northern bank that the quartz is observed to assume quite a new character, to constitute, as appears subsequently, one of the most important features in the remainder of the route.

A green schistus seems to prevail throughout this plain, and it continues as far north as Iddamacul, as may be observed from an examination of the wells; latterly also appearing above the surface in ridges of considerable elevation.

The strata of schistus in the bed of the Saghilair, which are nearly vertical, and of a bright green colour, present a very interesting appearance.

The direction of the strata at the ford corresponds with that of the bed of the river; and the stream, which appears subject to a very rapid rise and fall, has in consequence worn numerous deep narrow channels through the slate, presenting on all sides sharp perpendicular dykes of fifteen or twenty feet high, while they are often but a few inches in thickness. Almost immediately on reaching the north bank of the Saghilair, the quartz, which hitherto had never been met with but in the seams of the slate, and there seldom exceeding a breadth of eighteen inches, is now observed alone in immense blocks, and continuous masses, of fifty or sixty feet wide. Their direction corresponded, I think, generally with that of the strata of schistus, but they appeared above the soil unaccompanied by any other rock, and forming ridges of such magnitude and extent, as to give them the appearance of the summits of quartz hills, commencing to be denuded of soil, and forcibly impressing one with the idea of being in the vicinity of granite: nor was the impression, perhaps, altogether without foundation, as the small fort of Iddamacul, twenty miles further north, is built on an insulated hill of sienite.

The quartz ridges became gradually more numerous and extensive on my progress up the valley; but I lost them after leaving Iddamacul, and striking off to the westward by Giddeloor towards the Nulla Mulla range. Nothing could possibly be more interesting or striking than this small pile of sienitic masses, which, possessing all the peculiar rugged outline of a granitic hill, afforded the most singular contrast to the smooth, bare, undulated contour of the clay-slate ranges on either side of the valley. The valley was here seven or eight miles wide, and this hill rose out alone, as if forced up from below by volcanic agency, from the dead marshy flat in the centre of it. Its base might be 150 yards in diameter, and its perpendicular height about 100 feet. This sienite is composed of large crystals of black hornblende, and yellowish white felspar, very irregularly aggregated, with but little quartz, and that only in patches, and very unequally dispersed.

The sandstone which has been mentioned was usually found occupying the summits of the hills of clay-slate, and the opportunities, therefore, of actual examination were but rare. These opportunities occurred also at points very distant from each other, (but, perhaps, not the less corroborative from that circumstance, of the inference that the whole of these ranges are capped with varieties of sandstone,) viz. at *Curcumbaddy*, in the ghaut close to *Baulpilly*, in the passage through the range to *Cuddapah*, in the passage of the *Nulla Mulla* range, and lastly, in the fourth division, where, from the small elevation of the hills, these caps may be traced without the slightest interruption for upwards of five-and-twenty miles. The characters of the sandstone vary from that of a coarse conglomerate, such as that noticed on the route between Naggery and Pootoor, to that of the finest grain where it is difficult to distinguish it from quartz or hornstone, into both of which it seemed occasionally to pass. The colours were as various as the texture, being of all shades of red*, white, and green; some of the varieties met with on crossing the hills to Cuddapah were rather handsome.

The sandstone forming the cap of the Nulla Mulla hills, where I crossed them between Kistnumchettypitty and Madapurum, was of great thickness, about 300 feet perpendicular, and its acclivity on both sides, the route lying directly over it, extremely steep and difficult. A great deal of rock, much of the same nature as the cap, and interstratified with the clay-slate, prevailed, however, for a space of three or four miles, on both sides of this central ridge, but the clay-slate still continuing by far the most abundant, and in deep wells immediately at the foot of the cap on the east and west sides, exhibiting that rock alone to the very bottom. The above-noticed were the chief occasions on which the sandstone was observed in extended masses; but nodules of that rock, as well as considerable apparently unconnected masses, were met with in several instances in the valleys, more particularly at the village of Chillumpett, between Codoor and Pollempettah, in the first part of the march from Nundaloor to Wuntimettah, between Poornamila and Alinaggur, in the ditch at Iddamacul, &c.

[To be continued.]

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* The red varieties were most common, I think, west from Banaganapilly.

LXII. *A Letter from* Professor Airy *in reply to* Mr. Galbraith's *Remarks* (p. 182.) *on some late Computations of the Earth's Ellipticity.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN an article which appears in your Journal for September last, Mr. Galbraith expresses himself much astonished at the difference between the value of the earth's ellipticity which he has obtained, and a result at which I had arrived. I think that any person who reads my paper on this subject will see the ground on which such a difference might have been looked for; but for those who do not, a single line may serve to point out the state of the case. Mr. Galbraith's calculations proceed on the assumption that the earth is known to be an ellipsoid: mine, on the supposition that this is not known. It is manifest that to satisfy the observed curvatures in different places we shall have different proportions of the axes, accordingly as the meridian is supposed to be an ellipse, or to be some other figure.

I am by no means disposed to consider his hypothesis to be better founded than mine: but more pressing employments compel me at present to abstain from the discussion of this "much-vexed" topic, to which he so obligingly invites me.

I am, Gentlemen, yours, &c.

G. B. AIRY.

Observatory, Cambridge.
Oct. 17, 1828.

LXIII. *On* Mr. Dalby's *Method of finding the Difference of Longitude between two Points of a Geodetical Line on a Spheroid, from the Latitude of those Points and the Azimuths of the geodetical Line at the same.* *By* Dr. TIARKS, F.R.S. *&c.*

THE ingenious method first suggested by Mr. Dalby, of deducing the difference of longitude between any two points on a spheroid, from the latitude of these points and the inclination of the geodetical line connecting them to their meridians at these points, is founded on a remarkable property of spheroidical triangles formed by geodetical lines, which may generally be thus enunciated: The sum of the three angles of any spheroidical triangle formed by geodetical lines is a function of the latitudes of the angular points and their differences of longitude only, and is altogether independent of the excentricity of the spheroid. This sum, accordingly, is the same as the sum of the three angles of a spherical triangle whose angular points have the same relative situation to a particular diameter, which is considered as the polar axis; that is to say, the same latitudes respectively, and the same differences in longitude. The particular case of this general proposition which is employed by Mr. Dalby, is the one in which two of the geodetical lines are meridians, and where consequently one of the angular points is the pole of the spheroid itself: but it will be easily seen, that from the demonstration of the particular case the truth of the general proposition may be immediately derived. The method used in the Trigonometrical Survey requires, that if two points on a spheroid having respectively a certain latitude on meridians forming a certain angle are connected by a geodetical line, the sum of the angles of this line with the meridians of the points should be the same, whatever the ellipticity of the meridians may be; and, accordingly, that it should be equal to the two angles of the spherical triangle the sides of which are the co-latitudes of the points, and the inclosed angle the inclination of the two meridians. The inclination of the meridians of the spheroid or their difference of longitude is then derived from the two sides and the sum of the angles opposite to them: viz. the co-latitudes, and the sum of the azimuths. Mr. Dalby's method was first published by General Roy in the Phil. Trans. for 1790, in his own words and with his own demonstration. It would appear that this demonstration has not given general satisfaction; for I have observed that the want of success in the application of the method which is, indeed, acknowledged on all hands, has sometimes at least partially been ascribed to its incorrectness; whereas the principle on which it is founded is not only perfectly correct, but neither limited by the length nor the position of the geodetical line to which it is applied. Before I had seen Mr. Dalby's demonstration, I had convinced myself of the correctness of the method, with which I became acquainted through that part of the Trigonometrical Survey published in the Phil. Trans. for 1795, by a demonstration which, although perhaps substantially the same as Mr. Dalby's, yet differs in some respects from it. I hope that this demonstration will not be considered as perfectly useless at the present moment, and I shall add to it a few remarks on the cause of the failure of the practical application which has hitherto been made of this method.

Let 1. be the great semiaxis of the oblate spheroid or the radius of the equator.

e the excentricity of the elliptical meridians.

ω the angle formed by the planes of the meridians of the two points on the spheroid B and D. (fig. 1.)

λ, λ' their latitudes; and let λ be greater than λ'.

l, l' the reduced latitudes, or the angles dependent on λ, λ' by this equation $\text{tang.}\, l = \sqrt{(1-e^2)}\ \text{tang.}\, \lambda$.

m, m' the angles formed at the intersection of the geodetical line and the meridians between the former and each of the latter.

μ, μ' the angles in a spherical triangle, two sides of which are $90^\circ - \lambda$ and $90^\circ - \lambda'$, and angle between them $= \omega$; μ' being opposite to the side $90^\circ - \lambda$, and μ opposite to the side $90^\circ - \lambda'$, and

β the third side of the spherical triangle opposite to ω.

Let PDF and PEB represent the two meridians whose inclination to each other is $= \omega$, and let the lines BC and DA be perpendicular to the meridians at B and D. Draw AE parallel to CB and CF to AD, and join A and B and C and D by straight lines. Let CB be $= r$, and AD $= r'$; the angle EAB $=$ ABC $= y$, and the angle FCD $=$ CDA $= x$.

It is then clear, that the inclination of the plane BCD to that of the meridian PEB is $= m$, and that the inclination of the plane BCF to the plane of the same meridian, is $= \mu$, and that $\mu > m$. If we now assume that the three lines CB, CP, CF determine on a sphere described about C as a centre, the angular points of the spherical triangle PB'D' (fig. 2.), and

Fig. 1.

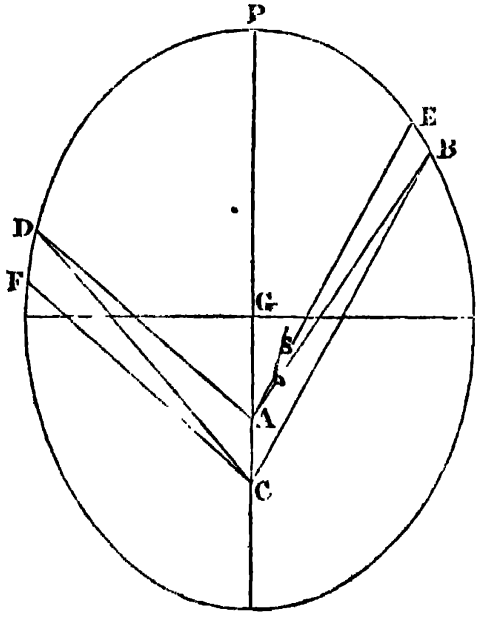
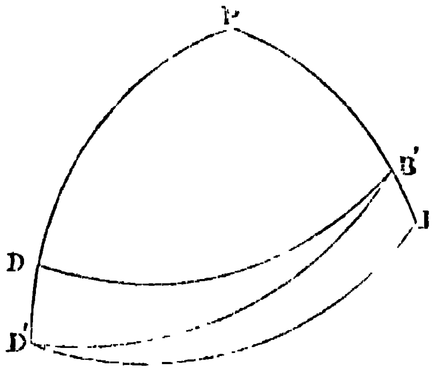


Fig. 2.



that on the arc PD', DD' is made equal to x, we shall have $PB' = 90^\circ - \lambda$, $PD' = 90^\circ - \lambda'$, $PB'D' = \mu$, $PB'D = m$, $PD'B' = \mu'$, $D'B' = \beta$. We have, therefore,

$$\text{Sin D} : \cos\lambda = \sin m : \cos(\lambda' + x), \text{ and}$$
$$\text{Sin } \beta : \sin \text{D} = \sin x : \sin(\mu - m), \text{ hence}$$

$$\text{Sin }(\mu - m) = \frac{\cos\lambda \,.\, \sin m}{\sin\beta.} \,.\, \frac{\sin x}{\cos(\lambda' + x)}.$$

In the same manner it will be seen that the angle of the geodetical line with the meridian of D is the inclination of the planes PAD and DAB = m', and that the corresponding μ' is the inclination of the plane DAE to that of the meridian PAD. If we, therefore, assume a sphere whose centre is A, the lines AE, AD and AP will determine on that sphere the angular points of the triangle PD'B' (fig. 2.); and if we produce PB' to B so that $BB' = y$, we shall have $PD'B' = \mu'$, $PD'B = m'$, and consequently,

$$\text{Sin B} : \cos\lambda' = \sin m' : \cos(\lambda - y), \text{ and}$$
$$\text{Sin } \beta : \sin \text{B} = \sin y : \sin(m' - \mu'), \text{ hence,}$$

$$\text{Sin }(m' - \mu') = \frac{\cos\lambda' \,.\, \sin m'}{\sin\beta} \,.\, \frac{\sin y}{\cos(\lambda - y)}.$$

From the triangles ACB and CAD (fig. 1.) we shall easily derive $\frac{\sin x}{\cos(\lambda + x)} = \frac{AC}{r'}$, and $\frac{\sin y}{\cos(\lambda - y)} = \frac{AC}{r}$, and as AC

$$= GC - AG = \frac{e^2 \sin\lambda}{\sqrt{(1 - e^2 \sin\lambda^2)}} - \frac{e^2 \sin\lambda'}{\sqrt{(1 - e^2 \sin\lambda'^2)}},$$

$$\text{or nearly } = \frac{e^2(\sin\lambda - \sin\lambda')\,(1 + \frac{e^2}{2}\sin\lambda \,.\, \sin\lambda')}{\sqrt{(1 - e^2 \sin\lambda^2)} \,.\, \sqrt{(1 - e^2 \sin\lambda'^2)}}, \text{ and}$$

$$r = \frac{1}{\sqrt{(1 - e^2 \sin\lambda^2)}},\ r' = \frac{1}{\sqrt{(1 - e^2 \sin\lambda'^2)}}, \text{ we have}$$

$$AC \quad \frac{e^2(\sin\lambda - \sin\lambda')\,(1 + \frac{e^2}{2}\sin\lambda \,.\, \sin\lambda')}{\sqrt{(1 - e^2 \sin\lambda'^2)}}, \text{ and}$$

$$\frac{AC}{r'} - \frac{e^2(\sin\lambda - \sin\lambda')\,(1 + \frac{e^2}{2}\sin\lambda \,.\, \sin\lambda')}{\sqrt{(1 - e^2 \sin\lambda^2)}}$$

Substituting these values of $\frac{AC}{r'}$ and $\frac{AC}{r}$ for $\frac{\sin x}{\cos(\lambda + x)}$, and $\frac{\sin y}{\cos(\lambda - y)}$ in the above equations, we obtain

$$\sin(\mu - m) = \frac{\cos\lambda \,.\, \sin m \,.\, e^2(\sin\lambda - \sin\lambda')\,(1 + \frac{e^2}{2}\sin\lambda \,.\, \sin\lambda')}{\sin\beta \,.\, \sqrt{(1 - e^2 \sin\lambda^2)}}$$

$$\frac{}{\sin\beta} \,.\, e^2(\sin\lambda - \sin\lambda')\,(1 + \frac{e^2}{2}\sin\lambda \,.\, \sin\lambda') \quad \text{and}$$

sin

$$\sin(m'-\mu') = \frac{\cos\lambda' . \sin m'}{\sin\beta} \cdot \frac{e^2 (\sin\lambda - \sin\lambda')(1 + \frac{e^2}{2}\sin\lambda . \sin\lambda')}{\sqrt{(1-e^2\sin\lambda'^2)}}$$

$$= \frac{\cos l' . \sin m'}{\sin\beta} e^2 . (\sin\lambda - \sin\lambda')(1 + \frac{e^2}{2}\sin\lambda . \sin\lambda').$$

But by the characteristic property of the geodetical line we have $\cos l \sin m = \cos l' \sin m'$; and it is therefore evident that $\mu - m = m' - \mu'$ or $m + m' = \mu + \mu'$, and the sum of the three angles $\omega + m + m' = \omega + \mu + \mu'$, independent of the ellipticity of the meridians. If we now conceive any three points on the spheroid connected by geodetical lines, and draw their meridians, the comparison of the angles of the three triangles thus formed, each by two meridians and a geodetical line, with the analogous ones on the sphere, will immediately prove the general proposition.

Example.—Professor Bessel has accurately deduced from the latitude of the observatory at Seeberg = 50° 56′ 6″·7 (λ'), the length of the geodetical line from Seeberg to Dunkirk (whose logarithm = 5·47830314) and the inclination of that line to the meridian of Seeberg = 85° 38′ 56″·82 (m') (the last two as resulting from General Müffling's great measurement), the following results, supposing $l\,e$ = 8·9054355 and log. semi-polar-axis = 6·51335464:

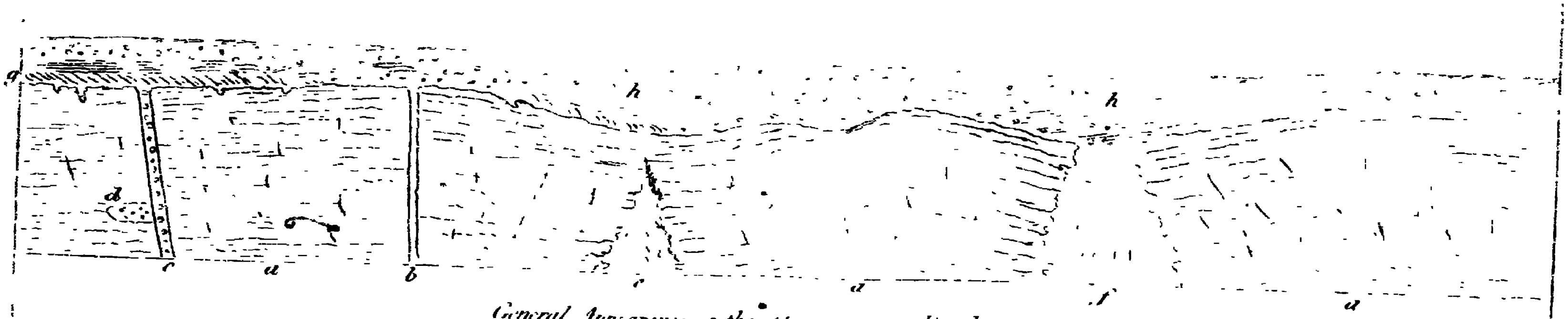
Latitude of Dunkirk .... = 51° 2′ 12″·719 (λ)
Inclination of the geodetical line to the meridian of Dunkirk.......... = 87 51 15·523 (m)
Difference of longitude between Seeberg and Dunkirk............. 8 21 19·04 (ω)

These quantities, therefore, certainly belong to the same spheroid, whether right or wrong as to the places named. I find from λ, λ' and $m + m'$ in the spherical triangle

μ = 87° 51′ 25″·78
μ' = 85 38 46·61
β = 5 15 28·44
ω = 8 21 19·09. And,

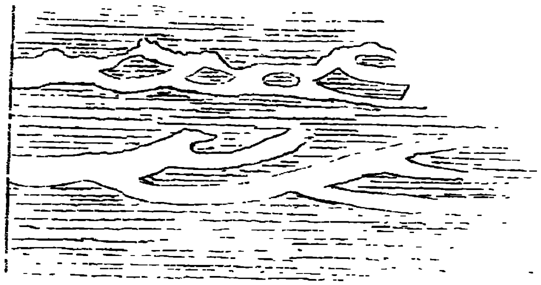
next: $2e^2 . \frac{\cos l . \sin m}{\sin\beta} \sin\frac{\lambda-\lambda}{2} . \cos\frac{\lambda+\lambda'}{2}\left(1 + \frac{e^2}{2}\sin\lambda\sin\lambda'\right)$ = 10″·26 and $\mu - m$ = 10″·257 as it ought to be. The equation for $\sin(\mu - m)$ is a complete check upon geodetical calculations of this nature; for if the parts used do not strictly belong to the same spheroid, the difference between the calculated value of μ and the given value of m will vary considerably from the value of the same difference by the formula.

The operations of the Trigonometrical Survey alluded to may be thus represented.—The quantities λ, λ', m, m' referring to Beachy Head and Dunnose, were assumed to be exactly known, and



General Appearance of the Quarries at Brotherton

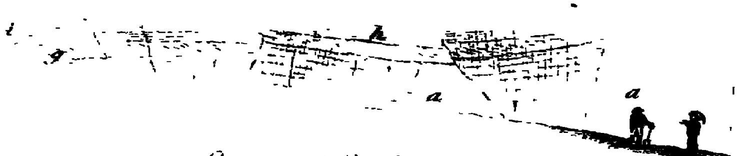
1



Layers of Gypsum in No. 5

3

2



Quarry at Brotherton

J. F. del. 1828

S. Porter sculp

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

DECEMBER 1828.

LXVII. *Geological Observations made in the Neighbourhood of Ferrybridge, in the Years* 1826–1828. *By* JOHN PHILLIPS, *Esq. F.G.S. Honorary Member of the Philosophical Societies of Yorkshire, Leeds, and Hull* *.

[With a Plate.]

TWO journeys to Ferrybridge, in company with the Rev. W. Bulmer, have furnished me with some observations on the geology of that neighbourhood, which but for many engagements I should some time since have had the honour of presenting to the Society. Several years have passed since I examined the range of magnesian limestone from Doncaster to Ferrybridge, and possessed myself of the general facts relating to the composition and succession of the several beds associated with it. My attention was again awakened to the subject by the freshwater shells and petrified wood found by the Rev. Wm. Richardson, during the excavation of the new canal through the alluvial soil in the valley of the Aire. Mr. Richardson obligingly offered to satisfy my curiosity on this subject by conducting me to the spot; and accordingly, in May 1826, Mr. Bulmer and myself availed ourselves of his friendly guidance. The same active inquirer furnished us occasion for another interesting visit in April 1828, by collecting some bones of antediluvian quadrupeds from a limestone quarry at Brotherton. The results of these examinations I now beg to offer to the consideration of the Society. Illustrative specimens may be consulted in the cabinets of the Institution.

The deposits in the neighbourhood of Ferrybridge lie in the following order:

1. Alluvial deposit of silt or fine clay in the valley of the Aire.

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* Read to the Yorkshire Philosophical Society, Nov. 4, 1828; and communicated by the Author.

2. Diluvial deposit consisting of pebbles and sand and clay spread indiscriminately over the surface of the strata.
3. Red and white marl with layers of gypsum.
4. Upper limestone in thin laminæ, with hardly a trace of magnesia; and few or no organic remains, except at the bottom.
5. Red and blueish-white clay with layers of gypsum.
6. Magnesian limestone in thick beds, some of which abound in organic remains.
7. Yellow or purple sand and sandstone used for moulding at the iron-works.

Beneath are the shales and sandstones of the West Riding coal district.

My remarks will be confined to the limestone and red marl formations, and to the diluvial and alluvial accumulations which cover them.

No. 6.—The yellow magnesian limestone is in this country a fine-grained rather powdery rock, never indurated to marmorean hardness as at Roche Abbey, nor composed of granular rhomboidal crystals as in the southern part of Nottinghamshire. It exhibits in one of the Weldon quarries a remarkable course of siliceous nodules, of an irregular oval figure, lying in one continuous bed of stone, like a layer of flints in a bed of chalk. No organic body is connected with them. When accidentally thrown into the limekiln they suffer fusion on the surface, in consequence of the lime which surrounds them, and become coated with porcelain or solid transparent glass. In this quarry the hollow sparry concretions so common in the magnesian limestone lie in layers in the upper part of the rock. No fossils are seen here; but between this place and Ferrybridge, Mr. Richardson collected a considerable number.

On the road from Ferrybridge to Pontefract, and near to the latter place, the magnesian limestone has been cut through in the road, and exhibits the hollow balls of calcareous spar in great variety and profusion. While I was occupied in extracting some of theſe from the south bank at the brow of the hill, an angular red substance fell down with the debris of the loose limestone, which upon examination surprised me very much; for it was *rock-salt.* (May 31, 1826.) My inquiries on the subject produced nothing satisfactory. The specimen is in the cabinet of the Yorkshire Philosophical Society.

In the same road, nearer to Ferrybridge, divided veins of sulphate of barytes may be seen crossing the loose beds of magnesian limestone in a north and south direction. This rock sinks regularly beneath the town of Ferrybridge, and is covered towards Knottingley by the other members of the magnesian formation. But there seems to be a considerable fault or dislocation in the valley of the Aire, which depresses the beds on the north side of that river; so that they have their outcrops further toward the west.

The fossils which I have observed in this rock in this neighbourhood, are the following:

Axinus obscurus	Min. Conch. of various magnitude, common.
Cardium........	N.S. one specimen.
Cucullæa.......	N.S. not plentiful.
Mytilus.........	N.S. This might be mistaken for *Gervillia*.
Terebratula ...	N.S. Differs from one in Durham, which resembles *T. sacculus*, Min. Conch.
Producta.......	perhaps a new species, decidedly unlike *P. calva*, or any other hitherto described from this formation.

No. 5.—The red blue and yellow clay, with gypsum of different sorts, which lies above the yellow magnesian limestone, was cut through in the new canal where the first bridge is thrown over it in Knottingley. We there saw the limestone No. 4. resting upon a layer of yellow clay or marl; below this a layer of blue clay, and under all the red clay with gypsum. A greater thickness of these beds is exposed in the gypsum quarries at Fairburn, on the north side of the Aire. The section at Lord Palmerston's quarry is very instructive, and easily accessible. Diluvial sand and pebbles lie on the surface of about 45 feet of the thin bedded limestone No. 4; below is yellow clay 2 feet in thickness, then blue clay 6 feet, and at the bottom 21 feet of red clay with irregular layers of coloured gypsum. This gypseous red clay ranges to the north and south of Ferrybridge, and in some places has been found 11 yards, or even, it is said, 15 yards thick. It contains no trace whatever of organic remains.

No. 4.—The limestone which is burnt at Knottingley and Brotherton for agricultural uses, has at the utmost a thickness of about 15 yards. In the quarries at Knottingley, 11 yards is above the average. The rock is laminated through its whole mass, but the lower layers are thicker. These are also in general less hard and less compact, hold a few sparry concretions, and some imperfect *Axini* and other bivalve shells. By these characters they seem analogous to the lower or magnesian rock; and accordingly the lime burnt from them is excellent for building, but useless or even injurious to land. I know not if the stone of these beds has been analysed. My friend Mr. George, Secretary to the Leeds Philosophical Society, found the upper part of the Knottingley and Brotherton limestone to be nearly pure carbonate of lime, with a very small proportion of carbonate of magnesia.

Vertical joints ranging N. and S. and nearly E. and W. cross each other, and divide the rock from top to bottom into vast rhomboidal prisms. The spaces left between their smooth sides are of different widths, and filled with various materials. The narrow fissures of this kind are sometimes nearly empty, or only partially occupied by soft fine clay introduced by water, and laid against the faces of stone: the wider ones are sometimes ten inches across, and filled by a confused assemblage of pebbles and clay, exactly similar to the general diluvial covering which is spread over the surface of the neighbouring country.

We must therefore suppose that at the time of the deluge these fissures were open in their full width, and admitted the diluvial debris. Their extreme regularity and parallelism, and the correspondence of their opposite faces, leaves no room for the supposition that they owe their existence to convulsions within the earth. Condensation of volume, in consequence of the process of induration, seems decidedly indicated by all the appearances.

The quarrymen at Brotherton sometimes meet with little irregular cavities, called *guts* or *brashes*, in the limestone, which are more or less distinctly connected with the vertical joints; and sometimes these cavities are filled with clay or with clay and pebbles, exactly like those in the fissures or "seams" themselves. Evidently, therefore, the brashes have been filled with pebbles from the seams, as these from the surface; and both at the same time, viz. the deluge.

Such holes have on several occasions yielded fossil bones in considerable plenty, but being neglected by the workmen they were not examined. Mr. Richardson being informed of an occurrence of this kind in the spring of 1828, took pains to collect the bones and observe the attendant circumstances. I had the pleasure of examining all the bones, and carefully repeated the investigation of their repository.

It was an irregular oblong space in the limestone a few feet in diameter, 10 yards below the surface of the ground, and connected directly with one long vertical joint ranging N. and S., and by a cross joint with another in the same direction. (Plate II. fig. 1.) Through these joints, I conceive, the pebbles and bones passed into the cavity. The bones belonged exclusively to oxen and deer. They were so friable that it was hardly possible to extract them in an entire state; but they became hard by exposure. Among them are the following characteristic parts:—

Ox.—Left condyle of the lower jaw. Astragalus.
Posterior molar tooth, lower jaw, left side.
Deer.—Base of the horn with a broken brow antler placed exactly as in the red deer.
Palmated portion of the horn, agreeing closely with that of the red deer.

Both specimens smoother and rather smaller than in the red deer.

Phalangial bone.
Part of the radius.
Upper part of a metatarsal bone.

The pebbles were numerous and various, and of different sizes, but none exceeded five inches in diameter. Those most easily recognised belonged to the following rocks:—

Dark mountain limestone.
Cherty sandstone, such as is dug near Leeds.
Sandstone with *Producta hemisphærica*, such as is found near Ripley and Harrogate.
Red sandstone of the carboniferous limestone series.
Clay and sand of different colours enveloping the bones.

The limestone quarries of Knottingley and Brotherton exhibit another phænomenon, of great interest to the geologist, and well known to the workmen from the loss and disappointment which it occasions:—I mean those masses of red and white clay called "*horses*"*, which range through the quarries, some twenty, and some a hundred yards in length, and from a few feet to many yards in breadth. On first viewing these "red horses," we are apt to imagine that they fill wide fissures in the rock, produced by ordinary dislocations, which uplift the strata on one side above those on the other: but this is not the case. They are indeed uniformly situated at the convergence of opposite declinations: but these are sometimes unaccompanied by any fracture in the upper beds; and the open space below, instead of being an ordinary fissure with parallel sides, widens greatly downwards like a wedge. In consequence of this peculiarity, the red horses do not always reach the surface of the limestone; and the workmen, after removing the upper beds of that rock, find with extreme vexation that they cover a subterranean mass of red clay. By tracing the course of one of these wedge-shaped dykes, we perceive that for some distance the limestone walls of the fissure are separate, and the red clay reaches to the diluvial covering; but further on, the limestone walls approximate, and finally unite above and exclude the clay from the surface. (See Plate II.)

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* A common term among workmen for dislocated and deteriorated strata.

The red and white clay in the deepest parts of a "horse" contains not a single pebble such as lie in the diluvium, but abounds in fragments seemingly very little worn, of the lower shelly bed of the limestone. At the surface, indeed, where the red clay and diluvium come in contact, the pebbles of the latter deposit are more or less mixed with the former; but still the appearances are such that no person can doubt the conclusion, that the diluvium was laid on the surface long after the red clay had been introduced into the chasm of the limestone.

Observing that the spaces which are filled by these red horses are not necessarily connected with the surface, but are often subterranean, and descend quite to the base of the limestone, while the long straight joints which were open to the surface are filled by other materials,—I am induced to believe that the red clay was forced into its present situation from beneath. In appearance it is very similar to the stratum which lies beneath the limestone: from all that can be observed, it is probably connected with it; and it is remarkable that the angular fragments which it contains belong chiefly to the lower shelly bed of the rock. In what manner such an upward flow of the red clay could be occasioned, is not easy to conjecture; nor will my silence on this subject be condemned by those who have learned from experience what difficulties attend the theory of ordinary dykes and mineral veins.

No. 5.—The red marl formation is seen reposing upon the limestone of Knottingley at the third bridge over the new canal through that village. It presents the usual appearance of red and blueish layers in frequent alternations, but contains, I believe, no gypsum.

No. 2.—Diluvium.

Over all the strata which have been mentioned, sand and gravel are extensively spread; but seldom accumulated in such vast quantities as in the lower country, which lies to the east and north-east. The surface of the upper limestone is in general regular, and the gravel upon it of nearly uniform thickness. In almost every quarry at Knottingley and Brotherton the fixed rock is covered by a variable layer of its own substance in the form of rubble, which sometimes is so slightly waterworn and so unmixed with other materials, that it appears to have been merely loosened and disturbed, but not transported. Above, we commonly find sand, with or without pebbles of sandstone and mountain limestone, resembling the varying beach thrown up by the sea; and then, in the Brotherton quarries, another layer of limestone rubble more mixed with pebbles from a distance. Sand and pebbles generally overspread the whole. The most curious circumstance relating to the arrangement of the diluvium is, that the rubbly limestone fragments are so deposited as to have their flat surfaces obliquely sloping toward the east, which could only have been occasioned by the continued action of a westerly current. Such a current is indicated by the nature of the sandstone and limestone boulders, which evidently belong to rocks in the western part of the county.

No. 1.—Alluvium.

The valley of the Aire is filled in its upper part with a variety of substances brought down by the stream and deposited in its floodway. Broken trees, pebbles, sand and clay, are met with in sinking on its banks. Nearer the sea this river flows through a flat country of silt or fine clay deposited by the tide, and protected by banks. The alluvial deposit at Ferrybridge and Knottingley is situated at the point to which the sea formerly flowed, and has probably been produced by land-floods and muddy tides. Its substance is a fine rather tenacious clay or silt, such as commonly subsides from agitated water.

The excavation for the new canal was continued for some length in this alluvium, and afterwards extended through the Knottingley limestone. When it approached the limestone, the workmen were surprised to find, at the depth of twelve feet, a quantity of hazel branches and roots, with abundance of nuts, some hawstones, green moss, bones, and shells. Their astonishment was increased when the hardness of the branches proved that they had become stone. Mr. Richardson collected the most interesting specimens, and presented duplicates to the Yorkshire Philosophical Society. The following is an enumeration of the varieties.

Shells.—None but freshwater kinds; as *Lymnæa putris*, *Cyclas cornea*, *Planorbis corneus.*

Nuts, in extreme perfection. Most of them were soft when first extracted, but they hardened by exposure. They were generally empty and light. But some few heavy specimens had undergone a singular change: the kernels were converted to solid white carbonate of lime, while the brown pellicle of the kernel remained unaffected, and the shells were not at all petrified. Sometimes crystallized calcareous spar was seen in the space between the pellicle and the inside of the shell.

Wood, believed to be chiefly *hazel roots.* Some of it is unaltered in substance, the rest more or less impregnated with carbonate of lime. Usually the central portion is lapidified, while the outward layers are unchanged: but sometimes the whole mass of wood is filled with stony particles, while the bark remains soft and pliable. One specimen shows the central wood petrified, the external layers decayed away, and the bark externally coated with pyritous carbonate of lime. Blue phosphate of iron, so common in alluvial deposits, colours some of the fragments of wood. The bark and soft wood and nutshells burn with a brilliant and continued light; even the fibrous stony substance emits a faint lambent flame.

Bones.—The lower jaw of a deer, about the size of the red deer, coloured by phosphate of iron.—Upper part of the tibia of an ox, in the sinuosities of which a hazel root has struck its branches, and over whose surface a pyritous layer is spread, like that which invests the bark of the specimen of wood already mentioned.

No other example has occurred to me of petrified nuts or wood in Yorkshire; and the cause of the peculiarity in this instance is not difficult of discovery. At Knottingley the vegetable relics were buried so near to the limestone rock that its calcareous springs obtained access, and in a long succession of years filled the interstices of the plants with a stony deposit. It is remarkable that the interior parts of the branches are often hardened to stone, while the bark and external layers of wood are not perceptibly altered; and that in the same manner the kernel of a nut should have become a mass of calc spar, while its shell has remained without any change of substance.

Except in the peculiar and interesting circumstance of the vegetable remains having become lapidified, little essential difference appears between this freshwater deposit and those in Holderness, which will be minutely described in my forthcoming publication on the Geology of the Yorkshire Coast; and in each case their high antiquity is proved by the deep sediment laid upon them. This sediment itself is of varying antiquity. It may conceal the most ancient and the most recent of the works of man;—a boat used in some distant ages of the world, as in Romney Marsh; or an English oar, coin, and horse-shoe, as in part of the excavation at Knottingley.

York, Nov. 3, 1828.

Reference to Plate II.

Fig. 1. A section representing the general appearance of quarries at Brotherton.

(*a*). The limestone rock, with straight fissures (*b*) and (*c*), (*c*), the latter filled with pebbles, clay, and sand, as also is (*d*) a "brash" connected with (*c*); (*e*) and (*f*) are red horses, the former not reaching to the top of the limestone, but the latter opening through it and covered by the diluvium.

(*g*) is the rubbly limestone with its fragments sloping to the east; (*h*) sand and pebbles of sandstone and limestone.

Fig. 2. Shows in a quarry at Brotherton a second layer (*i*) of limestone rubble above the sand and pebbles (*h*).

Fig. 3. Represents some irregular ramified layers of gypsum in red marl at Fairburn.

LXVIII. *On the Calculations requisite for predicting Occultations of Stars by the Moon*[*].

THE columns which are placed together on pages IV and VI of every month are to facilitate the approximate determination of the moon's daily motion. With regard to the manner of using them for the most frequent and most useful application of them, the prediction of the time at which the moon will occult a heavenly body, I beg here to give some further explanation on the most convenient form of the calculation.

Let α, δ, π, r denote the right ascension, declination, horizontal-equatorial parallax and semidiameter of the moon, as viewed from the centre of the earth, α', δ', r' the corresponding quantities for a point on the surface of the earth whose corrected latitude is φ, ϱ being the corresponding semidiameter of the terrestrial spheroid: next let θ signify the sidereal time, A, D the mean right ascension and declination of a star in the beginning of the year; A′, D′ the same for the apparent place of the star at another given time. The rigorous calculation of the time of the beginning and end of the occultation will require the following operations.

For at least two moments of time t, which are near the required one, the quantities α, δ, π, r, θ are to be deduced from the Ephemeris by interpolation; and from these the α', δ', r' are to be calculated. Developing the finite expressions into series, and neglecting the terms of the third order (π being considered as of the first), which are in most cases insensible, we have the following formulæ:

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[*] From Encke's *Astr. Ephem.* for 1830, p. 253.

$$\alpha' = \alpha - \frac{\rho . \sin \pi . \cos \varphi}{\cos \delta} \sin (\theta - \alpha)$$

$$- \tfrac{1}{2} \left(\frac{\rho . \sin \pi . \cos \varphi}{\cos \delta} \right)^2 \sin 2 (\theta - \alpha) - \ldots .$$

$$\text{tang}\, \gamma = \text{tang}\, \varphi . \frac{\cos \tfrac{1}{2} (\alpha' - \alpha)}{\cos \left\{ \theta - \alpha - \tfrac{1}{2} (\alpha' - \alpha) \right\}}$$

$$\delta' = \delta - \left(\frac{\rho \sin \pi . \sin \varphi}{\sin \gamma} \right) \sin (\gamma - \delta)$$

$$- \tfrac{1}{2} \left(\frac{\rho \sin \pi . \sin \varphi}{\sin \gamma} \right)^2 \sin 2 (\gamma - \delta) - \ldots .$$

$$r' = r + r \left(\frac{\rho . \sin \pi . \sin \varphi}{\sin \gamma} \right) \cos (\gamma - \delta) + \ldots .$$

From the calculations for two or more moments will be found the true parallactic motion of the moon in right ascension and declination $\Delta \alpha'$ and $\Delta \delta'$. The quantities A$'$ and D$'$ being likewise derived from A and D, we have, for the time of the beginning and end of the occultation, $t \pm \Delta t$; Δt being found by this quadratic equation

$$(A' - \alpha' - \Delta t . \Delta \alpha')^2 \cos \delta'^2 + (D' - \delta' - \Delta t \; \Delta \delta')^2 = r'^2$$

in which for r' the value corresponding to $t \pm \Delta t$ must already be adopted.

In the calculations for predicting occultations there is no preliminary knowledge of the times when they will happen. Numerous useless trials would therefore be necessarily made, if it were required to insure that no star had been passed over.

The distance of the two moments of time from which $\Delta \alpha'$ and $\Delta \delta'$ are to be calculated, may safely amount to an hour. The hour angle of the moon being $\theta - \alpha$, positive if west of the meridian, the calculations for parallax may be facilitated, if the motion of the moon is referred not to mean time, but to true lunar time. For if τ denote any hour angles (the western ones being positive), each differing 15° from the other during the greatest diurnal arc of the moon, consequently within about 15^h to 9^h, one may calculate for every τ a table with double entries δ and π, from which $\alpha' - \alpha$, $\delta' - \delta$, $r' - r$ may be immediately taken.

This would require altogether nineteen tables, which indeed would be only for one place, but of permanent use. We have here supposed that the quantities t, α, δ, r, π may be easily found for the different values of τ.

For this purpose are given the three columns on pages IV and VI, headed "Moon on the Meridian." They give the time and the place of the moon during her upper and lower culminations at Berlin; or if the sign ^{h} denote true hours of the moon, the time and place of the moon being referred to the centre of the earth, for 0^h and 12^h at Berlin. From these may be found by an easy interpolation, as only entire numbers and second differences enter into the calculation, time and place for

$$0^h, \pm 1^h, \pm 2^h, \ldots .$$

referred to Berlin; for any other place on the earth whose longitude from Berlin, positive if eastern, is l, these places and times would correspond to

$$\tau = 0^h + l, \pm 1^h + l, \pm 2^h + l, \text{ \&c.}$$

The deduction of these three columns is strictly founded on an interpolation with unequally increasing arguments. We know θ and α for every mean noon and midnight, and from these the times and α and δ are to be found which correspond to $\theta - \alpha = 0$, and $\theta - \alpha = 180°$. If the values of $\theta - \alpha$ are calculated for every noon and midnight, and are assumed as arguments of tables whose functions are respectively the mean half days, and α and δ from 12 to 12 mean hours, the formulæ explained in the paper "On Interpolation *" may be employed for finding the values of the above functions for $\theta - \alpha = 0$, and $\theta - \alpha = 180°$.

The greatest strictness, however, would not be required in these calculations. Even in the latest tables of the moon's motions there occur errors in longitude of $10''$, and the position of the smaller stars may possibly likewise be wrong by $5''$; and consequently the strictest calculation may still be liable to an error of half a minute in time. It is therefore not necessary in calculating the predicted occultations, to have the moon's place nearer than to 0·1 minute of a degree, and the time nearer than to 0·1 minute of time. Under these circumstances it will be sufficient, in deducing the time of the culmination from the values of $\theta - \alpha$ formed as above described, to use an indirect method by which, agreeably to the paper "On Interpolation," the mean days being considered as arguments, and the $\theta - \alpha$ as functions, the first differences of $\theta - \alpha$ are so corrected by an approximate estimation of the factor of correction, that they may be simply regarded as divisors, and the times of the culminations are then found by the quotients

$$\frac{\alpha - \theta}{\Delta(\theta - \alpha)} 12^h \text{ and } \left(\frac{\alpha - \theta - 180°}{\Delta(\theta - \alpha)} + 1\right) 12^h$$

In this manner the times of the culminations have been found accurately to 0·1 minute of time; an accuracy which has caused no greater trouble than the common results would have required, as it is always necessary to use logarithms.

By means of these times the corresponding places of the moon have been calculated from the differences of α and δ, which had already been calculated in examining the correctness of the tabular values. In general the limit of accuracy may be taken at $0'{\cdot}1$. But with regard to the δ in the places of the greatest changes, especially in the three first months, errors of $0{\cdot}3$ may occur, arising from the circumstance of these calculations having in the beginning only been made for the upper culmination. Greater deviations, if any should be found, must have been introduced by misprints.

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* This paper will appear in our next Number.—Edit.

In deducing the places for the single values of τ from the culminations, it appears unnecessary to go further than to second differences.

The tables for calculating the parallaxes for every value of τ will, however, considering the quick change of δ, not be of the most convenient form if made with double entries; and I have likewise in this case found more convenience in the method so frequently and successfully applied by Gauss, of avoiding the double entry by a table of single entry and a short logarithmic calculation.

As the moon's place is only known to $0'{\cdot}1$ of a degree, the parallaxes are only required to be known to $0'{\cdot}01$, in order to be quite sure that by them no other error shall be introduced. It will for this purpose be sufficient to have logarithms with only four places of decimals, which everybody may easily compile. Ten duodecimo leaves thus contain to the most convenient extent the numbers, the trigonometrical functions, and Gauss's auxiliary tables for addition and subtraction.

The form which I have chosen is this: For every τ from 15^h to 9^h I have calculated for all values of π from $10''$ to $10''$ within the limits of $\pi = 53'$ and $\pi = 62'\,10''$:

$$a = -\varrho \,.\, \sin\pi \,.\, \cos\varphi \,.\, \sin\tau \,.\, 3437{\cdot}75$$

$$\operatorname{tang}\gamma = \operatorname{tang}\varphi \frac{\cos\frac{1}{2}(\alpha'-\alpha)}{\cos(\tau-\frac{1}{2}(\alpha'-\alpha))}$$

$$\sin\gamma$$

In calculating γ, instead of $\frac{1}{2}(\alpha' - \alpha)$, has been adopted $\frac{1}{2}a \,.\, \sec \,.\, 21°$, the value of a being the one corresponding to $\pi = 57'\,40''$; sec 21° is about the mean value between the greatest and smallest sec δ which can take place for the moon. The adoption of these mean values cannot sensibly change the result of the calculation. A table was formed of the logarithms of a and b to four places of decimals; in these tables the minute of a degree was considered as unity. Next the quantities of the second order were arranged in tables of double entry, δ from $-30°$ to $+30°$ for every fifth degree, and π from $53'$ to $62'$ for every third minute:

$$a' = -\tfrac{1}{2}\left(\frac{\varrho\,.\sin\pi\,.\cos\varphi}{\cos\delta}\right)^2 \sin 2\tau \qquad .3437{\cdot}75$$

$$b' = -\tfrac{1}{2}\left(\frac{\varrho\,.\sin\pi\,.\sin\varphi}{\sin\gamma}\right)^2 \sin 2\,(\gamma-\delta)\,.\,3437{\cdot}75$$

$$\Delta r = +\,r\,.\left(\frac{\varrho\,.\sin\pi\,.\sin\varphi}{\sin\gamma}\right).\cos\,(\gamma-\delta)$$

The values are expressed in hundredth parts of minutes of a degree. By this means we find

$$\begin{aligned} \alpha' - \alpha &= a\ \sec\ \delta + a' \\ \delta' - \delta &= a\,.\sin(\gamma-\delta) + b' \\ r' - r &= \Delta r \end{aligned}$$

almost as accurately as it is required for the most exact calculation, as the error will very rarely amount to $1''$.

Besides these values, the parallactic velocity of the moon, as it may be called, or the quantities $\Delta\alpha'$ and $\Delta\delta'$ are required. The first differences of α and δ give her velocity in right ascension m, and in declination n, with reference to the centre of the earth; and as differences higher than the second are neglected, we may assume that for $\tau = 6^h$ and $\tau = 18^h$ the first differences are $12m$ and $12n$. The unity of time is here the true lunar time, whose ratio to mean time is determined by the first differences of the times calculated.

In the values for $\alpha' - \alpha$, $\delta' - \delta$, the three variable quantities are π, δ, and τ. The variation of the terms of the second order will never be of consequence; and in those of the first, π may without hesitation, and even δ may, be considered as constant. The influence of the latter assumption would only become sensible if the time of the beginning or end of the occultation should be far distant from the lunar hour assumed for τ; in which case, however, the first differential quotients only would likewise not be sufficient. If therefore we calculate for every τ, neglecting smaller quantities,

$$\begin{aligned} m' &= -\,\varrho\,.\sin\pi\,.\cos\varphi\,.\cos\tau\,.\,900 \\ n' &= -\,\varrho\,.\sin\pi\,.\cos\varphi\,.\sin\tau\,.\,900 \end{aligned}$$

and arrange their logarithms to four places of decimals for every π, we shall have very nearly

$$\begin{aligned} \Delta\alpha' &= m + m' \sec\delta \\ \Delta\delta' &= n + n' \sin\delta. \end{aligned}$$

It now remains only to facilitate the calculation of A$'$ and D$'$ from A and D; as the errors arising from neglecting this calculation, especially towards the end of the year, would far exceed the errors arising in the most unfavourable cases from the calculation, as here described, of the apparent place of the moon. With this view it is only necessary to remark, that a star in occultation can be distant from the apparent place of the moon only 15′, and from the mean place of the moon only 1° 15′. Within the limits of the moon's declination it may be assumed, that stars which are so near each other have perfectly the same corrections of their places. If we therefore consider the mean place of the moon as the position of a star, and calculate by means of the auxiliary table in the Ephemeris for every superior culmination throughout the year, the correction which must be applied to the mean place of a star of the same right ascension and declination, in the beginning of the year, in order to have the apparent place for the time of the upper culmination,—we have a table giving very nearly the apparent place of each star, as far as it can be occulted by the moon. These are the quantities which, in the tables headed "Auxiliary Tables for the Occultations of Stars," have been denoted ΔA and ΔD; so that taking ΔA and ΔD for the given moment of time from them, we have for all stars occulted

$$A' = A + \Delta A$$
$$D' = D + \Delta D.$$

In calculating these tables, as the time pressed, some of the small terms have been omitted, and only these forms have been assumed:

$$\Delta A = f + h \sin (H + \alpha)$$
$$\Delta D = i + g \cos (G + \alpha).$$

The declination of the moon will not exceed ± 19° in this year; and the terms multiplied by tang δ and sin δ will become small; likewise sec δ and cos δ may be put $= 1$.

In order now to reduce everything to a certain form, which greatly facilitates calculations which are to be frequently repeated, we may abbreviate the solution of the quadratic equation by introducing trigonometrical auxiliary quantities; and with this view the following expressions may be used:

$$\begin{aligned} \text{For } \tau = &\ \ 0^h + l \\ & \pm 1^h + l \\ & \pm 2^h + l, \text{ \&c.} \end{aligned}$$

the following quantities may be arranged in a table with single entry: log a, log b, γ, r, m', n', the argument being π for every tenth second: the quantities a', b', Δr for every τ, will form tables with double arguments δ and π.

Thus *ex. gr.* that part of the general table for Berlin (latitude 52° 31′ 15″, ellipticity $\frac{1}{302 \cdot 78}$), which is used for the occultation of 82 Leonis on the 5th April 1830, is as follows:

21^h

π	log a	log b	γ	r	log m′	log n′
54′ 0″	1·3670	1·6862$_n$	61° 27′·7	14′·72	0·7850$_n$	0·7850
10	1·3684	1·6876$_n$		14 ·76	0·7864$_n$	0·7864
20	1·3697	1·6889$_n$		14 ·81	0·7877$_n$	0·7877
30	1·3710	1·6902$_n$		14 ·85	0·7890$_n$	0·7890
40	1·3723	1·6915$_n$		14 ·90	0·7903$_n$	0·7903
50	1·3737	1·6929$_n$		14 ·94	0·7917$_n$	0·7917
55 0	1·3750	1·6942$_n$	61 27′·9	14′·99	0·7930$_n$	0·7930

a′

	π =	53′	56′	59′	62′
δ =	0°	+0′·15	+0′·17	+0′·19	+0′·21
	± 5	+0·15	+0·17	+0·19	+0·21
	10	+0·16	+0·17	+0·19	+0·21
	15	+0·16	+0·18	+0·20	+0·22

b′

	π =	53′	56′	59′	62′
δ =	−10°	−0′·20	−0′·22	−0′·25	−0′·27
	− 5	−0·24	−0·27	−0·30	−0·33
	0	−0·28	−0·31	−0·34	−0·38
	+ 5	−0·30	−0·34	−0·38	−0·42
	+10	−0·32	−0·36	−0·40	−0·44

Δr

	π =	53′	56′	59′	62′
δ =	−10°	+0′·06	+0′·07	+0′·08	+0′·09
	− 5	+0·08	+0·09	+0·10	+0·11
	0	+0·10	+0·11	+0·12	+0·13
	+ 5	+0·11	+9·12	+0·14	+0·15
	+10	+0·12	+0·14	+0·15	+0·17

For the whole Berlin lunar hour which is contained in τ, we take from the Ephemeris, pages IV and VI, the mean time T, α, δ, the length of the true lunar hour $60' + \Delta t$, as also *m* and *n*. The first quantities are to be taken with due regard to the second differences, where the interpolation requires only these convenient factors $\frac{1}{12}$ $\frac{1}{6}$ $\frac{1}{4}$ $\frac{1}{3}$ $\frac{5}{12}$ $\frac{1}{2}$.

From the auxiliary tables for occultations of stars we have π, ΔA and ΔD; and from the catalogue of stars, A and D.

We have then to calculate

$$\begin{array}{ll}
\alpha' = \alpha + a \sec \delta \quad + a' & \\
\delta' = \delta + b \sin(\gamma - \delta) + b' \;, & r' = r + \Delta r \\
\Delta\alpha' = m + m' \sec \delta, & \Delta\delta' = n + n' \sin \delta \\
A' = A + \Delta A \;, & D' = D + \Delta D
\end{array}$$

If we now put

$$\begin{array}{ll}
(A' - \alpha') \cos \delta' = N \sin \eta & \Delta\alpha' \cos \delta' = M \sin \zeta \\
(D' - \delta') \quad\quad = N \cos \eta & \Delta\delta' \quad\quad = M \cos \zeta
\end{array}$$

where the different signs determine the quadrants in which η and ζ are, (N and M being always considered as positive,) and if we likewise determine i by this equation: $\frac{N \sin(\eta - \zeta)}{r'} = \sin i$, we have for the mean time of the beginning and end of the occultation $T + t + l$, the double value of t in minutes of time being derived from this equation:

$$t = \frac{60 + \Delta t}{M} \left\{ N \cos(\eta - \zeta) \mp r' \cos i \right\}$$

The upper sign is for the beginning, the lower for the end of the occultation.

If $N . \sin(\eta - \zeta) > r'$, no occultation takes place: the moon passes to the north of the star if $\sin \eta - \zeta$ is positive, to the south if $\sin(\eta - \zeta)$ is negative. For the place of the star's disappearance and reappearance on the moon's limb, the angle u, counted from the northernmost point of the moon's disk through east, south, and west, is found by these equations:

$$r' \sec \delta' \sin u = A' - \alpha' - \Delta\alpha' \left(\frac{t}{60 + \Delta t} \right)$$

$$r' \cos u = D' - \delta' - \Delta\delta' \left(\frac{t}{60 + \Delta t} \right)$$

where, with rare exceptions, u must be taken for the disappearance in the two first, for the reappearance in the two last quadrants. As an approximate value of u is only required, the second formula will be sufficiently accurate.

Among the various values of τ, it is most advantageous to take the one which lies nearest to the true conjunction of the star with the moon in right ascension. The choice of this value is facilitated by the consideration, that for positive values of τ the apparent right ascension is smaller than the true one; for negative ones greater; or that the parallax in right ascension has a contrary sign to τ. If, however, the true conjunction should happen in the middle of a lunar hour, it would be necessary, in order to attain all the accuracy of which the method admits, to make the calculation for the two hours between which it lies, and to take out of the two values, always the one which is nearer to a lunar hour, if one would not besides apply for this case a simple interpolation.

As an example, I add the complete calculation of the occultation of 82 Leonis, where every one will easily perceive the signification of the numbers from the auxiliary and parallax tables.

1830, April 5.

82 Leonis.	169° 13′·70	+4° 14′·29
	+0 ·41	−0 ·21
	169 14·11	+4 14·08

$\tau = 21^{\rm h}$ $\pi = 54' \, 11''$

7$^{\rm h}$ 20′·9	168° 47′·8	+ 4° 40′·8	+ 29′·31	− 9′·35
1·3685	1·6877$_n$	61 27·7	0·7865$_n$	0·7865
0·0014	9·9225	56 46·9	0·0014	8·9116
+ 23·44	− 40·75	14·76	− 6·10	+ 0·50
+ 0·16	− 0·31	+ 0·11	+ 23·21	− 8·85
+ 23·60	− 41·06	+14·87	log. 61′·8...	1·7910
			log. cos δ′...	9·9989
169 11·40	+ 3 59·74	259° 45′·6		
+ 2·71	+ 14·34	9·9930^n	9·2499^n	+ 12·1
0·4319	1·3646	9·9918	1·5609	+ 14·8
1·1565	0·9469$_n$	9·4146	− 6·5	1·0828
10 40·6	110 55·0	1·5691	7 14·4	1·1703
9·9924	9·9704		+ 9·6	1·1723
1·1641	1·3942	Disappearance	7 4·8	35°
1·1723	0·3968	Reappearance	7·24·0	

The rigorous calculation has given

Disappearance	7$^{\rm h}$ 4′ 21″
Reappearance	7 24 16

The calculations for the Ephemeris may be facilitated by some auxiliary tables.

With this view I have calculated by interpolation the position of the moon for every third lunar hour. This table, while it increases the safety, reduces besides the calculation of the rising and setting of the moon, every one of which would otherwise have required a separate calculation, to a mere copying of figures. For if we write the table from which we take for any given declination the semidiurnal arc of the moon, in such a manner that the lunar hours of the form $3n$ are always taken separately, and the excess of the semidiurnal arc above these lunar hours is converted by the mean motion of the moon into mean time, we have without any further correction the times of rising and setting, more accurately than they are ever required for any purpose. A second auxiliary table for occultations of stars, is a table which contains the time in which the moon is above, and the sun below the horizon.

The times of rising and setting of both these heavenly bodies being placed opposite to each other, this is likewise only copying of numbers.

A third auxiliary table contains the stars which within the largest possible limits may be occulted. For this purpose I sought for the beginning, middle, and end of the year, the declination which the moon has for every 5° of right ascension, and took in all the stars which are 40′ further north, and 1° 40′ further south, than the northernmost and southernmost limits of those zones.

By means of these tables the decision of the possibility of an occultation is in general so much facilitated, that besides the 130 occultations given in the book, only two were calculated in which the moon remained distant somewhat more than 2′, the limit to which I had proposed to myself to find out the conjunctions.

LXIX. *On the Height of the Aurora Borealis above the Surface of the Earth; particularly one seen on the 29th of March, 1826.* *By* John Dalton, *F.R.S.**

APPREHENDING that the Royal Society will favourably receive accounts that have a direct tendency to determine the height of that interesting phænomenon, the Aurora borealis, I have been induced to transmit some observations that were made upon a very remarkable one, which appeared in the evening of the 29th of March, 1826. From some recent observations, an opinion seems to be entertained by some writers, that the aurora is not so high as has generally been estimated; but it is only from facts and observations such as the following, I conceive, that any near approximation to the true height can be obtained.

The aurora borealis above mentioned, was of a kind very rarely occurring. It assumed the appearance of a rainbow-like arch, stretching across the mid-heaven, at right angles to the magnetic meridian. It was subject to very little change of position for an hour or more, and therefore afforded time to observe the angle of its elevation above the horizon. In the period of five years' observations at Kendal formerly, above one hundred appearances of the aurora occurred to me, and only one of the kind just described. I had not an opportunity of seeing the one which is the subject of this paper, but it was seen here (at Manchester) by a friend of mine about 9 o'clock on his returning home from a visit to me. He did not indeed observe the luminous arch, either from its having vanished, or from the obscurity of our atmosphere; but he remarked some beams or corruscations in the north-western hemisphere, of a low altitude; and not having seen an aurora for a long time, he induced the family at home to go out and catch a glimpse of the phænomenon, now much more rarely seen than formerly.

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* From the Philosophical Transactions for 1828. Part II.

A few days afterwards I accidentally noticed a paragraph in the Lancaster Gazette describing the luminous arch of the aurora, as well as the accompanying appearances; and as such a striking and unusual phænomenon could not fail to attract general attention, I examined the provincial newspapers and other periodicals of the time, and took occasion soon after to make inquiries personally, or by writing, of such individuals of judgement as had seen the phænomenon in various places near the line of the magnetic meridian. The result was, a collection of a more complete and extensive series of observations than was ever before made, in all probability, towards determining the height of the luminous arch of the aurora.—I shall now proceed to detail some of the particular observations.

The accounts represent the arch to have been seen in places 170 miles distant in a north and south direction, and forty-five miles distant in an east and west direction, comprising an area of seven or eight thousand square miles; but it must have been much more extensively visible, as in most cases the writers of the different accounts describe their situation as central with regard to the phænomenon. It was seen at Edinburgh and Leith, Kelso, Jedbergh, and Hawick in Scotland; at Carlisle, Penrith, Keswick, Cockermouth, and Whitehaven in Cumberland; at Kendal and at Kirkby-Stephen in Westmorland; at Lancaster, Preston, Warrington, and Manchester in Lancashire; and at Doncaster in Yorkshire. Descriptions of the phænomena as seen at most of these places were immediately given in the newspapers of Lancaster, Kendal, Carlisle, Whitehaven, Kelso, &c., and some of these accounts were copied into the London papers soon after.

All the accounts that I have seen from places between Lancaster and Edinburgh, as well as at these two places, agree that a luminous arch was first seen about 8 o'clock in the evening; that it continued without much motion for an hour nearly, and then gradually vanished, leaving the northern sky illuminated as usual after an aurora borealis of the common kind; so that it seems impossible to doubt that the same arch was seen at all the places of observation, and at the same time.

A good description of the phænomenon was published by Messrs. Coldstream and Foggo in the Edinburgh Journal of Science for June 1826; it is as follows:

"March 29th. Immediately after the fading of the evening twilight, at 8^h 15^m P.M., a bright luminous ray was seen to rise from the eastern horizon, gradually to extend itself towards the zenith, and thence towards the western horizon, presenting, when completed, the appearance of an arch of silvery light, similar to that seen here on the 19th March, 1825.

"When first formed it was a few degrees to the north of the zenith of this place; the light in the centre was rather diffuse; its edges were irregular; and the western limb had, as it were, a plumose appearance. It soon evinced a decided motion towards the south, and in a few minutes reached our zenith. Its edges were now sharply defined, and throughout its whole course it was nearly uniform in appearance and breadth; the intensity of its light in the zenith had increased, while in the same quarter the breadth had considerably diminished.

"The direction it now had was very nearly at right angles with the magnetic meridian.

"At ½ past 8, faint beams of the aurora began to rise from the northern horizon, and at one time promised to form a splendid display; but the corruscations never became very vivid; they were not rapid in their motions, and did not flit along the horizon.

"The arch still continued its motion towards the south, and in 15 minutes passed through a space of about 20°. Its southern edge reached a point about 24° or 25° south of the zenith, beyond which it did not go. The light now became gradually fainter, and at length disappeared.

"Meanwhile the aurora in the north continued to play, but with no increase of vividness. For some minutes, soon after 9 o'clock, we observed broad bands of light, having their longer axes (which generally subtend angles of about 18° or 20°) parallel with the horizon, darting with great velocity across the illuminated space from east to west and from west to east. These formed, ran their course, and vanished in a moment; they had no vertical motion, but they appeared at various degrees of elevation, never higher however than 30°. Soon after this interesting (and perhaps unusual) display, the beams disappeared, and nothing was left but a diffuse luminousness along the horizon."

At Jedburgh, Hawick and Kelso, places about forty miles south of Edinburgh, the phænomena were much the same as above, as appears from the Kelso Chronicle. (See also the London Courier, April 7th, and other of the daily papers.) At Jedburgh the arch is said to have commenced at 8^h 15^m on the W. by S. point of the horizon, to have passed south of the star Aldebaran, between Castor and Pollux, and over Arcturus; its altitude 60° from the S.; waves of light seemed to run along the arch. At 8^h 30^m the whole advanced 20° to the S. At Hawick it was at first 20° S. of the zenith, and at 8^h 40^m it was stationary at 37° S. of the zenith; the arch passed 6° N. of Arcturus, 7° S. of Cor Caroli, 6° N. of Coma Berenices, through the hind foot of Ursa Major, 4° N. of Asellus Borealis, 6° S. of Pollux, through the head of Monoceros, through the three stars in Orion's girdle, and 1° S. of Rigel. From this it would seem that the arch, instead of appearing low in the north from the last-mentioned places, as it must have done if situated only five or even ten miles above the earth's surface, appeared as far to the south of the zenith as at Edinburgh, or rather further. This latter it could not do; and in such circumstances it is reasonable to allow a difference of a few degrees in the estimates of altitudes of arches neither well defined nor absolutely fixed, and possessing several degrees of breadth; but it clearly shows the arch was not low. The author of the Hawick account signs, Gideon Scott.

At Carlisle, seventy-five miles S. of Edinburgh, the phænomena were much the same as in the preceding accounts. See the two weekly newspapers of that city.

About Cockermouth, twenty-five miles S. of Carlisle, I conversed with many persons who had seen the phænomena. One young gentleman, Mr. Harris, had committed to paper at the time some notes upon it, with which he favoured me. According to these, he first saw the arch at 7^h 45^m P.M., it extended nearly from the western to the eastern horizon, through the W. part of the head of Orion, over Castor and Pollux, S. of Ursa Major, and ended in Corona Borealis; it continued with little variation in its situation till near 10 o'clock. At first the west end of the arch was most luminous, and finally before it vanished the east end was the most brilliant. The eastern end waxed and waned frequently. The sky was very clear, a few streamers appeared low in the horizon.

At Keswick, about twelve miles east of Cockermouth, the appearance was described as follows, in a letter to me from Mr. Otley.—This gentleman is known to the public by an elegant little description of the Lakes and Mountains of the North of England, and is familiar with observations relating to meteorology, and to the angles of elevation of objects. "About 8 P.M., a luminous arch appeared very brilliant; the outside of the curve seemed a little south of the zenith. The eastern end tapered to a point above the horizon; the western end was broader, and lost in a cloud which rested on the mountain. It disappeared about 10 o'clock."

At Whitehaven, one hundred miles from Edinburgh, and a few miles more to the westward, a minute description of the phænomenon was given in one of the newspapers of that place, by Mr. Holden, lecturer on astronomy, who happened to be there at that time. At 8^h 45^m the east leg of the arch covered α Coronæ Borealis, the northern edge of the bow touched Castor near its greatest altitude, and the west leg went over the three small stars marked λ in the head of Orion. The breadth at greatest altitude was 4° 40′, but tapered down to the horizon, where it was not more than one-fourth of that breadth. The east leg was 15° north of the east point, and about the same number of degrees south of the zenith; and the west leg was 15° south of the west point. At 9^h 8^m the arch had moved southward, Pollux touched the north of the bow, the west leg extended over α Orionis, and the east leg was still upon α Coronæ Borealis, but this star had been moving in its apparent track by the earth's motion for the space of twenty-three minutes. He saw several small clouds move before and cover portions of it for a few seconds of time.

From Kirkby-Stephen, about forty-five miles east of Whitehaven, a good description of the phænomenon is given in the Westmorland Gazette. The mean breadth of the luminous arch exceeded that of the rainbow, the vertex broader, the extremities narrower, and the light more dense. The arch gradually faded about 10 P.M., having existed nearly two hours. The light was white and transparent. Position at 9 P.M., the arch of a great circle from E. 25° N. through the zenith to W. 25° S. At first the eastern extremity of the arch was near β Herculis, thence it passed the north side of Corona Borealis, through the midst of the seven stars in the Great Bear, over the zenith to the north of Castor, exactly over Bellatrix, after which it contracted to a point in Eridanus just above west. This writer makes no mention of any appearance of the common aurora borealis at the same time.

Accounts from Penrith were much the same as the preceding ones; but I had no opportunity of seeing any of them.

At Kendal, which is 110 or 115 miles S. of Edinburgh, and very nearly on the same magnetic meridian, (consequently the same part of the arch must have crossed the meridian at both places,) the following is a description of the phænomena as they appeared there, and might have been adopted with very little error, it should seem, for that at Edinburgh or any one of the intervening places, except as to the altitude of the summit of the arch. " A most magnificent meteor was observed here between 8 and 9 o'clock. The appearance was that of a luminous arch, stretching quite across the heavens. Its direction was that of the magnetic east and west, intersecting the magnetic meridian at right angles. At the same time a splendid light was observable in the northern horizon. This meteor was similar in some particulars to one which appeared a few years ago." [Query in 1819?] " The arch itself appeared like two frustums of cones, with the less extremity in the horizon, and their bases meeting in the zenith. The densest parts of the bow were those near the horizon, and the west end the denser of the two."

The phænomenon was seen at Lancaster, twenty miles S. of Kendal, and 130 miles S. of Edinburgh; it was described in the next Lancaster Gazette, but without being specific as to the altitude of the centre of the arch. Inquiry having been made of an intelligent medical gentleman who had seen it, he described the luminous arch as extending from east to west across the zenith, the light increasing in intensity from the arch of the zenith to the line of the horizon; there were those faint corruscations which usually attend an aurora borealis. This was about 8 o'clock: at $10^h\ 30^m$ P.M. there was a luminous appearance along the northern horizon.

The aurora was seen at Preston, twenty miles S. of Lancaster; but I have not been able to learn the particular appearances at that place. It was also seen at Doncaster in Yorkshire, but I have not noticed any description of its appearance at that place.

At Warrington the luminous arch was seen by a friend of mine, Mr. Joseph Crosfield, who was so obliging as to give me interesting information on the subject, both verbally and by writing. He saw the arch about 9 o'clock, or between that and 10, in company with two other persons, to whom he pointed it out at the time. At the first glance he took it for the milky way, but soon discovered his mistake. The direction of the arch was from W.S.W. to E.N.E. passing to the north of the zenith. The western branch was longer and more brilliant. He saw no northern lights at the time, neither did he apprehend the phænomenon was connected with them. On elevating the pole of a celestial globe till the axis passed through a series of angles with the horizon, I desired him to fix upon an elevation which he judged most nearly to coincide with the elevation of the centre of the luminous arch. On examination, the angle was found to be 61°. I then fixed the axis at 70°; this he was almost certain was too high. When it was fixed at 50°, he was still more certain it was too low.

The aurora was seen at Manchester, as has been stated; but it does not appear to have attracted much attention at this place. I have not been able to trace any account of the phænomena having been seen further south.

These are all the material observations I have collected; from which it must appear that the descriptions every where given evidently apply to the same luminous arch. In proceeding from north to south we find the arch gradually advancing in altitude, always crossing the meridian to the *south* of the zenith, till we arrive about Kendal, at which place it crossed nearly in the zenith, and when at Warrington its culminating was to the *north* of the zenith. It is further remarkable, that in all the places the arch seemed to terminate nearly in the magnetic east and west, or at two opposite points of the horizon; these facts indicated the great height and extension of the arch.

In order to apply the data to calculate the height of the arch, it is evident that observations at the extremities of the magnetic meridians are to be preferred, and those on or near the same meridian, all other circumstances being the same. Unfortunately, the Edinburgh and Hawick observations do not harmonize together: however, those at Jedburgh, a place nearly of the same latitude as Hawick, seem to show that both the others are wrong, or rather perhaps, that they had not been cotemporary with each other and the rest of the observations. The Hawick altitude is probably too low, and that at Edinburgh considerably too high.

In this uncertainty we may be allowed to take the observations at Whitehaven and Warrington as guides. Those places are very nearly on the same magnetic meridian; they are distant eighty-three miles, giving an extensive base: the observations were nearly cotemporary, and made on the same part of the arch, the altitude at Whitehaven being 75° from the south, and that at Warrington 61° from the north. From these data, I find the height of the arch very nearly one hundred miles above the earth's surface, and its position vertical about Kendal and Kirkby-Stephen, which accords well with the observations at those places. This conclusion is corroborated by the observations at Jedburgh and Warrington, where, if we take the angles of elevation at 60° and 61° respectively, and the distance on the magnetic meridian 120 miles, the height will be found between 100 and 110 miles. But, lastly, if we assume the angle at Edinburgh to be correct at 65°, and that at Warrington at 61°, the height comes out 150 or 160 miles, and its position vertical about Carlisle, which is in opposition to the general tenor of the rest of the observations.

As for the heights of the streamers or vertical beams seen low in the north, we have no sufficient data for determining it. But it is evident that the beams which were seen low at Edinburgh were the same as those seen still lower at Cockermouth, Kendal, Lancaster, and Manchester, at which last place the angle was about 10°, as my informant says. Now an object elevated about 25° from the north at Edinburgh would apparently be 10° or 12° at Manchester, if its real height were about one hundred miles above the earth's surface.

On the whole, I think it is fairly to be inferred that the height of the arch could not differ much from one hundred miles; and that its breadth would be eight or nine miles, and its visible length in an east and west direction, from any one place, would be about 550 miles.

Observations on other Auroræ.

The height of a luminous arch calculated by the late Mr. Cavendish, F.R.S. in the Phil. Trans. for 1790, is entitled to notice. It was found to be betwixt fifty-two and seventy-one miles. The observations, however, were made at too small a distance from each other to admit of precision. A base of at least forty or fifty miles seems necessary, where the object to be measured is generally neither steady nor well-defined.

The luminous arch seen at Keswick and Kendal by Mr. Crosthwaite and myself, on February 15th, 1793, was calculated to be 150 miles high; but this was from a base of only twenty-two miles. (See my Meteorological Observations and Essays, page 69.)

Dr. Thomson has given a brief history of the Aurora borealis in the Annals of Philosophy for 1814, vol. iv. He has copied a table from Bergman, being estimates of the heights of about thirty auroræ observed during the last century, calculated from observations made by different persons in various places. According to these results, the auroræ would seem to be of variable heights, from 130 to 1000 or more miles. The places of observation are often unsuitably situated; and the data from which the calculations were made not being given, I apprehend the great differences in the heights arise more from defects in the observations than from real differences.

In the same volume Mr. Longmire gives a description of a luminous arch seen at Troutbeck near Kendal, on the 11th of September, 1814. It was similar to that above described, and

was

was most extensively seen: namely, at Glasgow, Dumfries, and Annan in Scotland; at Dublin and Newry in Ireland; and at Whitehaven, Carlisle, Kendal, Lancaster, Warrington, and Liverpool in England. It was accompanied with the usual appearances of the aurora borealis, or streamers distant in the north. The observations are insufficient for calculating the height. I find in my journal the aurora was noticed at Manchester that evening, but no particulars are given. Mr. Longmire mentions a similar arch seen at Kendal and Dublin on the 17th of April the same year. An aurora was seen in London at the same time. (Annals of Phil. vol. iii. p. 400.)

1819, October 17th.—A remarkable aurora borealis was seen this evening in very distant parts of England and Scotland. Mr. Otley of Keswick first drew my attention to this, by communicating the notes he made at the time upon it, on the occasion when he favoured me with his remarks upon that of the 29th of March, 1826. After which I collected such other accounts as I could meet with from the journals of the time. The series of observations is as follows:—

Annals of Philosophy, vol. xiv. p. 472. Account from Newton-Stewart, (Scotland,) October 18th.—" A singular and beautiful phænomenon appeared in our atmosphere here last night (17th), about 8 o'clock: it was a bow or arch of silvery light stretching from east to west, and intersecting the hemisphere [meridian] at a few degrees to the southward of the zenith. After it had remained very bright for twenty minutes or so, dark blanks were first observed to take place here and there, and then, after expanding a little in breadth and shifting for a short way further to the southward, it disappeared. Some time before its appearance the atmosphere had been very cloudy; but when it was formed the sky was free from clouds, except towards the horizon to the westward and northward, where they hung very dark and heavy.—It was strikingly different from any of the usual forms of the boreal lights, which too were seen very vivid in the course of the evening."

Keswick.—Mr. Otley's account:—" About 7 P.M. (the 17th), a dense cloud appeared in the horizon to the N.N.W. bounded by a bright line, the rest of the heavens being starry. Presently beams of an aurora began to shoot towards the Great Bear. About 8 o'clock a luminous arch extended from west to east, the crown of the arch at first appeared to me a little to the north of the zenith, and after some time to the south of it, and again more northerly before it disappeared, which it did suddenly, a few minutes after 9 o'clock."

Manchester.—I have an account in my journal of an aurora seen here the same evening, but no particulars are given.

London.—The aurora was seen in and about London the same evening. (See pages 478 and 480, vol. xiv. Annals of Philosophy.)

Gosport.—In the same volume of Annals, page 395, there is an account of the same aurora as seen at Gosport Observatory, Hampshire, on that evening, by Dr. Burney. The following is an extract: "On the 17th instant, at 7 P.M. a light about 30° on either side of the magnetic north point appeared in the shape of a luminous arch whose apex was 18° above the horizon." He then describes several beams of the common aurora which successively appeared and traversed about for a time chiefly within the arch, and then vanished and were succeeded by others. After which, he adds: "Soon after this (9 o'clock) the luminous arch in the northern hemisphere entirely disappeared, and some haze collected near the horizon."

Gosport and Keswick are very nearly under the same magnetic meridian, and 265 miles distant. Newton-Stewart is N.W. by W. of Keswick, distant about sixty-five miles, but only thirty-five miles in a meridional direction. Now I imagine it will be allowed that an extraordinary luminous arch seen at Newton-Stewart to cross the meridian a few degrees south of the zenith, and to continue from 8 to near 9 o'clock, nearly in that position, must have been the arch seen at Keswick at the same time to cross the meridian in like manner from east to west, and to pass nearly through the zenith. It may well be supposed, then, that this arch crossing through the zenith at Keswick would have a very diminished altitude if seen at Gosport, 265 miles south. From the account I have extracted, it appears that a luminous arch was seen there at the same time it was seen at the other places, and crossing the meridian at right angles, only its altitude 18° from the north, instead of being in the zenith, as at Keswick, or a few degrees south of it, as seen in Scotland. And further, the arch vanished at all the places at the same time. It scarcely admits of doubt, then, that these arches were all one and the same. By calculation from the data at Gosport and Keswick, I find the height of the arch above Keswick to be 100 or 102 miles; from which the angle of elevation from Newton-Stewart must have been 71° from the south, or the zenith distance of the arch 19°.

A luminous arch was seen at Kendal on the 27th of December 1827, of which my friend Samuel Marshall was so good as to write me a circumstantial account. It was first seen at ten minutes past 6 in the evening, being an arch between the magnetic east and west, and passing through the zenith. It was broadest in the zenith, and it was more condensed in the eastern extremity than in the western. Another parallel arch appeared about 20° north of the former, of rather less intense light; and the northern horizon was luminous as usual on such occasions. After ten minutes or more, the arches advanced each of them to the south 20° with their centres. The appearance lasted about half an hour. A few streamers were seen in the east, which moved slowly northward. Mr. Marshall thinks the appearance would have been splendid if the moon had not shone at the time: a halo round the moon vanished when the bow approached it. I observed a halo round the moon at Manchester that evening.

Mr. Buchan, a gentleman accustomed to meteorological observations, had mentioned his having seen a similar arch at Manchester on that evening; but apprehending it might only have been a local phænomenon, I did not inquire particulars till I received the above account from Kendal. Mr. Buchan informs me he saw a luminous arch that evening, about nine o'clock; the arch was highest to the west of the meridian, and its altitude was very nearly the same as the north pole, just under which it passed; he estimates it at 53°, and thinks it could not be above 1° more or less. As this observation was not contemporary with that at Kendal, nothing certain can be deduced from them; but it may not be amiss to observe that an object in the zenith at Kendal, and elevated 53° from the north at Manchester, must be nearly one hundred miles high.

The results of this series of additional observations agreeing so nearly with that of the 29th of March 1826, I am induced to believe that these luminous arches of the aurora which occasionally appear, stretching from east to west, are all of the same height, and that height about one hundred miles. What length the upright beams,—or to speak more properly, those parallel to the dipping needle,—may be, which are the ordinary forms of the aurora, we have not observations to determine. Whether those beams arise above the arches, as from a base, or whether they descend below, as if appended to the arches, we cannot absolutely determine. It is remarkable that the arches and beams should rarely, if ever, be seen cognate or in juxta position, but always in parts of the heavens at a considerable distance from each other.

Manchester, March 18, 1828.

P.S.—Query, Are the parallel bands usually about 20 degrees asunder? If so, their distance from each other will be about thirty-six miles.

LXX. *On*

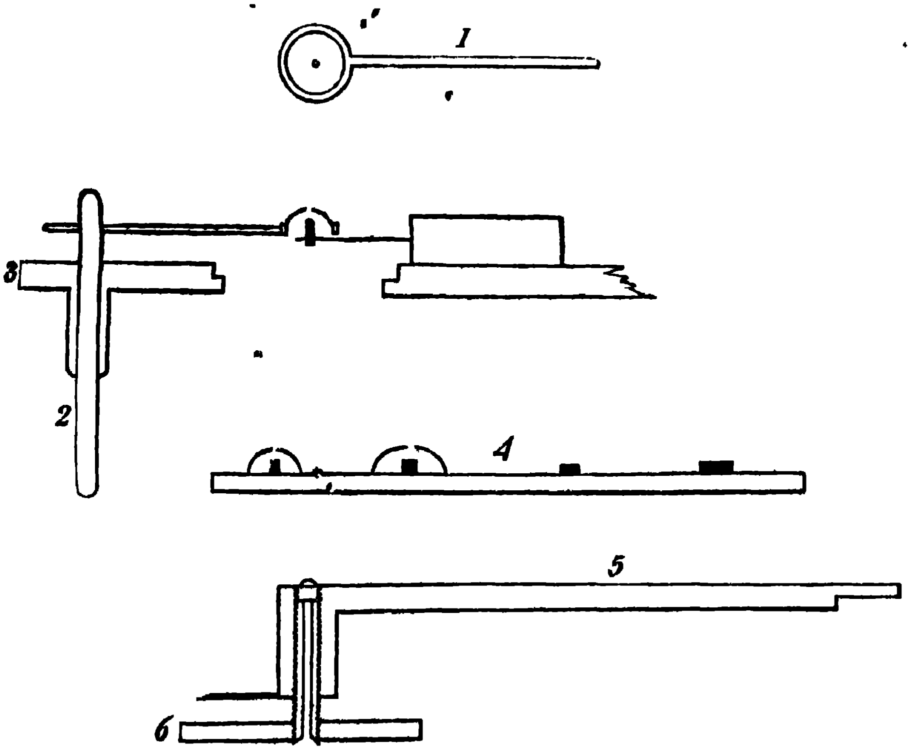
LXX. *On the Fitting-up of Microscopes for the Examination of Opaque Objects requiring high powers; and on the Construction of a Focimeter.* By *Mr.* G. Dakin.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

As microscopes are now fitted up by the opticians, a numerous and beautiful class of objects is entirely lost to observers; viz. those opaque objects which require a high power. My single microscope, which I had purposely fitted up with the highest powers both opaque and transparent, is almost useless for this purpose, as the highest opaque eye-piece is 1-6th of an inch focus. I have seen the scales of the diamond-beetle as an opaque object through a lens which I think Dr. Goring said was 1-60th of an inch focus. He has certainly carried it quite to the maximum, as there was rather a want of light and distinctness, which I attribute in a great measure to the best part of the speculum being lost by the introduction of the deep convex cup which holds the lens; nevertheless the beautiful lines on the scales were plainly to be seen, even with this high power. I have made the whole of my transparent eye-pieces answer as opaque ones, in which the lenses are placed on the *outsides* of the speculums; consequently the whole of the central and best part of the speculum is brought into action. Opticians seem to forget that the light which is thrown on the speculum is already condensed, and that such large ones are not necessary, unless it be to compensate for the clumsy manner in which they generally fit up the opaque slides. Objects of this sort ought to be fixed on small cylinders of jet or ebony, and these glued on slips of glass: by this means little or no light is lost. The speculums I made were about $\frac{1}{2}$, $\frac{3}{8}$, $\frac{1}{4}$, $\frac{1}{6}$, and $\frac{1}{8}$th of an inch diameter: they should be fitted into a brass ring (fig. 1.), to which should be soldered a piece of wire, fitting a hole in a larger piece of wire (fig. 2.), which should slide easily through the stage (fig. 3.); or they may be laid on the slips of glass over the objects (fig. 4.), provided the cylinders are adjusted to bring the objects into the focus of the speculums. When the highest powers are used, take a very small piece of the object and lay it on a small cylinder, which must have a fine hole in the side, for the purpose of fixing it on the point of a fine needle; place the object in the centre of the stage, and bring the speculum down so as to illuminate the object as much as possible. The eye-piece must then be brought down close to the hole in the speculum, the eye being now placed in its right position. The eye-piece may be raised to its true focal distance; this will prevent the lenses being injured by coming in contact with the speculum.

The highest power in my microscope is about 1-46th inch focus; but a lens of 1-30th inch focus may be used by a novice with ease and very great advantage, and the object will be as well illuminated as by one of the large speculums. As they are very easily made, I shall describe the method of making a 1-4th inch one. Melt a little fine silver by the blowpipe into a globule about 1-10th of an inch diameter; hammer it out till it is full 1-4th inch wide, and as thick as stout foolscap paper: take a piece of brass wire, barely 1-4th inch thick, and file one end of it hemispherically; then lay the silver on a piece of lead about 1-4th inch thick. Hollow the silver to the shape of the wire, and then drive it quite through the lead, turning the wire at the same time: make a small hole in the centre, and fix it on the end of a stick of sealing-wax; then file a piece of slate-pencil, barely 1-4th inch diameter, to fit the silver cup, and grind them together with fine emery, till it is of a uniform figure quite over the surface; then wash the emery off, and grind the scratches out with the pencil, washing the mud away with clear water. To polish it, cover the pencil with thin silk, rub a little tallow and jeweller's rouge on the silk, and work them together till the speculum is beautifully polished.



I strongly suspect that the focal distances of lenses are generally underrated by opticians. To prove mine, I made what may be called a Focimeter. It consists of a flat piece of brass, which fixes on the arm of the microscope (fig. 5.); at the end is a female screw that has exactly fifty threads to the inch, to which is fitted a male screw about half an inch long, with a hole drilled quite through it, and a large ivory head (fig. 6.), with twenty divisions fixed on its lower end; consequently one division on the head is equal to the 1-1000dth of an inch. After the end of the screw and the brass plate are ground together on a flat hone, there must be glued over the hole in the male screw, a small piece of the outer membrane of the eye of the *Libellula grandis*, and a piece of very thin foil with a small hole in its centre laid over the female screw. When used, fix it to the arm of the microscope, and throw the light up the hole by the mirror; lay the highest power on the foil, turn the screw up till it just touches the foil, and set down the number of turns and divisions that it takes to bring the object down to the focus of the lens.

Proceed in the same manner with the rest of the powers; they may then be easily reduced to the nearest fraction having one for a numerator: by this means I found that my highest power, which I considered 1-60th of an inch focus, was only 1-46th inch focus, and the rest in proportion. I have made a double convex lens, which is the smallest I have ever seen, and is only 1-100dth inch focus, as it took but half a turn of the screw to bring the object down to the focus. The thickness of the foil may be neglected even in the highest powers; as it does not amount to the 1-1000dth of an inch.

Dr. Goring uses the scales of the *Lepidoptera* as test-objects: but the most beautiful and delicate test-objects that I have ever seen are the scales of the *Lepisma saccharina;* they are so very thin, and the lines upon them are so very fine, that they will bear almost any power. The beautiful green convex scales of the small English diamond-beetle, which is very common in the summer months, is a very good opaque test-object.

The focal distance of large lenses for telescopes, &c. is reckoned from the centre of the body of the lens (under half an inch focus); but in my opinion the focus of microscopic lenses ought to be computed from the surface next the radiant object, otherwise another operation is necessary; viz. taking the thickness of the lens and adding half of it to the focal distance: but as small lenses are so very apt to be lost or cracked, it is much safer and better to reckon from the surface.

I beg to remain yours, &c.

Ninesham, Oct. 23, 1828. G. Dakin.

LXXI. *On*

LXXI. *On the Method in the Trigonometrical Survey for finding the Difference of Longitude of two Stations very little different in Latitude.* *By* J. Ivory, *Esq. M.A. F.R.S. &c.*[*]

IN this Journal for October last, I endeavoured to prove that the method in the Trigonometrical Survey for finding the difference of longitude of two stations, not much different in latitude, was insufficient, and led to erroneous results. The principle of the method is this, That the latitudes being the same, the difference of longitude is independent of the excentricity, or it is the same on the surface of a sphere and a spheroid of small oblateness; which, in reality, is consistent neither with experience, nor with other methods of investigation of undoubted accuracy. We are told indeed, that the incorrectness of the method, and its want of success in practice, is now allowed on all hands; but the date at which the delusion was dissipated is not mentioned. As my intention was merely to overturn an insufficient method of calculation, not to establish a new rule, I neglected such small quantities as could not be distinguished from the unavoidable errors of observation. Thus the small quantity in the value of β, which I neglected (p. 243), would be produced by a small variation in the length of the chord γ, amounting to about 18 feet in the distance between Beachy Head and Dunnose, more than 65 miles. Assuredly a method of calculation which requires such nice accuracy in the data of observation is not a very solid foundation on which to place any conclusion respecting the figure of the earth. There is also an omission in the equations (x) (p. 243), arising from supposing $R = a$ (p. 242), which, however, will affect the azimuths only a small fraction of a second in the ordinary circumstances of the problem; that is, when one azimuth is greater, and the other less, than 90°. In the extreme case, of which there is no instance that I know of, when the azimuths are both less than 90°, and nearly equal, the last-mentioned omission will affect the accuracy of my formula, because the quantity neglected does not vanish when the two latitudes become equal. There is, however, no doubt that, in the ordinary circumstances of the problem, my method, which takes the excentricity into account, comes nearer the truth than the method in the Survey, which entirely neglects the figure of the earth; and this seemed sufficient to answer my purpose. But it is not easy to put down an authorized error; although I shall now attempt to accomplish this task by new investigations, to which it will be impossible to object.

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[*] Communicated by the Author.

Using the same symbols as in this Journal for October, the two following equations, which are rigorously exact, contain the full solution of this problem; viz.

$$\left.\begin{aligned}
&\Delta=\sqrt{1-e^2\sin^2\lambda},\ \Delta'=\sqrt{1-e^2\sin^2\lambda'},\ Q=\frac{\sin\lambda}{\Delta}-\frac{\sin\lambda'}{\Delta'},\\
&\frac{\sin\omega}{\tan m}+\cos\omega\ \sin\lambda-\cos\lambda\ \tan\lambda'=\frac{\cos\lambda}{\cos\lambda'}\cdot e^2\,\Delta'\,Q\\
&\frac{\sin\omega}{\tan m'}+\cos\omega\sin\lambda'-\cos\lambda'\tan\lambda=-\frac{\cos\lambda'}{\cos\lambda}\cdot e^2\,\Delta\,Q
\end{aligned}\right\}\text{(A)}$$

These equations express the condition that two vertical planes, one at each station, intersect in the chord joining the stations. As the investigation is merely elementary, it may be omitted. The two equations, although very simple, are alone sufficient for the solution of the problem, since the excentricity and the difference of longitude are the only unknown quantities they contain. In order to simplify, I shall put $x=\sin\lambda-\sin\lambda'$; and I shall write $\frac{\cos\lambda}{\cos\lambda'}\cdot e^2 x$ and $\frac{\cos\lambda'}{\cos\lambda}\cdot e^2 x$, for the quantities on the right-hand sides. The equations coincide with the surface of a sphere when $e^2=0$, and when $x=0$; and as x is always very small in the practical application of the problem, it is with difficulty that the quantities on the right-hand sides enable us to distinguish between the sphere and a spheroid of small oblateness. It is here indeed that the difficulty of the problem lies; and it will easily be conceived that, without nice discrimination, one case is apt to be confounded with the other, as it actually is in the method of the Trigonometrical Survey. In order to solve the equations so as to give full effect to the excentricity of the spheroid, it would be requisite to free them from the almost evanescent factor x; but this is what I shall not at present attempt to accomplish. As I write in haste, I shall not inquire how the value of the excentricity is to be deduced, but shall confine my attention to the difference of longitude, supposing that the observations have been made upon a spheroid of a known figure.

The two equations may be brought to a form fit for calculation by the following transformation: viz.

$$\left.\begin{aligned}
&\tan u=-\frac{1}{\sin\lambda\tan m},\quad \tan u'=\frac{1}{\sin\lambda'\tan m'},\\
&\frac{\cos(u+\omega)}{\cos u}=\frac{\tan\lambda'}{\tan\lambda}\left(1+\frac{e^2x}{\sin\lambda'}\right),\\
&\frac{\cos(u'-\omega)}{\cos u'}=\frac{\tan\lambda}{\tan\lambda'}\left(1-\frac{e^2x}{\sin\lambda}\right).
\end{aligned}\right\}\quad\text{(B)}$$

With the data at Beachy Head and Dunnose, and making $\frac{e^2}{2} = \cdot 00324$, I have deduced these values of ω, viz.

$$\begin{aligned} \omega &= 1^\circ\ 26'\ 58''\cdot 3 \\ \omega &= 1\ \ \ 27\ \ \ 0\cdot 8 \end{aligned}$$

These results approach near the true quantity, but they do not agree, which shows that there is some inconsistency in the observed quantities. If we add $0''\cdot 6$ to the difference of latitude, the formulas will give results very nearly equal to one another and to the true quantity. Thus, making

$$\begin{aligned} \lambda &= 50^\circ\ \ 44'\ \ 21''\cdot 3 \\ \lambda' &= 50\ \ \ 37\ \ \ 4\cdot 7, \end{aligned}$$

I have found

$$\begin{aligned} \omega &= 1\ \ \ 27\ \ \ 3\cdot 8 \\ \omega &= 1\ \ \ 27\ \ \ 7\cdot 2, \end{aligned}$$

and the mean of these values almost coincides with the exact quantity.

What has now been said is decisive of this question. The equations to which the problem has been reduced, which are rigorously exact, prove that the excentricity has an influence on the difference of longitude, however minute that difference may be, and however difficult to bring it to an exact valuation. The investigation we have employed is drawn from the principles of elementary geometry. We have advanced no vague reasoning about the well-known properties of spheroidical triangles and geodetical lines, in a case where, in fact, there is neither any such triangle nor any such line.

If we combine the two equations (A) so as to exterminate the excentricity, we shall obtain the following equation; viz.

$$0 = \sin\omega\left(\frac{\cos\lambda'}{\cos\lambda\tan m} + \frac{\cos\lambda}{\cos\lambda'\tan m'}\right)$$
$$+ \cos\omega\,(\cos\lambda'\tan\lambda + \cos\lambda\tan\lambda') - (\sin\lambda + \sin\lambda').$$

Here, then, we have an equation which is independent of the excentricity, and which therefore expresses a property common to the sphere and to any spheroid. But if any one should imagine that, now certainly by this exact equation, the difference of longitude may be found independently of the excentricity, he is advised to consider well the principles on which he proceeds before he begins to calculate, lest he should lose his labour.

Dr. Tiarks has treated of this subject in the last Number of this Journal. He is fortunate enough to take the right side of the question, standing forth as the champion of the method in the Survey. But he has entirely mistaken the nature of the problem and the difficulties that must be overcome in solving it. There is no spheroidical triangle; there is no geodetical line. The data consist of the latitudes, nearly equal, and the azimuths, of two distant stations, yet sufficiently near for one to be seen from the other. Now in the case of such distant stations, the observed azimuths are not the same with the azimuths in an imaginary geodetical line passing through the stations. In this, and in other respects, there is great want of precision and geometrical rigour in Dr. Tiarks' argumentation. He has, in fact, brought forward nothing new either in proof, or in elucidation of the problem of which he treats; he has merely added the name of Dr. Tiarks to the other authorities that have upheld the correctness of the method of calculation in the Survey, which it is, nevertheless, just to characterize as the greatest delusion that has ever prevailed in practical mathematics. I can add nothing more on this subject at present; but on another occasion I will give another solution of the same problem, more nearly allied in point of form to the speculations of Dr. Tiarks, which will enable us better to appreciate their merits. When the principles of the problem are fully unfolded, we may then discuss the question, Whether the calculations in the Survey are correct or not?

Nov. 1828. J. IVORY.

LXXII. *Notice of the Geological Features of a Route from Madras to Bellary, in April and May* 1822. *By Captain* W. CULLEN, *of the East India Company's Artillery service.*

[Concluded from p. 363.]

THERE are two instances of the occurrence of a rather remarkable variety of quartzose rock in the course of the preceding route, which have not been yet noticed, because it differed very sensibly from any of the others, both in its colour and composition, and appeared likewise to be altogether independent of them. The first occurred on the west side of the pass between Baukrapett and Cuddapah, occupying about one mile of the route, and appearing to constitute the hills on both sides. The lower end of the line of hills, crossed three or four miles before reaching Nundialpett, were composed of rock precisely similar, and were, I imagine, merely a continuation of those forming the pass. I do not recollect any appearances of stratification in either case, although the course of the range was conformable to the general direction. Perhaps this rock rested on clay-slate, and may be considered as merely one of those transitions, formerly alluded to, from sandstone

stone to quartz*. All the hills on the right of the road from Chinoor towards Nundialpett exhibited parallel lines on their slope, similar to those of clay-slate formerly adverted to.

The second variety occurred four or five miles north of Poornamila, distinctly interstratified with clay-slate. Immediately on leaving that village, a low but very extensive and sharp ridge is observed to issue from the eastern boundary of the Poornamila plains, and crossing the road obliquely, to enter the clay-slate range forming the western boundary of the valley, which extends by Alinuggur towards Cummum.

This low range is very conspicuous, being at least six or eight miles in length, and running directly across the north-east portion of the plain. The high road lies immediately over it, near its western extremity. The rocks, in this instance, were distinctly stratified, and formed the crest on the centre of the ridge, bounded on both sides by clay-slate of a brownish gray colour and silky lustre.

I do not observe that I have any specimens of the quartz rock forming the west side of the pass to Cuddapah, but externally it resembles the specimens from Nundialpett. This latter was internally of a dark bluish gray colour, with rather a granular appearance, and having small earthy specks of a rusty brown, sparingly dispersed through it. The second variety in the ridge north of Poornamila appeared to be composed of similar ingredients, but the earthy specks in much greater quantity, and giving a decided colour to the rock. It contains also a few distinct crystals of a black or deep red colour, which appear to be garnets, and which may explain the nature of the rusty specks.

No limestone of any consequence was observed before reaching Wuntimettah, although calcareous depositions are to be met with; such as a white tufa in wells between Codoor and Pollumpettah, occasional traces of marl, and a singular reddish coloured limestone, or marl conglomerate, near Rajampett, between Pollumpettah and Nundaloor.

From the similarity of colour in the clay-slate, flinty-slate, and calcareous schistus, it is not improbable, however, that I may have occasionally been mistaken in my judgement as to their nature in the early part of my march, and even when better acquainted with them; for their resemblance is sometimes so strong as to render recourse to a test absolutely necessary; an operation which would have occupied too much time to be constantly put in practice while actually on route.

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* I have since visited the inner range of hills near Cuddapah, running north-west from the pass. They consist of sandstone; towards their base rather coarse grained and dark coloured, but on the summit extremely compact and of a light colour, and giving the hills much of the appearance of quartz. This upper portion is very like the variety alluded to near Nundialpett, but wanted the rusty specks.

An old pagoda at Wuntimettah is built of a dark blue limestone, of a fine and close texture, taking a good polish, and altogether a handsome, though rather a brittle building material. Some of the blocks were varied with shades of gray or white. Stratified masses of this limestone cross the road in great abundance for the first five miles, as far as the village of Baukrapett, from whence commences a considerable ascent, leading through the clay-slate range which forms the eastern boundary to the low and extensive flat country of the district of Cuddapah.

The deep covering of soil on the plains on the west side of this range concealed all traces of rocks; but the same dark blue limestone appeared to be the universal building material at Cuddapah, and must doubtless have been procured close at hand, although I did not myself meet with any quarries in the immediate vicinity. Varieties of this limestone were, however, common during all the rest of my progress to the north; although latterly, that is to say, after passing Poornamila, it altered considerably in its colour as well as in its texture, becoming more granular and crystalline, as well as of a light gray colour.

Calc-tufa, marls, and conglomerates were in great abundance from the first appearance of the regularly stratified limestone, in distinct depositions of considerable extent, as well as filling the seams of the clay-slate, particularly in the green variety near and subsequent to Poornamila. The red coloured, porphyritic-like limestone, which I have mentioned as occurring first near Rajampett, was met with frequently afterwards in the defile between Nundaloor and Wuntimettah, on entering the plains of the Saghilair, between Poornămila and Alinuggur, &c., and always in horizontal masses of very considerable extent, and from one and a half to two or three feet thick. Schist was generally found immediately below, and its seams filled with depositions of the same nature.

This conglomerate is composed principally of spherical nodules of a kind of calc spar of a dull reddish colour, from one-tenth to five- or six-tenths of an inch diameter, in an earthy basis of a light brown; small angular particles of quartz are also sparingly dispersed through it. This conglomerate is extremely hard, but not always compact, having frequently numerous vesicular cavities.

At Cuddapah I first noticed a very singular variety of calc-tufa in nodules, or blocks, of from six to eighteen inches diameter, extremely hard, and of a dirty white or gray colour, as if from an intermixture of clay. They were very vesicular internally, and externally covered with hard, knotty, irregular-shaped protuberances, which were of a size proportionate with that of their respective nuclei. They were used, loosely piled together, to form inclosures.

This variety of calc-tufa was met with frequently afterwards, and in great abundance; but I never had a good opportunity of examining it *in situ.*

In the banks of a nulla between Cuddapah and Chinnoor, as also in the bank of the Pennar at the latter village, I had a transient view of it in the mass; and it there appeared to form a horizontal stratum of two or three feet in thickness, a few feet below the surface.

Flinty slate and hornstone still remain for notice. I have mentioned the appearance of the latter at Curcumbaddy, in contact with the clay-slate; and the only recurrence of it that I recollect, possessing decidedly the character of that rock, was at the village of Yenapilla, three or four miles south-east of Nundaloor, where it appeared in large globular blocks, very much intersected by veins of quartz, over a space of four or five square miles. The flinty slate was much more abundant, being found interstratified with the clay-slate almost every march from Curcumbaddy to Wuntimettah.

The colour and texture of the hornstone were generally very uniform in each situation; but the flinty slate was frequently very much veined with different shades of gray or blue. The strata were sometimes, however, perfectly uniform in their colour, being occasionally of a dark blue, at other times of a light gray, and externally strongly resembling the dark blue limestone of the district of Cuddapah.

These were the principal rocks that occurred in the clay-slate of the second division.

Besides varieties of all the rocks hitherto enumerated, several others will be found amongst the specimens forwarded, which the limits proposed for this paper will not admit of describing at present.

The third division, or compact blue limestone, will easily be disposed of, as it occupies the whole of the flat country, extending from the Nulla Mulla range to Banaganapilly, to the exclusion of every other rock. Its texture within this tract was tolerably uniform, but it exhibited a considerable variety of colour; near the Nulla Mulla hills it was generally of a dull lead gray, and this variety, on fracture, presented a foliated appearance; about Jellila, and particularly in the bed of the nulla at Tungtoor, where it was quarried for building, the limestone was of a dull brick red, and frequently also variegated with stripes and shades of a light gray or white.

In approaching Banaganapilly, and all about that town, the limestone was in great abundance, and its colour blue of different shades, but chiefly dark. The deposition of soil on these plains afforded only a few opportunities of examining the limestone; but, where visible, the strata seemed to be *nearly horizontal.* These varieties, especially the last, were very compact and fine grained, and with rather a conchoidal fracture. The fine village of Banaganapilly, and the celebrated diamond deposit, is situated near the base of a low range of tabular land, running about north and south, and forming the western boundary of the great limestone field just described. The range is here abrupt and precipitous, and very irregular and broken in its outline, running out into headlands, as well as having deep narrow valleys penetrating far within it to the westward.

Banaganapilly lies under the point of one of these promontories, which projects five or six miles from the main range. These hilly tracts form the fourth division, or second of clay-slate, but, perhaps, do not terminate that of the limestone; for, although the depth of alluvium in this narrow valley is so great as to conceal all rocky strata, yet, from the numerous nodules of limestone on the surface, and the amazing depth and extent of the depositions of calcareous tufa, and the reappearance of strata of limestone some miles east of Jeldroogum, which then continue, with little interruption, even on to the table land three or four miles west of that village, it is not improbable that a connection subsists throughout. With exception of the cap, which is of sandstone, and which has already been adverted to, the whole mass of land forming the right hand boundary of the valley commencing near Banaganapilly, and extending to Jeldroogum, appears to consist of clay-slate nearly to the summit. There appeared to be table land also to the left, or south, and which, I imagine, also to be covered with sandstone (the Banaganapilly promontory I ascertained to be so, by ascending three or four miles to the west); but the immediate boundary of the valley on this side, although, like the former, composed of clay-slate, instead of a sandstone cap, was crested in its whole length with a sharp black ridge of trap rock, formed of loose blocks piled upon each other.

The apparent base of the trap observed a pretty uniform level; nor was the ridge of much depth, the peaks, merely by rising somewhat higher occasionally, giving the bed an appearance of greater dimension. Its extreme narrowness, deep black colour, and the total absence of all traces of vegetation, formed a singular contrast to the rest of the hills, which were covered with long dry grass, and scattered jungle bushes. The connection of this singular bed of trap with the rocks at its two extremities, would probably offer a most instructive subject of investigation.

At the end, near Banaganapilly, it first offers itself to notice in a small, steep, conical hill, composed solely of green-stone. I had previously, however, immediately almost on leaving Banaganapilly, observed several partial appearances of lines of black rock, jutting out a few feet above the soil, in the plain on the north side of and near the base of the Banaganapilly promontory. These, I have now little doubt, were a continuation of the green-stone bed. But it is at the western extremity at the head of the valley, close to Jeldroogum, where the hills close in on all sides, and where the route is crossed by the bed of trap, and all the rocks with which it is associated, that the attention is most forcibly excited. About one-half or one-third of a mile west of Jeldroogum the route lies over a ghaut, or ridge, of 300 or 350 feet perpendicular height, and, descending nearly as much on the opposite side, continues up a pretty level narrow valley for a mile or two, and then finally leads up another ghaut, of about the same height, on to a continuous table land, extending to Piaplee.

Blocks of limestone, of a light blue or gray colour, are common in the interval between Jeldroogum and the ghaut; but the low hills, forming the foot of the ghaut, were covered with large dark masses of rock, which I had imagined to be trap, but which, on examination, also proved to be limestone of a very dark blue. Limestones of other colours were also found intermixed, such as green, gray, &c.; the former on fracture, at a little distance, resembling a good deal a fine-grained green-stone; but the whole of these were covered with a dark crust of clay-slate, so that, unless close to them, it was not easy to distinguish the line of separation between the limestone and trap, which was the next in the order of succession, and at only a short distance from the foot.

The trap was again succeeded by limestone, and the latter by clay-slate, nearly to the summit, which was capped with rock of a beautiful flesh colour, with specks and shades of a delicate green, as if connected with its vicinity to the trap, and of so close and fine a texture as to appear homogeneous even through a lens. It exhibited a conchoidal fracture. The descent of the ghaut on the opposite side consisted of clay-slate nearly to the foot, when the limestone reappeared, and these two rocks then continued to alternate with each other to the foot of the second ghaut, which, like all the former, was composed of clay-slate, capped with quartzose sandstone. Limestone of a yellowish white colour was found in considerable quantity near the top of the ghaut. This high land appears to extend far to the north and south, and uninterrupted by hills; but on the west side, at the distance of three or four miles, was bounded by a chain of hills, running also nearly north and south, and marking the termination of the second tract of clay-slate. This line of hills appeared to be composed of clay-slate and the quartz sandstone, but neither of them exhibiting, at a distance, those distinct stratified appearances hitherto so common. The route lies over a low part of the chain, and almost immediately afterwards all traces of clay-slate disappear. The last rock, connected apparently with that formation, was a sandstone conglomerate, precisely similar to those singular nodules picked up between Naggery and Pootoor, and which here constituted two or three entire hills. Close to the road, a small open space of level ground, not more than three hundred yards wide, separated them from another small group of hills composed wholly of granite.

From this point commences the fifth, or granite division, the rocks prevailing, I believe, over the whole remaining interval to Bellary. I can, however, only speak with perfect certainty of the first fifteen or twenty miles, and of the last eight, having passed over the remaining part at night. The trap dykes, holding their usual course, were very common during the first-mentioned portion of granite.

Regarding the œconomical purposes to which the several rocks met with in the course of the foregoing route might be applied, I believe I shall best consult the pleasure of the Society in simply referring to the specimens which have been forwarded. These, although individually extremely small, may enable persons better acquainted with the subject than myself, to form a judgement of the probable degree of their utility. The principal rocks, granite, trap, sandstone, schistus, and limestone, were, it will have been observed, in great abundance, and in considerable variety, even on the high road; but a little inquiry would probably discover numerous other varieties, more useful, as well as more ornamental. The limestone particularly, of which there would appear to be a great variety, in colour as well as texture, from its facilities of working, would seem to merit some attention. I have seen some very handsome tablets of black or dark blue limestone from Kurnool. It has already been noticed, that the whole of the plain between the meridians of Kurnool and Cummum, and from the Kistnah to the Pennar, is composed of that rock. Some of the limestones, or marbles, may prove to possess that peculiar combination of clay, silica, and carbonate of lime, which is found to afford those valuable cements having the property of setting under water. Major De Haviland, of the Engineers, had, I believe, already directed his attention to the discovery of such varieties.

From my imperfect acquaintance with the subject, I am aware, that in the above summary of the situations of the rocks over so great a space, I must necessarily have omitted noticing many circumstances which it would have been of consequence to record; but daily marches of from fifteen to twenty miles in the hottest season of the year, may of themselves be deemed an apology for those, as well as other inaccuracies that may, perhaps, hereafter be discovered. I have, however, endeavoured to be faithful in my narrative of facts; and as such, in the present limited state of published information on these subjects in this country, they will, I trust, be received with indulgence.

W. CULLEN,
Captain of Artillery.

Bellary, May 1823.

The altitudes, in the section which accompanies this paper, are deduced from barometrical observations, regularly made throughout the march. Observations of the maximum and minimum heights of the column of mercury were taken daily at each stage, and the mean of these was generally used in the calculations. Corresponding observations were obligingly furnished to me by Mr. Goldingham from the observatory; from a comparison with which, and with another series of observations made by Major Beckett at Bellary, the altitudes of the several stations have been determined.

In former instances, most of the approximate altitudes which I have had opportunities of ascertaining in different parts of the peninsula, have been deduced solely from my own observations, a mode originating in some degree in necessity, and subsequently adhered to, partly from the same cause, but partly also from choice, from a desire to ascertain how far such approximations might be depended upon.

Few or no barometrical observations were at that period regularly made at any of the inland stations under this Presidency; and the stations where such records were kept were so remote from each other, as well as from my routes, as to occasion frequently very great differences in the altitudes derived from them.

I proposed accompanying this paper with a copy of my own barometrical observations, as well as of the corresponding ones at Madras and Bellary, with the several altitudes deduced from them; but I have not been able to accomplish my intention.

Colonel Lambton's base line near Gooty is nearly 1200 feet above the sea, and he assumed this as the mean altitude of the interval between that station and Bellary; an inference to which he was perhaps led by the plain, open appearance of the intermediate country. The barometer, however, stood on the 2d June at

Guddicul,	28600 m.	99°	Goontacul,	28528 m.	91°
	29920	92½		29910	87

Feet.
making Guddicul 1420 } nearly above the sea.
Goontacul 1479 }

Madras, July 1823. W. Cullen.

Although the section referred to above, is not given in the Madras Transactions, we have thought it right not to suppress the Postscript, in which some of the altitudes in question are stated.—Edit.

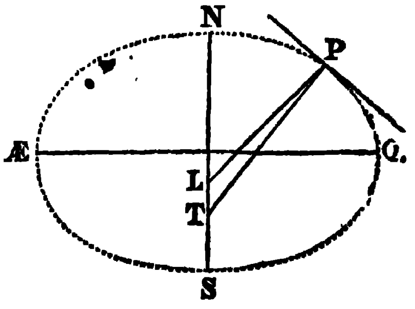
LXXIII. *On the Figure of the Earth.* By S. Sharpe, *Esq. F.G.S.**

Most of the investigations into the figure of the earth have been on the supposition that its section through the poles were an ellipse, which, however, several mathematicians have shown not to be exactly true.

Thomas Simpson has shown, that in an elliptical globe, with a given axis major, and a given period of rotation on its axis minor, its ellipticity is given also.

There is water enough on our earth to show that its shape is that which it would take if liquid, therefore the plumb-line is every where perpendicular to the tangent.

Let PT be perpendicular to the tangent at P on the ellipse ÆNQS: then if the earth were at rest, the plumb-line would point nearer to the centre, as PL; and the angle LPT is the measure of the centrifugal force caused by the rotation on its polar axis NS. Hence the connection between the ellipticity and velocity of rotation.



I have gone over Thomas Simpson's work, and find:

1st, If the pendulum at the equator be 39·014 inches, a body falls 16·044 feet per second; not $16\frac{1}{12}$ feet, as according to Thomas Simpson,—

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* Communicated by the Author.

2dly, If the earth's equatorial radius be 20,927,442 feet; not 21,000,000,—

3rdly, If a sidereal day contain 86,164 mean solar seconds; not 86,160,—

The gravity at the equator is to the centrifugal force as 1 to $\frac{1}{288\cdot33}$, and the ellipticity $\frac{1}{233}$; not $\frac{1}{231}$, as according to T. Simpson: the difference is not much.

And as we know from observations with the pendulum that the ellipticity is less, the polar section of the earth is not an *exact* ellipse, and the usual reduction of the observed latitude ($\tan \lambda = (1-e^2) \tan l$) not *strictly* correct.

Canonbury. SAMUEL SHARPE.

LXXIV. *An Abstract of the Characters of* Ochsenheimer's *Genera of the Lepidoptera of Europe; with a List of the Species of each Genus, and Reference to one or more of their respective Icones.* *By* J. G. CHILDREN, *F.R.S. L. & E. F.L.S. &c.*

[Continued from page 354.]

Genus 19. ZYGÆNA, *Fab.*, *Latr.*, *Hübn.*

ANTHROCERA, Scop. (Steph.)

Legs, tibiæ with short, close-set scales; the posterior with very small calcaria, or spines. (Latr.)

Wings, deflexed, (Latr.) broad; anterior generally steel-blue, with red spots; posterior generally red.

Antennæ slender at the base, thickening into an abrupt, bent fusiform club, with the apex simple; in the males robust. (Steph.)

Palpi reaching beyond the clypeus, cylindric-conic, acuminated, densely clothed with hair. (Steph.)

Antlia fine, moderately long.

Abdomen nearly cylindrical, obtuse; (Latr.) thickly clothed (as well as the *head* and *thorax*) with short silken hairs, with a few scales intermixed. (Steph.)

Flight, diurnal.

Larva, villose, fusiform, generally spotted with black on a pale ground; *head* small. (Steph.) No horn on the last segment of the body. (Latr.)

Pupa elongate. (Steph.)

Metamorphosis in the air, in a pretty solid silken cocoon, attached to the branch or leaf of a plant. (Latr.)

Obs. These insects are sluggish, and fly but little; they commonly remain on the plants on which the females deposit their eggs. . Neither sex lives longer than is necessary for copulation and laying the eggs, resigning their lives as soon as those objects of their existence are accomplished.

	Species.	Icon.
1.	Z. *Erythrus*, Hübn.	Hübn. Sphing. Tab. 18. f. 87. (fœm.)
2.	— *Minos*, Hübn.	Ernst, III. Pl. XCV. f. 133. a—d.
3.	— *Pluto*, Ochs.	Hübn. Sphing. Tab. 18. f. 88. (mas.)
4.	— *Brizæ*, Hübn.	Hübn. Sphing. Tab. 18. f. 85. (mas.) Tab. 2. f. 6. (fœm.)
5.	— *Scabiosæ*, Fab.	Ernst, III. Pl. XCVI. f. 134. a—d.
6.	— *Achilleæ*, Esp.	Ernst, III. Pl. XCIX. f. 141. a—d.
7.	— *Punctum*, Ochs.	Hübn. Sphing. Tab. 26. f. 119. (mas.)
8.	— *Sarpedon*, Hübn.	Hübn. Sphing. Tab. 2. f. 9. (mas.)
9.	— *Exulans*, Hübn.	Hübn. Sphing. Tab. 2. f. 12. (mas.) Fab. 20. f. 101. (fœm.)
10.	— *Cynaræ*, Hübn.	Hübn. Sphing. Tab. 17. f. 80. (fœm.)
11.	— *Meliloti*, Esp.	Hübn. Sphing. Tab. 17. f. 82. (mas.)
12.	— *Trifolii*, Esp.	Ernst, III. Pl. XCVII. f. 136. a—e.
13.	— *Loniceræ*, Hübn.	Hübn. Sphing. Tab. 2. f. 7. (fœm.)
14.	— *Filipendulæ*, Linn.	Ernst, III. Pl. XCVII. f. 137. a—f.
15.	— *Transalpina*, Hübn.	Hübn. Sphing. Tab. 3. f. 15. (mas.) f. 19. (fœm.)
16.	— *Medicaginis*, Hübn.	Hübn. Sphing. Tab. 4. f. 20. (mas.) ?
17.	— *Hippocrepidis*, Hübn.	Hübn. Sphing. Tab. 5. f. 32. (fœm.) Tab. 17. f. 83. (mas.)
18.	— *Angelicæ*, Ochs.	Hübn. Sphing. Tab. 26. f. 120. (mas.) f. 121. (fœm.)
19.	— *Dorycnii*, Hoffmansegg *.	
20.	— *Peucedani*, Hübn.	Ernst, III. Pl. XCVIII. f. 139. a—e.
21.	— *Ephialtes*, Linn.	Ernst, III. Pl. C. f. 144. a. b.
22.	— *Stoechadis*, Borkh.	Hübn. Sphing. Tab. 4. f. 24. (mas.)
23.	— *Lavandulæ*, Fab.	Ernst, III. Pl. CI. f. 145. a. b.
24.	— *Rhadamanthus*, Hübn.	Hübn. Sphing. Tab. 4. f. 23. (mas.)

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* Z. alis anticis viridibus, punctis sex coccineis: posticis coccineis, margine latissimo cyaneo; abdomine cingulo suprà coccineo.—*Ochs.* II. 69.

Species.	Icon.
25. Z. *Onobrychis*, Fab..	Ernst, III. Pl. XCIX. f. 140. a—h.
26. — *Occitanica*, de Villers................	Hübn. Sphing. Tab. 22. f. 106. (mas.) 107. (fœm.)
27. — *Fausta*, Linn......	Ernst, III. Pl. C. f. 142. c. d.
28. — *Faustina*, Ochs.[*]	
29. — *Hilaris*, Ochs. ...	Hübn. Sphing. Tab. 26. f. 123. (fœm.)
30. — *Læta*, Hübn......	Ernst, III. Pl. C. f. 142. a. b.
31. — *Sedi*, Fab.	Hübn. Sphing. Tab. 28. f. 132. (fœm.)

Genus 20. SYNTOMIS, *Hoffmansegg*[†], *Latr.*

AMATA, Fab.	GLAUCOPES, Hübn.
SPHINX, Linn.	ZYGÆNA, Ross.

Wings, anterior large, posterior very small; deflexed.
Antennæ filiform; (subfusiform. Latr.)
Palpi, inferior nearly cylindrical, obtuse, not reaching beyond the clypeus. (Latr.)
Abdomen cylindrical. (Latr.)

Species.	Icon.
1. S. *Phegea*, Linn......	Ernst, III. Pl. CII. f. 147. c. d.
2. — *Cerbera*, Linn.[‡]..	Cram. Pap. exot. Tab. 83. f. F.

Genus 21. THYRIS, *Hoffmansegg*, *Latr.*, *Hübn.*

SPHINX, Fab.

Legs hairy, spinous.
Wings divaricate, nearly horizontal, eroso-dentate, (Latr.) with white semi-transparent maculæ.
Antennæ, scarcely fusiform, nearly subsetaceous, simple; apex not tufted, (Latr.) alike in both sexes.
Palpi cylindrico-conical, the last joint nearly bare, acuminate; reaching much beyond the clypeus. (Latr.)
Abdomen conical, anus not barbate. (Latr.)
Flight, diurnal.

Species.	Icon.
1. T. *Fenestrina*, Fab...	Ernst, III. Pl. CXXII. f. 167. a. b.

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[*] Z. alis anticis nigro-viridibus; maculis sex rubris confluentibus luteo marginatis; posticis rubris, limbo æquali nigro; collari rubro; pedibus nigro-viridibus cinguloque abdominis rubro subtus non coeunte.—*Ochs.* II. 99.

[†] According to Latreille, this genus was established by Illiger:—*Nouv. Dict. d'Hist. Nat.* xxxii. 320.

[‡] An Europæa?—*Ochs.*

Genus 22. STYGIA, *Drap.*, *Latr* *.

BOMBYX, Hübn.

Legs, posterior tibiæ with very distinct spines and calcaria.

Antennæ short, insensibly diminishing in size from the base to the apex, curved, covered beneath with a double row of small laminæ; apex simple.

Palpi thick, cylindrical, squamate, reaching beyond the clypeus.

Tongue obsolete, or none.

Anus barbate.

Species	Icon.
1. S. *Australis*, Drap. (Bombyx terebellum, Hübn.).	Hübn. Bombyces, Tab. 57. f. 244. (mas.)

Genus 23. SESIA, *Fab.*

ÆGERIA, Fab. TROCHILIUM, Scop. SESIA, Hübn.

Legs, tibiæ clothed with long scales; the posterior with four elongated spines or calcaria, disposed in pairs. (Latr.)

Wings long and small, displayed nearly horizontally; the superior incumbent on the inferior; the latter hyaline; the former very narrow, and usually fenestrate. (Latr.)

Antennæ fusiform, incrassate towards the middle, simple; apex with a small scaly tuft.

Palpi reaching beyond the clypeus, pointed. (Latr.)

Antlia long and thin, except in the two first species.

Abdomen sub-cylindrical; anus barbate. (Latr.)

Flight, diurnal.

Larva, nearly bare, cylindrical, without any horn at the extremity of the body. (Latr.)

Pupa elongate; back spinous.

Obs. The caterpillars of the *Sesiæ* gnaw out the interior of vegetables, and make themselves a more or less solid cocoon therein, composed of the detached and pulverized portions, which they agglutinate, and connect together by silk. They usually pass the winter in that state, and become perfect insects in the following spring. (Latr.)

Ochsenheimer very justly observes that the term *Sesia*, being derived from the Greek word σης, (*tinea*,) would be more correctly spelt *Setia*, since the genitive of the original is σητος.

Authors are not well agreed as to the arrangement of the species of this genus. Stephens separates those which occur in Britain into two families, *Sesiidæ* and *Ægeriidæ*; the first containing the genera *Macroglossa*, Ochs. and *Sesia*, Fab. the second those of *Trochilium*, Scop. and *Ægeria*, Fab. Ochsenheimer comprehends the whole in the two former genera; but two out of the six species which he places under *Macroglossa*, namely *S. Fuciformis* and *Bombyliformis*, Stephens arranges with his *Sesiæ*. Latreille observes (*Nouv. Dict. d'Hist. Nat.* xxxi. 105.), that "Scopoli separated from the *Sphinges*, properly so called, certain species which have the abdomen terminated by a tuft, and of them he formed his genus *Macroglossum*. Other naturalists united them to the *Sesiæ*. Fabricius, in his *Systema Glossatorum**, comprehends under the latter generic name only those species (i. e. the *Macroglossa* of Scopoli); our *Sesiæ* being considered by him as forming the genus *Ægeria*. M. Ochsenheimer, in his work on the Lepidoptera of Europe, has, with reason, rejected this arrangement, and in that respect his opinion and mine coincide."—*Latr.* l. c.

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* Generic Characters, from Latreille.

Species.	Icon.
1. S. *Apiformis*, Linn.†	Ernst, III. Pl. XCI. f. 121. a. b.
2. — *Bembiciformis*, Hübn.†(*Crabroniformis*, Steph.)	Hübn. Sphing. Tab. 20. f. 98. (fœm.) Lewin, Linn. Trans. III. Pl. I. f. 6—10.
3. *Asiliformis*, Fab.‡ ...	Ernst, III. Pl. XC. f. 119. a. b.

4. S. *Rhin-*

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* Where is that work to be met with?—C.

† Trochilium, Stephens. The first genus of his family Ægeriidæ, which he thus characterizes:—"Fam. IV. ÆGERIIDÆ." "*Antennæ* fusiform, a little curved, ciliated in the males, the apex terminating in a plume of scales: *ocelli* two, minute, placed between the antennæ and the base of the thorax: *palpi* elongate, thickly clothed with scales and long hairs, the last joint elongate: *abdomen* cylindric, tufted at the apex: *wings* horizontally displayed, generally naked, with the tips above opaque: *larva* sub-cylindric, hirsute, tail-less: *pupa* elongate, with a row of spines on each segment; changes in the interior of plants."—*Illust. Brit. Entom.* I. 136.

The generic characters of Trochilium are detailed as follows:

"Genus 29. TROCHILIUM, *Scopoli.*

Antennæ short, more or less serrated, especially in the males, stout, gradually incrassated nearly to the apex, which is curved, acuminated, and terminates in a hairy tuft: *palpi* moderately long, parallel, suddenly recurved, the base very hairy, the apex scaly and attenuated: *head* small: *clypeus* densely clothed with elongate hairs: *thorax* and *abdomen* stout, the latter with a very small tuft at its apex: *wings* with the tips not clothed transversely with scales."—*Steph.* l. c. p. 137.

‡ Ægeria, Stephens.—"Genus 30. ÆGERIA, *Fabricius.*

Antennæ long, slender, gradually increasing in size nearly to the apex, which is slightly curved and acuminated; in the males they are slightly ciliated, sub-serrated, or pectinated: palpi longer than the head, divaricating, gradually reflexed, thickly clothed beneath with scales and long hair; the terminal joint somewhat naked and acuminated: *head* moderate;

Species.	Icon.
4. S. *Rhingiæformis*, Hübn.	Hübn. Sphing. Tab. 7. f. 41. (fœm.)
5. — *Sphecicormis*, Hübn.[*]	Ernst, III. Pl. XC. f. 120. a. b.
6. — *Scoliæformis*, Laspeyres.	Hübn. Sphing. Tab. 23. f. 111.
7. — *Hylæiformis*, Lasp.	Hübn. Sphing. Tab. 22. f. 108. (mas.) Tab. 8. f. 48. (fœm.)
8. — *Doryliformis*, Hoffm.[†]	
9. — *Chrysidiformis*, Hübn.[‡]	Ernst, III. Pl. XC. f. 118. a. b.
10. — *Prosopiformis*, Ochs.	Hübn. Sphing. Tab. 19. f. 93. (mas.)
11. — *Ichneumoniformis*, Fab.[‡]	Ernst, III. Pl. XCII. f. 124. a—d. Curtis, Brit. Ent. Pl. LIII.
12. — *Cynipiformis*, Esp.[‡]	Ernst, III. Pl. XCII. f. 125. a. b. (mas.) c. d. (fœm.) Steph. Pl. II. f. 2. ♂.
13. — *Melliniformis*, Lasp.[§]	Lasp. Ses. Europ. f. 5. 6.
14. — *Andrenæformis*, Lasp.	Esp. Schm. II. Th. Tab. XLIV. Cont. 19. f. 1. 2.
15. — *Stomoxyformis*, Hübn.[‖]	Ernst, III. Pl. XCIII. f. 126. a. b. d. Steph. I. Pl. XI. f. 3.

moderate: *clypeus* densely clothed with flat scales: *thorax* and *abdomen* rather slender, the latter with a large trilobed tuft, variable in form at its apex: *wings* transversely covered with scales at the tip."—*Steph.* l. c. p. 138.

Stephens subdivides the genus Ægeria into sections.

A. *Antennæ* much shorter than the body: of the males distinctly pectinated: *anterior wings* nearly clothed with scales: *abdomen* robust.

B. *Antennæ* as long as the body: of the males generally ciliated: *anterior wings* with the disc, hyaline.

- a. *Abdomen more or less robust and abbreviated, fasciated with whitish or yellow.*
- b. *Abdomen slender and elongated, generally with a single red belt.*
- c. *Abdomen beltless.*

The last subdivision of Sect. B. has only one individual, viz. *Sphinx,* (*Sesia*) *Ephemeræformis,* Haworth; a species not mentioned by Ochsenheimer.

[*] Ægeria, Sect. B. a. *Steph.* l. c. p. 140. Sp. 2. Pl. 11. f. 1.

[†] *Ses. Doryliformis*—alis hyalinis, marginibus fasciâque fuscis; abdomine barbato, nigro, segmento quarto margine albo; antennis ferrugineis, apice nigris.—*Ochs.* II. p. 141. *Sp.* 9.

[‡] Ægeria, Sect. B. a.—*Steph.*

[§] *Ses. Melliniformis*—alis hyalinis, anticis apice inauratis, marginibus fasciâque nigris; abdomine barbato, nigro, cingulis flavis; barba flava.—*Ochs.* II. p. 154. n. 14.

[‖] Ægeria, Sect. B. b. *Steph.*

	Species.	Icon.
16.	S. *Culiciformis*, Linn.[*]	Ernst, III. Pl. XCV. f. 126. e. f. Pl. XCIII. f. 126. c. Steph. I. Pl. X. f. 3.
17.	— *Mutillæformis*, Lasp.[*]	Ernst, III. Pl. XCIII. f. 127. a—d.
18.	— *Typhiæformis*, Lasp.	Bork. Eur. Schm. II. Th. S. 174. n. 13. c. fig. 4. 5. Tab. adj. Hübn. Sphing. Tab. 7. f. 42. (mas.)
19.	— *Formicæformis*, Lasp.[*]	Ernst, III. Pl. XCV. f. 132. a—d. Steph. Pl. XI. fig. 4.
20.	— *Nomadæformis*, Lasp.	Ernst, III. Pl. XCIV. f. 129. a—d.
21.	— *Cephiformis*, Goldegg.[†]	
22.	— *Eucercæformis*, Ochs.[‡]	
23.	— *Tipuliformis*, Linn.[§]	Ernst, III. Pl. XCIV. f. 130. a—d.
24.	— *Masariformis*, Ochs.[‖]	
25.	— *Tenthrediniformis*, Hübn.	Ernst, III. Pl. XCIII. f. 128. a—d.
26.	— *Philanthiformis*, Lasp.	Laspeyres, Ses. Europ. fig. 23, 24. (mas.) 25, 26. (fœm.)
27.	— *Tineiformis*, Hübn.	Hübn. Sphing. Tab. 7. f. 46. (fœm.)
28.	— *Brosiformis*, Hübn.	Hübn. Sphing. Tab. 25. f. 116. (mas.)

[To be continued.]

LXXV. *Attack of* Berzelius *on* Dr. Thomson's "*Attempt to establish the First Principles of Chemistry by Experiment.*"

OUR scientific readers need not be reminded, that in the work above quoted, Dr. Thomson has endeavoured to fix the combining equivalents of chemical substances.

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[*] Ægeria, Sect. B. b. *Steph.*

[†] *Ses. Cephiformis*, alis hyalinis, anticis marginibus fasciâque nigro-cæruleis; abdomine barbato flavo, cingulis tribus flavis.—*Ochs.* II. p. 169. *Sp.* 22.

[‡] *Ses. Eucercæformis*, alis anticis fuscis apice inauratis, maculis duabus hyalinis; abdomine barbato nigro, strigâ medii interruptâ flavâ.—*Ochs.* IV. p. 171. No. 22.

[§] Ægeria, Sect. B. a. *Steph.*

[‖] *Ses. Masariformis*, alis hyalinis, anticis apice flavo irroratis, marginibus fasciâque nigris: abdomine nigro, cingulis tribus flavis; barbâ terminali flavâ.—*Ochs.* II. p. 173. *Sp.* 24.

Dr. Thomson has found reason to adopt an idea, suggested some years ago by Dr. Prout, that the numbers which express the atomic weight of bodies are multiples by a whole number of the atomic weight of hydrogen; and his favourite object, visible in almost every page, is to prove the coincidence to be perfect. In this attempt he has been so successful, that the correspondence between his hypothesis and the result of his experiments is startlingly precise. As the accuracy of his results, which, if true, are very important, can be duly estimated only by an analyst of extensive experience, we looked forward with impatience to hear the opinion of Berzelius. His opinion has at length reached us; and as it is expressed in language extremely strong and extremely unusual, we think it necessary to employ his own words. We have accordingly translated a few passages from the *Yahres-Bericht* for 1827 (Woehler's Translation).

" This work belongs to those few productions from which science will derive no advantage whatever. Much of the experimental part, even of the fundamental experiments, appears to have been made at the writing-desk; and the greatest civility which his contemporaries can show its author, is to forget that it was ever published." (page 77.)

" Thomson has published an essay 'On the method of analysing sulphate of zinc;' a subject scarcely requiring, one would think, a separate essay, since the composition of this salt is known with considerable certainty. The great importance attached to it is owing to the circumstance, that in his large work on the atomic weights and chemical proportions, the analysis of this salt is the basis on which the whole superstructure is founded. In describing this analysis, Thomson states that the oxide of zinc was precipitated by carbonate of soda, and that 18·125 grains of crystallized sulphate of zinc yielded 8 grains of anhydrous neutral carbonate of zinc. In this fundamental analysis are two errors;—errors of such a nature as it is difficult to commit, and which appear to prove that the results were invented.—Some one had told Thomson that his whole work was of little value, because, in the fundamental experiment, the zinc was precipitated by carbonate of soda in the cold. To this privately communicated remark, Thomson openly replied, That he had supposed chemists would have given him credit for a knowledge of the mode of separating oxide of zinc from acids, and had therefore omitted details; but as he found this opinion erroneous, it became necessary to publish a full account of his process. Ninety grains (five atoms) of sulphate of zinc were precipitated by carbonate of soda, and yielded from 29·3 to 31·03 grains

of

of carbonate of zinc dried at the temperature of 212° Fahr. As this carbonate, when heated to redness, yielded 20·37 of oxide, it was anhydrous and neutral. The filtered solution was boiled, and the oxide of zinc which subsided, after being collected and ignited, weighed 4·54 grains. The solution was then evaporated to dryness in a porcelain vessel, and the salt again dissolved in water; when some oxide of zinc remained, which, when dried at a red heat, weighed 0·431. The solution was then again evaporated to dryness, and the residue ignited; and on being dissolved in water a little silicate of zinc was left, which after decomposition yielded 0·22 grains of oxide of zinc. To the remaining alkaline solution, after being neutralized by muriatic acid, a few drops of hydro-sulphuret of ammonia were added, which threw down a quantity of sulphuret corresponding to 0·65 grains of oxide of zinc. All these five portions together make up the sum of 26·211 grains, the weight of five atoms of oxide of zinc; so that 5·245 is the weight of one atom. Thomson, on this occasion, does not appear to have reflected on the fact, that neutral carbonate of zinc is never obtained by precipitation from an alkaline carbonate; and he has left this difficult point unexplained. The reason why all the oxide of zinc contained in a solution cannot be precipitated in the cold, is, that a portion is dissolved in the form of bicarbonate, while that portion of oxide which loses its carbonic acid, subsides in the form of a subcarbonate. We have seen in the experiments of Boussingault, that even the sesquicarbonate of soda precipitates a subsalt. Consequently, the statement as to the nature of the first and largest quantity of the precipitated oxide of zinc is obviously erroneous. And such is the method and result of one of the fundamental experiments on which Thomson's whole system stands, by which he obtained more accurate results than any preceding chemist, and through which he established for ever the atomic weights of bodies. The character of this work of Thomson's ought to exclude it from notice here; but it appears to me, that love for the real progress of science makes it imperative to detect quackery, and expose it to the judgement of every one as it merits." (page 181.)

We have thought it right to give as nearly as possible the very words of this critique, that scientific men may judge of its tone and merits. With respect to the former, we regret to see the dignity of science sacrificed by the intemperance of those who profess to be her advocates. It well becomes Berzelius to expose fallacy in argument, or detect error in analysis; but let him not pass beyond the limits of fair criticism: let him not arraign the character of an individual, who may be actuated by motives and principles as pure as his own. Intemperate attacks, such as this, reflect back upon their author, and indicate a mind inflamed by pique, jealousy, or some unworthy passion. We know not whether any cause for such feelings may exist in the present case, nor does it concern us to inquire; but we know Dr. Thomson to be devotedly attached to his profession, and we believe him to be sincere and honourable in his transactions. If deception exists at all, we are satisfied that Dr. Thomson himself is more deceived than any one. It is possible that, misled by a favourite hypothesis, he may, like many before him, have been too eager in seizing facts favourable to his views, and too tardy in perceiving those that are unfavourable. On this we offer no opinion at present; but must confess that several circumstances concur in shaking that confidence in the accuracy of his results which we once entertained. Dr. Thomson must be aware that the composition of the chloride of barium, as stated by him, has been declared by Berzelius to be erroneous; and that this error, if such, will vitiate many of his analyses. Would it not be prudent in Dr. Thomson to come forward and correct any mistake which he may have committed, rather than by delay allow others to do so for him? Does not the deference which British chemists have of late paid to him in adopting his atomic weights, impose on him the duty of admission or defence?

LXXIV. *On the Luminous Belt of September the 29th.*

To Mr. Richard Taylor.

Sir,

THE luminous belt which exhibited itself on the evening of September the 29th, in the present year, having been noticed and described from various parts of England, I beg leave to communicate its position as observed at a point very distant from most of the other stations, and therefore likely to be affected by a considerable parallax.

I was then at Rosemorran, the seat of George John, Esq., an elevated situation near Penzance, and about twelve miles from the Land's End. My attention was called to this unusual phænomenon at about eight o'clock. The belt then appeared to rise from the horizon, somewhat to the southward of west, and ascended with a steady light and uniform subtense, of perhaps three degrees, towards the zenith, passing over various stars that were scarcely altered in their appearance, till it reached Alpha Lyræ, then somewhat south of west, and nearly

62 or 63 degrees high. From thence diminished in brightness it became soon blended with the milky way, and ceased to be distinguishable. The belt seemed exactly similar to a ray of the northern light, except that not the least corruscation was to be observed. Its position could not be much out of the magnetic equator.

Sir William Elford, F.R.S. has favoured me with a detail of the appearances seen near Totness, very much agreeing with the above statements.

I am, yours, &c.
Davies Gilbert.

LXXVII. *Proceedings of Learned Societies.*

LINNÆAN SOCIETY.

Nov. 4.—MR. Bicheno read a paper On the advantages attending the use of the English Language in Natural History.

The author insists that the use of Latin and other foreign languages in the classification of Natural History has retarded its progress; and that an acquaintance with the productions of nature was more extensively diffused before the Latin became the vehicle of communication. Gerard has recorded some thousands of English names of plants, derived from an English stock, which are no longer in use; while every indigenous species, however, to our ancestors seems to have had a familiar name in English, Welsh and Erse. Every word particularly had its appropriate appellation. Ask a farmer now how many plants he is acquainted with, and he will betray an incredibly scanty stock of knowledge, for one who is traversing the carpet of the earth many hours of every day of his life: he will confound under the name of Charlock more than half a dozen species, and out of a hundred kinds of Grass he will be acquainted with a most insignificant number.

Professor Michaelis remarks, that the Eastern nations must have been better acquainted with the vegetable kingdom than ourselves; since we find more than 250 plants named in the Old Testament, by writers who have made use of these names in prose and metre only incidentally, and not as botanists; and that in all probability such knowledge must in those times have been very generally diffused.

Of all sciences Natural History is one of observation. To lock it up therefore in a dead language, or to give it less currency among the rural part of the population, whose business is with the operations of Nature, is to confine it within the bounds of the closet. For the want of names the attention of the English scholar is not arrested, or he has no means of recording his observations; and the learned, pent up in cities, or only making an occasional excursion out of them, turn their more copious vocabulary to less account, and their faculty of observing to such trifling and minute particulars as their studies will afford.

The great storehouse of botanical and zoological names is still to be found in the writings of the Greeks. Among them, learning and science, philosophy and poetry, went hand in hand. The people were taught every thing; Natural History was diffused; and they have handed down for our use a stock of names which no other language can equal.

It may perhaps remain doubtful whether the old English names are not too much forgotten to be revived, and whether it is not now, in many cases, as easy to establish the foreign names. Still there would remain much that might be familiarized to the English scholar by the use of his native language.

Among living languages, ours is admirably adapted for the teaching of sciences in which new terms are requisite. Foreign words are easily engrafted or transplanted into it. It allows of a great variety of compounds and diminutives, and those terminating either as substantives, adjectives, verbs or participles, both active and passive. It has been asserted by competent judges, that it is in these respects as accommodating as the Latin, and not much less so than the Greek. To the honour of the late President of the Linnæan Society, he did much to familiarize the recondite language of botany to the English student. The probability is, that naturalists will very soon be forced to adopt their native language as a vehicle of communication. Our popular schools, universities, gardens and publications all tend this way.

Such an occurrence need not prevent the learned of all nations from still holding intercourse, and it would throw open to multitudes a boundless field of experiment and observation. Wherever a learned language has been in the possession of a privileged class, history and experience show that the excluded party is left in ignorance; and the establishment of a "universal character," so much desired by some of the philosophers of the 17th century, instead of advancing knowledge, as they imagined, would most materially have retarded it, and have kept the people in darkness.

A paper was also read, entitled, Description of a new species of *Phalangista*, by Thomas Bell, Esq. F.R. & L.S.

Ord. *Marsupiata.* Genus *Phalangista.*

Ph. gliriformis, Dorso rufo-cinereo, gulâ fulvâ, maculâ post aurem utrinque albâ: auribus nudis. *Hab.* Australasia.

ROYAL ACADEMY OF SCIENCES OF PARIS.

June 2.—The Academy received an essay On the duodecimal calculus, by M. Gautier de Nuits.—A memoir On the developments of plane curves, by MM. Dubois Aimé, and Bigeon.—A new notice respecting the yellow fever. By M. Leymerie.—A note by M. Julia de Fontenelle, in reply to the claims which had been made on the subject of his memoir On human combustions.—A letter from M. Baudelocque, inclosing some discoveries in the art of Midwifery.—Afterwards, the reports of the several Committees respecting Montyon's prizes were read.—The sitting terminated with hearing part of a memoir by M. Villermé On the mean height of the inhabitants of France.

June 9.—The following manuscripts were presented. A letter from MM. Parkes and Company, with some specimens of salt from their establishment.—A note from M. Dizé On an indelible ink.—A sealed packet from M. Comté, containing a continuation of some physiological experiments.—M. Cuvier presented some teeth of the gigantic tapir, dug up in the arrondissement of Saint-Gaudens, by M. Cabuel, engineer of bridges and highways.—The Commission deputed to examine the memoirs on statistics, made a report.—M. Le Roy d'Etielles read a memoir On asphixy.

June 16.—In this public sitting there were heard: The historical eulogy of M. Ramond, by M. Cuvier.—The historical eulogy of M. Charles, by M. Fourier.—And a physiological memoir On the brain, by M. Magendie.—Want of time prevented M. Prony from reading a memoir which he had compiled On the labours of the late M. Perronet.

June 23.—Manuscript pieces received at this sitting: A letter from the Minister of the Interior respecting a memoir by M. Braun On the direction of balloons.—A letter from M. Pétard relative to a disease by which he had been attacked.—A description of the instruments used in measuring geodesical bases. By M. Imbert.—Notice of the discovery of a luminous liquid. By M. Kirch.—M. Mathieu, on behalf of a Committee, made a favourable report on the *Sécateur perspectif* of M. Lalanne.—M. Navier gave an advantageous account of a memoir by M. Corancez, intitled *De l'Integration de quelques équations aux différences partielles, et du mouvement de l'eau dans les vases.*—Agreeably to the report of a Committee, the Academy decided that the memoir presented by M. Foville On the anatomy of the brain should be printed in *Recueil des Savans etrangers.*—M. Adolphe Brongniart read a memoir, intitled *Nouvelles récherches sur le pollen et les granules spermatiques des végétaux.*

June 30.—M. Lionville presented a memoir On electro-dynamic phænomena, and particularly on the mutual actions of the pole of a magnet and a conducting wire.—M. Timel, author of a work On the intellectual organization of man, requested the appointment of Commissioners.—The Academy afterwards heard at this sitting: A note by M. Mirbel, On a remarkable development of the stalk of the *Calycanthus floridus.*—A memoir by M. Becquerel, On the electricity excited by the friction of two metals.—A memoir by M. Daussy, intitled *Détermination des positions géographiques de Malte, Milo et Corfou.*—The conclusion of M. Villerme's memoir On the mean height of man in France.—A report by M. Ampère, On a projected instrument by M. Nobili, intended to measure the intensity of electric currents.—M. Gay Lussac presented a specimen of the ultramarine prepared by M. Guimet, and a new pyrophorus formed by calcining sulphate of potash with charcoal.

July 7.—M. Delattre de Rougemont deposited a sealed packet containing a description of a new method of curing a severe disease.—M. Tournal sent the second part of his Geognostic description of the environs of Narbonne.—The Minister of War communicated the report which he had received respecting the effects of lightning upon the powder-magazine at Bayonne.—M. Russel d' Inval requested that the processes which he had proposed for facilitating arithmetical calculations might be examined.—M. Lassis again expressed his opinion on the subject of epidemic diseases.—A letter was read, in which chloride of lime was mentioned as a remedy for the distemper in dogs.—MM. Chevalier and Langlumé sent their memoir On lithography.—M. Poisson read a notice On the problem of waves.—M. Benoiston de Chateauneuf communicated a manuscript work respecting the wheat-harvest of France in former and present times.

LXXVIII. *Intelligence and Miscellaneous Articles.*

METEORIC IRON IN FRANCE.

M. HERICART DE THURY, on 13th of October last, read a notice to the Academy of Sciences respecting the discovery of a mass of meteoric iron existing at Caille, near Grasse, department du Var. In August last, M. Brard sent from Fréjus a specimen of the mass in question, with respect to the origin of which he did not decide. The examination made by the author caused him to suspect that it might be meteoric iron, and he therefore wrote to M. Brard to beg that he would go to the place, in order to determine the nature of the mountain on which it was discovered; to examine the mass of supposed meteoric iron; and to collect from the inhabitants all the information which they could give him. The following is extracted from the account given by M. Brard:—The mass of iron, which had been for two years placed at the door of the church at Caille, has been in that village about 150 years. It was discovered in the mountain of Audebert, a league off, and was drawn by four oxen into a court or garden in the village, where it seems to have been forgotten; but an inhabitant having inclosed it in a wall, it was claimed as an object held in some veneration; the wall was pulled down by the authorities, and the enormous mass was deposited in the principal street of the village, from which it was removed to the spot which it now occupies.

The form of the mass is very irregular; its external colour blackish brown, with a shade of lead-colour; it is shining, but occasionally spotted with yellow rust; its internal colour is whiter than that of common iron. It weighs about 1000 or 1200 pounds.

The mountain on which this mass was found is of considerable altitude, and similar to those which surround it;—there are no appearances of iron-works having ever existed in the neighbourhood.

This iron has the crystalline appearance which marks its meteoric origin, and M. Laugier has found that it contains nickel. Application has been made for its removal to Paris, and this has probably been already accomplished.

It was reported in the village that two smaller masses were found with it, which were used for making horseshoes, nails, &c. It was also proposed to heat this mass, and thus divide it and apply it to the same purposes; fortunately for the interests of science the greatness of the mass prevented the intended destruction.—*Le Globe.*

IRON TRADE.

In the year 1740 the whole iron made in Great Britain was
17,000 tons from 59 furnaces
In 1788, it had increased to 68,000 tons from 121 furnaces.
In 1796, it had increased to 125,000
In 1806, it had increased to 250,000
In 1820, it had increased to 400,000
In 1827, it had increased to 690,000 tons from 284 furnaces.

The different counties in which it is made are as under in 1827.

Staffordshire......	216,000 tons from	95 furnaces.
Shropshire	78,000 tons from	31 furnaces.
South Wales......	272,000 tons from	90 furnaces.
North Wales	24,000 tons from	12 furnaces.
Yorkshire........	43,000 tons from	24 furnaces.
Derbyshire	20,500 tons from	14 furnaces.
Scotland	36,500 tons from	18 furnaces.
	690,000 tons from	284 furnaces.

About 3-10ths of this quantity is of a quality suitable for the foundry, which is all used in Great Britain and Ireland, with the exception of a small quantity exported to France and America. The other 7-10ths is made into bars, rods, sheets, &c., of which a large quantity is exported to all parts of the world.—*Repertory of Arts, October* 1828.

EARLY HISTORY OF ELECTRO-MAGNETISM.

In the year 1801, Gautherot brought two fine piano-forte strings in contact, one with the upper and the other with the lower end of the pile, keeping the extremities fluttering in the air. When these ends touched each other, he says (*Ann. de Chim. tom.* xxxix. p. 209.) "A very decisive adhesion took place; they seemed united as by a magnetic power, which was so strong that he could move the united wires in every direction to a distance of some centimetres." Thus it may be seen that, in the investigation of Nature, a few detached observations are insufficient, but that they must be pursued and combined; by which means it was that Œrsted became the discoverer of electro-magnetism.—*Schweigger's Jahrbuch*, 1828. p. 110.

RUSSIAN COINAGE OF PLATINA.

A letter from Professor Breithaupt to Dr. Schweigger-Seidel, an extract from which is given in a late Number of the *Jahrbuch de Chemie*, &c., confirms the statement some time since made by the newspapers, that the Russian Government had resolved to coin a large sum in Siberian platina. It appears that Count Denidoff, the proprietor of the locality where the platina was discovered, has disposed of to the Government the quantity of that metal which had been collected. He has sent four young Russians, destined for official situations in Siberia, to be educated at the Mining Academy of Freyberg.

ANALYSES

ANALYSES OF MINERALS.

Radiolite, by Professor Hünefeld.

Silica	0·41880
Alumina	0·23790
Soda	0·14069
Potash	0·01012
Water	0·10000
Oxide of iron	0·00910
Carbonate of lime	0·02500
Matrix	0·05500
	0·99661

Datholite from the Hartz, by Dr. DuMenil.

Lime	35·59
Silica	38·51
Boracic acid	21·34
Water	4·60
	100·04

Bismuthblend of Breithaupt, by Professor Hunefeld.

Carbonate of bismuth	58·8
Arsenic acid	2·2
Silica	23·8
Arseniate of cobalt, copper and iron	5·9
Matrix	9·1
	99·8

White ironsinter from Freiberg, by M. K. Kersten.

Arsenic acid	30·25
Oxide of iron	40·45
Water	28·50
	99·20

Schweigger's Jahrbuch, 1828.

ANALYSIS OF THE IPECACUANHA BRANEA. BY M. VAUQUELIN.

This is the root of the *viola ipecacuanha* from Rio Janeiro; it is of a pale white colour, of the size of a quill, crooked and knotty at unequal distances. Its fracture is even, and slightly resinous; its odour is disagreeable, and its taste, at first scarcely perceptible, is afterwards acrid and nauseous. Sixteen grammes of this root reduced to a fine powder were macerated in water for forty-eight hours. The liquor was decanted and fresh water added to the powder, and the maceration continued for the same time: this water was scarcely coloured; it was poured off, and the residuum was treated with boiling water till it was exhausted. The whole of the liquors were mixed and filtered, and evaporated with a very gentle heat.

The liquid became turbid, and deposited a flocculent substance of a dirty white colour, which being collected by a filter was proved to be albumen, by the ammonia and fetid oil which it yielded on distillation. The liquor from which the albumen was separated, after due evaporation, was treated with a small quantity of carbonate of magnesia to saturate the acid it contained; an insoluble salt was formed, which being separated by the filter was found to be gallate of magnesia, but in very small quantity.

The solution again evaporated to dryness on a salt-water bath yielded a brown extract, which attracted moisture slightly. This extract being digested in cold alcohol, the resin and emetin were dissolved, and the other substances remained unacted upon: the solution after filtration was evaporated in a similar way, a dark-coloured extract obtained, which being treated with hot water the emetin was dissolved and the resin separated.

The solution of emetin gently evaporated to dryness, weighed a gramme and a half. On again treating, the precipitate formed, at the time of the separation of the emetin and resin, with hot alcohol, and filtering it, a fatty matter and wax separated on cooling: the residuum being treated with cold water, the gum was separated, and the amylaceous fecula was precipitated. During the exposure of the emetin to heat, a pellicle of small crystalline scales is formed, which is probably the matter that imparts bitterness and acridity to the emetin.

Sixteen parts of the root appear to consist of

Emetine	1·50
Resin	0·60
Gum	0·20
Albumen	0·30
Starch	3·20
Crystalline scaly matter	0·85
Ligneous matter	7·00
Fatty matter, wax, and loss	0·05
	16·00 *

PROSPECTUS OF A PLAN FOR INVESTIGATING THE NATURAL PRODUCTIONS OF JAMAICA.

It is with great pleasure that we give insertion to the following circular, which has been transmitted to us for that purpose: we hope, for the interests of science, that the proposed investigation will speedily be set on foot.

"It has long been matter of regret among men of science, that those natural productions of Jamaica which are not as yet the immediate objects of commerce, should be so little known in England. Its geology and mineralogy have been but partially examined. The history of its quadrupeds, birds, insects, reptiles, and fish, has been almost entirely neglected. The botany has, indeed, been more attended to; but it is acknowledged by the most intelligent men, and among the rest by Humboldt, that there is no tropical region where the native plants have been as yet accurately enumerated; that the Flora of none of the greater islands of the Antilles has been properly examined; and that, even in our times, travellers in Hispaniola, Jamaica and Cuba, daily find lofty trees which constitute new genera; while the

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* There is a considerable error in the statement of this analysis; the products amount to only 13·65 instead of 15·95, as here stated.—Edit.

medicinal and other useful qualities of its innumerable vegetable productions remain, in a great degree, unascertained. This account of the very imperfect state of our knowledge, which is by no means confined to one island, but is almost equally true *in respect to all*, has suggested to a few persons the expediency of an attempt to investigate the natural history of Jamaica.

"They are persuaded that whatever information may be acquired in respect to it, will be immediately beneficial to the other West Indian colonies, Jamaica being only selected as affording the most extensive field for studying the productions of a climate common to all. They consider it an object of great importance to the whole body, that no means should be omitted or overlooked which may contribute to connect the West Indies with the more enlightened classes at home; and they should very much regret if *all* did not seek to participate in the honour arising from the prosecution and dissemination of scientific discoveries, as well as in the other advantages which have hitherto closely and inevitably attended them. Impressed with these opinions, and in part relying on the known spirit of philosophical research which exists so remarkably in this country, they have ventured to hope that it may be possible to raise such a sum among those more closely connected with the colonies, as well as among those who are influenced by a general attachment to the cause of knowledge, as may be sufficient to send out scientific men well fitted for examining the state of Jamaica as connected *exclusively* with its natural history, and the *probabilities afforded* by its advantages of soil and climate for the cultivation of articles as yet new to its commerce. They have reason to think that it will be easy to find such competent persons, in whose zeal and intelligence entire confidence may be reposed, and who will be most ready to undertake the investigation. They have sanguine hopes that some public bodies may take an interest in the object, and be inclined to give it countenance and support without depriving it of the character of an individual and scientific enterprise; and they feel persuaded that the subscription of a comparatively moderate sum would fully accomplish this desirable object within a very limited period.

"Being anxious to forward a plan which appears to them so full of utility, they have taken the liberty of circulating this address: but as all arrangements for carrying it into execution, for disposing of the seeds and specimens, as well as for giving publicity to the information which will no doubt result from it, must of course depend upon the money raised, and the view which the subscribers may take of the best mode of accomplishing their own objects;

"It is proposed that if the scheme meets with such support as they fully anticipate, a general meeting should be called as early in the fall of the year as is convenient, for the purpose of considering the general arrangements, and the best means of carrying them into effect. In the mean time, those gentlemen who are inclined to afford their cooperation, are requested to send such an intimation to Henry Bright, Esq. M.P. 2, Stone Buildings, Lincoln's Inn, London, who has undertaken to lay the substance of the information received before the gentlemen at whose suggestion this circular has been issued."

ON A METHOD BY LAPLACE OF SOLVING EQUATIONS OF THE THIRD DEGREE. BY M. FAYOLLE.

To the Editors of the Philosophical Magazine and Annals.

Messieurs,

Je vous envoie, comme une suite à l'article sur les équations du second degré*, une méthode très élégante de Laplace, pour résoudre les équations du 3^e^ degré par les tables des sinus.

Dans l'équation

$$x^3 \mp px + q = 0,$$

q étant positif, supposons

$$x = r\left(z \pm \frac{1}{z}\right),$$

nous aurons

$$x^3 \mp px + q = r^3\left(z^3 \pm \frac{1}{z^3}\right) \pm (3r^3 - pr)\left(z \pm \frac{1}{z}\right) + q = 0$$

Soit $r^2 = \frac{p}{3}$,

et $-3q\sqrt{3} = 2h$;

on aura $z^3 \pm \frac{1}{z^3} = 2h,$

équation du 6^e^ degré, mais résoluble à la manière de celles du second.

Supposons 1° que le signe supérieur ait lieu, et que h, abstraction faite du signe, soit < 1.

Alors $\frac{q^2}{4} - \frac{p^3}{27}$ est une quantité négative, et l'équation proposée tombe dans *le cas irréductible*.

Si l'on fait $z = \cos u + \sin u \sqrt{-1}$,

on aura d'abord

$$x = \sqrt{\frac{p}{3}}\left(z + \frac{1}{z}\right) = 2\cos u\sqrt{\frac{p}{3}};$$

on aura ensuite

$$z^3 + \frac{1}{z^3} = 2\cos 3u,$$

et partant $\cos 3u = h$.

Soit A le plus petit des angles dont le cosinus est h, et que les tables feront connaître. On aura pour $3u$ les trois valeurs, (c étant la demi-circonférence,)

$$A,\ 2c + A,\ 4c + A;$$

et parconséquent les trois valeurs de x seront

$$x = 2\sqrt{\frac{p}{3}}\,.\cos\frac{1}{3}A,$$

$$x = 2\sqrt{\frac{p}{3}}\,.\cos\left(\frac{2c + A}{3}\right),$$

$$x = 2\sqrt{\frac{p}{3}}\,.\cos\left(\frac{4c + A}{3}\right).$$

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* See above, p. 314.

OPENING OF
THE UNIVERSITY OF LONDON,
FOR THE SESSION 1828—9.

THE Council published last July, a general view of the nature and objects of th Institution. They are now advanced so far in their preparations for opening th University as to be able to lay before the public an account of the proposed metho of tuition and of the Courses of Study adapted to particular objects; with a deta of the days and hours when the several Professors are to teach, and of the Fees.

This "Second Statement" is to be had of J. Taylor, Bookseller to the Univer sity, 30, Upper Gower Street; Longman and Co., Paternoster Row; Murra Albemarle Street; at the University Chambers, 29, Percy Street, Bedford Square of Black, Edinburgh; and Hodges and Smith, Dublin. Price 1*s.* 6*d.*

The "First Statement" to be had as above, Price 1s.

The following Abstract contains the more important parts of it.

I. *Of the Days and Hours when the different Branches of Education are to be taught.*

The different branches of study for which Professors have been appointed, are such as properly belong to an University; and in the arrangement of the subjects to be taught in the Classes which will be attended by the junior students, it has been assumed that they will come possessed of that elementary knowledge which boys who leave school at fourteen or fifteen years of age have generally acquired.

The Lecture Rooms will be open to all who comply with the terms and the regulations of the University, without limitation as to age, and without examination as an indispensable preliminary. Persons who wish to attend the Lectures of one Professor only will be admitted; but those who intend to apply for University Certificates and other distinctions, must go through certain courses of study; for these testimonials will be granted to such students only, as upon examination at regular intervals are found to possess that knowledge by which the value of the Certificate or of the academical honour will be determined.

The following are the days and hours when the different Classes are to be taught. Unless otherwise expressed, the Lectures are to continue throughout the whole Academical Session, which will commence the First of November, and continue to the middle of July, with the exception of the Medical Classes, which will open the First of October, and terminate about the end of May. There will be short vacations at Christmas and Easter.

ROMAN LANGUAGE. THE REV. JOHN WILLIAMS, A.M. of Balliol College, Oxford.

Junior Class. Every morning, (except Saturday,) from 8½ to 10½. About 340 hours of instruction, Fee 7*l.* 10*s.*—*Senior Class.* Every morning, (except Saturday,) from 11 to 12½. On Saturday, from 10½ to 12. About 300 hours of instruction, Fee 7*l.* 10*s.*

GREEK LANGUAGE. GEORGE LONG, Esq. A.M. late Fellow of Trinity College, Cambridge.

Junior Class. Every day, (except Saturday,) from 11½ to 1½. About 340 hours of instruction, Fee 7*l.* 10*s.*—*Senior Class.* Every morning, (except Saturday,) from 8½ to 10½. About 340 hours of instruction, Fee 7*l.* 10*s.*

ENGLISH LANGUAGE. THE REV. THOMAS DALE, A.M., of Corp. Chris. Coll., Camb.

Class for the Principles and Practice of Composition. Tuesday and Thursday, from 2½ to 4. Satu day, from 8½ to 10. About 150 hours of instructio Fee 6*l.*

English Literature. Monday and Friday, from to 2. About 70 hours of instruction, Fee 4*l.*

GERMAN LANGUAGE. LUDWIG VON MÜ LENFELS, LL.D.

Tuesday and Thursday, from 2½ to 4. Saturd Morning, from 8½ to 10. About 150 hours of instr tion, Fee 6*l.*

German Literature. Wednesday and Saturd from 1 to 2. About 70 hours of instruction, Fee

ITALIAN LANGUAGE. AN. PANIZZI, LL.

Monday, Wednesday, and Friday Mornings, fr 10½ to 11½. About 100 hours of instruction, Fee

Italian Literature. Wednesday and Friday, fr 3 to 4. About 70 hours of instruction, Fee 4*l.*

SPANISH LANGUAGE. DON A. GALIA

Tuesday, Thursday, and Saturday Mornings, fr 10½ to 11½. About 100 hours of instruction, Fee

Spanish Literature. Tuesday and Thursday, fr 3 to 4. About 70 hours of instruction, Fee 4*l.*

HEBREW. HYMAN HURWITZ, Esq.

Junior Class. Monday, Wednesday, and Fri Mornings, from 9½ to 10½. About 100 hours of struction, Fee 5*l.*—*Senior Class.* Tuesday Thursday Mornings, from 9½ to 10½. About hours of instruction, Fee 5*l.*

HINDOOSTANEE. J. B. GILCHRIST, LL.

Students will have the liberty of attending the fessor of Hindoostanee for such portion of the sion as shall suit their convenience. The Fee for hour daily during the Session will be 7*l.* 10*s.*

ORIENTAL LITERATURE. FREDERIC SEN, Phil. Doct.

Dr. Rosen will give instruction in the Ar Sanskrit, and Persian Languages, and the ar ments will be made to suit the convenience of dents, as soon as the Professor shall have had a sonal communication with those who wish to at his Classes.

MATHEMATICS. AUGUSTUS DE MORGAN, A.B., of Trinity College, Cambridge.

Junior Class. Monday, Wednesday, Friday, 2½ to 4. Saturday, from 10½ to 12. About hours of instruction, Fee 7*l.*—*Senior Class.* day and Thursday, from 2½ to 4. Saturday, fro to 10. About 150 hours of instruction, Fee 6

NATURAL PHILOSOPHY AND AS NOMY. The REV. D. LARDNER, LL.D., F.

Junior Class. Every day, (except Satur from 1 to 2. About 170 hours of instruction, Fe —*Senior Class.* Tuesday, Thursday, and Satu from 11 to 12. About 100 hours of instruction, F

CHEMISTRY. Ed. Turner, M.D., F.R.S.E.

Every morning, (including Saturday,) from 10 to 11. *First Course.* About 100 hours of instruction, Fee 4l. *Second Course.* About 100 hours of instruction, Fee 3l. These Lectures, forming a branch of general as well as of Medical education, will commence on the First of November.

It is intended that Occasional Lectures shall be given during the Session, upon The Application of Mechanical Philosophy to the Arts, upon Civil Engineering, and upon Chemistry applied to the Arts, the particulars of which will be announced hereafter.

BOTANY. John Lindley, F.R.S.

Every morning, (including Saturday,) in May, June, and July, from 8 to 9. About 80 hours of instruction, Fee 4l.

POLITICAL ECONOMY. John R. MacCulloch, Esq.

Monday, Wednesday, Friday, and Saturday, from 10 to 11, from 1st of February, to the End of the Academical Session. About 100 hours of instruction, Fee 5l.

JURISPRUDENCE. John Austin, Esq., Barrister at Law.

Monday, Wednesday and Friday Mornings, from 9 to 10. About 100 hours of instruction, Fee 5l. These Lectures will be suspended during the Quarter Sessions and Spring Circuit.

ENGLISH LAW. Andrew Amos, Esq., Barrister at Law, late Fellow of Trin. Coll., Camb.

Monday, Wednesday, and Friday Evenings, (except during the Quarter Sessions and Spring Circuit,) from 6½ to 7½. About 100 hours of instruction, Fee 5l.

ANATOMY. Granville Sharp Pattison, Esq.

Every day, (except Saturday,) from 2 to 3½.

First Course	About 120 hours of instruction each,	Fee 4l.
Second Course		3l.

DISSECTIONS AND DEMONSTRATIONS. James R. Bennett, Esq.

First Course	Four Months each,	Fee 3l.
Second Course		2l.

PHYSIOLOGY. Charles Bell, Esq., F.R.S.

Monday, Wednesday, and Friday, from 11 to 12.

First Course	About 50 hours of instruction each,	Fee 3l.
Second Course		2l.

COMPARATIVE ANATOMY AND ZOOLOGY. Robert E. Grant, M.D.

Monday, Wednesday, and Friday, from 3 to 4. bout 100 hours of instruction, Fee 5l.

NATURE AND TREATMENT OF DISEASES. John Conolly, M.D.

Every morning, (except Saturday,) from 9 to 10.

First Course	About 80 hours of instruction each,	Fee 3l.
Second Course		3l.

MIDWIFERY. David D. Davis, M.D.

Monday, Tuesday, Thursday, and Friday, from 10 to 11.

First Course	About 60 hours of instruction each,	Fee 3l.
Second Course		3l.

ATERIA MEDICA AND PHARMACY. Anthony Todd Thomson, M.D.

Every morning, (except Saturday,) from 8 to 9.

First Course	About 80 hours of instruction each,	Fee 3l.
Second Course		3l.

Hospital Attendance. Aware of the great im- nce of affording to the pupils of a Medical hool the opportunity of observing Hospital [illegible]ctice, and of receiving Clinical instruction, Council will make every effort to obtain a [illegible]pital which shall be under their own direc- In the mean time, an opportunity has oc- [illegible] of making an arrangement by which the [illegible] will have it in their power to witness the Medical and Surgical practice in the Middlesex Hospital; and Dr. Watson, one of the Physicians, and Charles Bell, Esq., one of the Surgeons, of that Hospital, having accepted the appointments of Professors of Clinical Medicine and Clinical Surgery, the Lectures in this department will be delivered at the University as follows:

Clinical Medicine, on Monday and Wednesday Evenings, from 6 to 7.
About 60 hours of instruction, Fee £4.

Clinical Surgery, on Tuesday and Thursday Evenings, from 6 to 7.
About 60 hours of instruction, Fee £4.*

Dispensary Attendance. The Council are preparing to establish a Dispensary in one of the adjoining streets, to be attended by the Medical and Surgical Professors of the University.

The Council will continue, until further notice, to receive applications from Candidates for the following Professorships which are still vacant.

Logic and Philosophy of Human Mind. Moral and Political Philosophy. History, Ancient and Modern. Surgery. Mineralogy and Geology. French Language and Literature.

II. *Of the Fees to be paid by the Students.*

The Fee which has been stated to be payable for each Course includes the remuneration to the Professor, and a payment to the fund from which the annual expenses of the University are to be defrayed. It has been deemed necessary to state the number of hours of tuition, including examinations, because the term "*Course of Lectures,*" as commonly used, would give an inadequate idea of the amount of instruction which the student will receive at the University for each Fee.

• That scale of Fees applies to students who shall be nominated by a Proprietor; all who are not so nominated must pay an addition of 1l. 10s. to the University Fund for each Course of Lectures continuing throughout the Academical Session, unless they attend more than three. If they attend a greater number, this extra payment will not be required for the additional Courses. The additional Fee to be paid by students not nominated, will be 15s. for each of those Courses which continue during one half of the Session, and for the Summer Course of Botany.

Those who become regular University students,—that is, students entered for a complete course of education, whether general or professional,—must pay a Matriculation Fee of 2l. the first year of their attendance, which will entitle them to admission to the Library and Museums for four successive Sessions. Occasional students must pay annually 10s. for one Course, and 1l. for two or more Courses, which payment will entitle them to admission to the Library and Museums.

The Council are now ready to receive the names of Pupils who mean to attend the University during the Session which is to commence next October.

The Register in which the names will be inscribed, will be found, until further notice, at the University Chambers, No. 29, Percy Street, Bedford Square, every day, except Sunday, from nine in the morning to five in the afternoon. The parents and friends of pupils, or the pupils themselves who may wish to see THE WARDEN before entering their names, may call at the Chambers between Two and Five o'clock, on Mondays, Wednesdays, or Fridays. As an interview is desirable between the Professors and their pupils, in order that the Professor may ascertain the state of preparation of the student, the address of each may be obtained at the Chambers also. Students are requested to enter their names, and see the Professors whose Courses they mean to follow, as early as possible, as it is important for the interest of the students that the Professor should know their previous attainments, so that he may adapt his instruction accordingly.

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* Until a Professor of Surgery is appointed, Mr. Bell will make such additions to this Course as shall make it sufficiently comprehensive for the Certificate required by the College of Surgeons.

Proprietors have the right of having one student of their nomination at the University for every share of One Hundred Pounds which they possess; and Donors of Fifty Pounds have the same privilege.

III. *Of the Courses of Study.*

It is probable that, in the majority of instances, those who enter as regular University students will be disposed to attend the Lectures in that order which the Council think most advantageous for a complete general education. To such students, the following Course for four years is recommended. Those who are already advanced beyond the Junior Classes may begin in that part of the Course for which they are most fitted by previous attainments; but as the examinations for the University Certificates and other distinctions will include what is taught in the earlier as well as in the later years of study, the pupil ought to ascertain before he passes over a Junior Class, that he is fully prepared to undergo an examination upon all that is to be learned in it.

FIRST YEAR.—Junior Latin, Junior Greek, and Junior Mathematics.

SECOND YEAR.—Senior Latin, Senior Greek, and Senior Mathematics.

A student may this year substitute *Junior Natural Philosophy* for *Senior Mathematics*, or he may attend both Classes.

THIRD YEAR.—Chemistry, Junior Natural Philosophy, Logic, and the Philosophy of the Human Mind.

FOURTH YEAR.—Jurisprudence, Political Economy, Senior Natural Philosophy, and Moral and Political Philosophy.

	£.	s.	d.
Average annual expense of the four years to students nominated by a Proprietor for the above education	22	7	6
Average annual expense of the four years to students not nominated .	26	17	6

In addition to the above branches, the student will have time to study the Modern Languages and attend the Lectures on History, and some of the most popular among the Professional Courses.

The arrangements for the Medical School have been so framed as to enable the pupils to comply with the regulations which must be observed by Candidates for the Diplomas of the Royal College of Surgeons, or the Company of Apothecaries.

The expense of the Courses required by the College of Surgeons to a student at the University, will be as follows:

	£.	s.	d.
3 Courses of Anatomy	10	0	0
1 Course of Physiology	3	0	0
1 Course of Surgery	3	0	0
1 Course of Chemistry	4	0	0
2 Courses of Midwifery	6	0	0
Dissections and Demonstrations . .	5	0	0
Matriculation Fee	2	0	0
Total Fees of the University, if nominated by a Proprietor . .	33	0	0
Addition, if not nominated . . .	4	10	0
	37	10	0
One Year at Middlesex Hospital .	22	6	0
	59	16	0

The expense of the Courses required by the Company of Apothecaries to a student at the University, will be as follows:

	£.	s.	d.
1 Course of Materia Medica . . .	3	0	0
1 Course of Chemistry	4	0	0
2 Courses of Anatomy	7	0	0
1 Course of Physiology	3	0	0
2 Courses of Practice of Medicine .	6	0	0
Matriculation Fee	2	0	0
Total Fees of the University, if nominated by a Proprietor . .	25	0	0
Addition, if not nominated . . .	4	10	0
	29	10	0
Six Months at Middlesex Hospital .	10	10	0
	40	0	0
Or, Nine Months at the Dispensary.	7	7	0
Fees as above	29	10	0
	36	17	0

The time which the Professors are to devote to their pupils will be considerably greater, in most of the Classes nearly one half more, than has hitherto been the case in the Courses delivered at the Medical Schools in London; for the subjects of the Lectures will be treated more minutely, and there will also be weekly examinations of the pupils.

Rooms are provided for the accommodation of the pupils during the intervals of lecture; and suitable refreshments will be sold within the Building, by a Steward (to be appointed by the Council) upon his own account, but subject to regulations both as to what he is to supply and the prices to be charged.

IV. *Of Examinations and Certificates.*

It is intended that, *in every class* in the University, the Professor shall devote a certain portion of the hours of instruction in each week to the examination of his pupils.

The manner of conducting these examinations, and the frequency of their recurrence, must necessarily vary: and as each Professorship will probably require a separate plan of examination, the details can only be properly settled in concert with the several Professors.

Persons who may be desirous of attending Lectures in the University, without submitting to examination, will be at liberty to do so. No student, however, who wishes to obtain a Certificate can be exempted from the examinations.

It is intended that, in addition to the weekly examinations, there shall be at the conclusion of every Session a public examination of all who may be desirous of obtaining a Certificate from the Professor whose Course they have attended. As the value of these documents must depend upon the strictness of the examinations, such a system will be adopted as shall most accurately determine the attainments of those to whom they are granted.

Besides Certificates of the Professors, the University will grant Certificates of general proficiency in literature and science. Every student will be required to produce a certain number of Professors' Certificates, before he can be allowed to enter upon the examination for the General Certificate.

It would be premature at present to fix the conditions on which General Certificates will be granted. The Council will be enabled hereafter to frame the regulations with greater precision, profiting by the suggestions which experience will afford, when the system of the University is in operation.

V. *Library and Museums.*

The Council have set apart a portion of the funds at their disposal for collections in Anatomy, Natural History, Books, and Philosophical apparatus; and they propose in the month of October to open the small Library and Anatomical Museum. The Council have availed themselves of an opportunity of adding to this Museum a more perfect collection of drawings of morbid structure than, it is believed, has hitherto been applied to the purposes of teaching and study, and which will be peculiarly valuable to the student of the Practice of Medicine. Dr. A. T. Thomson is collecting a Museum of Materia Medica on a more extensive scale than has hitherto been attempted in that branch of medical science.

Dr. Lardner has been employed nearly a year in the collection of philosophical apparatus; and Dr. Turner is preparing all that is necessary to render the laboratory complete and efficient.

VI. *Houses for the Reception of Boarders.*

The Council feel that their direct interference in the management of houses opened for the reception of boarders must necessarily be inefficient; and unwilling to give a pledge which they cannot redeem, they will not attempt to lay down any rules for the conduct of the students beyond the walls of the University. They therefore earnestly recommend to parents and guardians, in the first instance, to be scrupulously careful in examining the references given by persons opening such houses, and the experience of a very short period will establish the character of those in whom confidence may be placed.

Some of the Professors have informed the Warden that they intend to receive boarders in their families.

This Statement by the Council is followed by a brief Outline by each Professor, pointing out the manner in which they mean to treat their several subjects.

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ADVERTISEMENTS.

RELIGIOUS INSTRUCTION.

the undersigned Professors in the University [illegible] London, who are Clergymen of the Esta-[illegible] Church, having from the period of our [illegible] entertained the intention of providing religious instruction for those students who are members of our Church, do hereby Give Notice, That final arrangements have been at length made, with the full approbation of the Council, for that purpose.

An Episcopal Chapel has been purchased contiguous to the University, to be called "THE UNIVERSITY CHAPEL," where accommodation will be afforded to the students for attendance at divine service, and where a Course of Divinity Lectures will be regularly delivered during the Academical Session. Parents and others interested in this arrangement, may learn further particulars by applying to Mr. Taylor, 30, Upper Gower Street.

THOMAS DALE, M.A. Camb.
DIONYSIUS LARDNER, LL.D. Dublin.
JOHN WILLIAMS, M.A. Oxon.

BOARDING HOUSES.

MR. TAYLOR, Bookseller and Publisher to the University, No. 30, Upper Gower Street, with the sanction of the Council, has opened a REGISTER, in which the names of those Housekeepers will be inserted who are willing to receive Boarders. Those who wish to have their names entered and retained in this Register, must comply with the following Conditions:

That the persons applying shall produce satisfactory proof of correctness of character, with regard to religious and moral habits. A testimonial from the Minister to whose congregation they belong, will be indispensable.

That they will require their boarders to be at home at an early hour of the night.

That they will not suffer gaming or licentious conduct on the part of the boarders.

That they will require their boarders to attend some place of public worship.

That they will make an immediate report to the parents or friends of their boarders, in case of any irregularity of conduct or serious illness.

That they will not receive any boarders except students of the University.

The names of such persons as shall be discovered to have evaded these rules, will be erased from the Register, and notice of that erasure will be sent to the parents or relatives of all the students who may be boarded in their houses.

⁂ Notice must be sent to Mr. TAYLOR of the names of the Boarders, and the names and addresses of their parents or relatives.

THE REGISTER WILL CONTAIN,

The names and addresses of the applicants.

Their religious persuasion.

The names and addresses of the persons by whom they are recommended.

The description of the several persons of whom their family consists.

The number of boarders they propose to take.

Their terms.

A Fee of Five Shillings must be paid for Registration, and an additional Fee of Five Shillings annually for each Boarder.

PRIVATE TUTORS.

MR. TAYLOR, with the sanction of the Council, has also opened a REGISTER, for receiving the names of PRIVATE TUTORS, in the various Departments of Literature and Science taught at the University.

[illegible]

ANNUAL EXAMINATION

OF THE

SCHOLARS AT WYKE-HOUSE,

SION-HILL,

NEAR BRENTFORD, MIDDLESEX;

JUNE 16th—18th, 1828.

The small proportion of the paper occupied by the account of the classical examination might lead some to suppose that these studies were considered of minor importance, and engaged but a small part of the scholars' time. The fact, however, is, that from the nature of the subject it would be impossible to enter into that detail of the classical part of the examination which is absolutely necessary in the mathematical.

Two gentlemen, the one of the University of Oxford and the other of that of Cambridge, examined the pupils in the classics on the 10th and 11th of June privately, and by their decision the prizes were awarded.

A. J.

James Whiting, Printer, Beaufort House, Strand.

CLASSICS, &c.

FIRST CLASS.—AGE FROM 14 TO 16.

Greek.—Homer's Iliad, Dalzel's Analecta Majora, vol. i. and ii. Huntingford's Exercises, and Eton Grammar.

Latin.—Horace, Livy, and Cicero; Elegantiæ Latinæ, Themes, Versification, and Eton Grammar.

Morals.—Greek and Latin Testament, Grotius de Veritate.

History.—Tytler's Elements of General History, Potter's Grecian Antiquities, Composition, and Poetry.

French, Geography, and the Construction of Maps.

Mathematics.—The Geometry of Euclid and Legendre, Algebra, Trigonometry, &c. See pages 5—13 of the paper.

SUBJECTS FOR EXAMINATION.

Iliad, book iv.; the Anabasis of Xenophon, book i.; Livy, book xxviii.; Horace, book ii. of the Odes.

PRIZE—*A Gold Medal.*

SECOND CLASS.—AGE FROM 12 TO 14.

Greek.—Analecta Græca Minora, Exercises, and Eton Grammar.

Latin.—Virgil, Sallust, Exempla Moralia, Bland's Verse Book, and Eton Grammar.

Morals.—Greek and Latin Testament, Selectæ è Prof. Hist.

History.—Of Greece and of Rome, and Adam's Roman Antiquities; English Composition.

French, Geography, with the Use of Globes and Maps.

Mathematics.—Euclid, Algebra. See the paper.

SUBJECTS FOR EXAMINATION.

Selections from Dalzel's Analecta Græca Minora; and Virgil's Æneid, book iii.

PRIZE—*A Silver Medal.*

THIRD CLASS.—AGE FROM 10 TO 12.

Greek.—Edward's Delectus, and Eton Grammar.

Latin.—Ovid's Metamorphoses and Epistles, Nonsense Verses, Ellis's Exercises and Eton Grammar.

Morals.—Latin Testament, Selectæ è Veteri Testamento.

History.—Of England, with Geography.

French.—English Grammar and Exercises.

Algebra, with Arithmetic and Writing.

SUBJECTS FOR EXAMINATION.

Four Pages of Greek Delectus; and Ariadne to Theseus, from Ovid's Epistles.

PRIZE—*A Regent's Virgil.*

FOURTH CLASS.—AGE FROM 9 TO 11.

Latin.—Ovid's Epistles, Eton Grammar, New Testament in Latin, Ellis's Exercises, and Exempla Minora.

History of England, Geography, and French Reading with Exercises.

Arithmetic, English Grammar and Exercises, Writing.

SUBJECT FOR EXAMINATION.

Dido to Æneas, from Ovid's Epistles.

PRIZE—*A Regent's Horace.*

FIFTH CLASS.—AGE FROM 8 TO 10.

Latin.—Delectus, Eton Grammar, and Exercises.

English Grammar, Reading, Writing and Arithmetic, French Reading, and Geography.

SUBJECT FOR EXAMINATION.

Three Pages of Valpy's Delectus.

PRIZE—*Sharpe's Edition of Beattie's Poems.*

SIXTH CLASS.—AGE FROM 6 TO 8.

Eton Latin Grammar, Valpy's Delectus, French, English Grammar and Reading, Arithmetic, and Writing.

PRIZE—*Sharpe's Edition of Goldsmith's Poems.*

MATHEMATICS.

FIRST DIVISION.

Prize, *A Gold Medal—Age from* 15 *to* 18.

SECOND DIVISION.

Prize, *A Silver Medal—Age from* 12 *to* 15.

ALGEBRA.

1. Multiply $x^2 + xy + y^2$ by $x^2 - xy + y^2$.
2. Multiply $x^{\frac{1}{2}} - \frac{7}{8}y + \frac{2}{3}$ by $x^{\frac{1}{2}} - 2y + 1$.
3. Multiply $a^{\frac{9}{2}} + a^3 b^{\frac{3}{2}} + a^{\frac{3}{2}} b^3 + b^{\frac{9}{2}}$ by $a^{\frac{3}{2}} - b^{\frac{3}{2}}$.
4. Multiply $1 + a + a^2 + \ldots. + a^n$ by $1 - a$.
5. Divide $x^6 + 3x^4 + 3x^2 + 1$ by $x^2 + 1$.
6. Divide $a^2x^4 - y^4$ by $a^{\frac{1}{2}}x - y$.
7. Divide $x - 6561$ by $x^{\frac{1}{4}} + 9$.
8. Divide $a + x$ by $a - x$.
9. Divide a^x by $a - 1$.
10. If 180 men, in 6 days, of 10 hours each, can dig a trench 200 yards long, 3 wide, and 2 deep; in how many days, of 8 hours long, will 100 men dig a trench of 360 yards long, 4 wide, and 3 deep?
11. What is a decimal fraction? Prove that in the multiplication of decimal fractions, there are as many decimal places in the product as in the multiplier and multiplicand together.
12. Resolve the number 315000 into factors, each of which shall either be a prime number or a power of a prime number.
13. $\sqrt{18} = \sqrt{8} + \sqrt{2}$.

    $\sqrt{32} - \sqrt{2}$.

14. The squares, cubes, &c. of numbers less than unity, are less than the numbers themselves; prove this, and also that $\frac{1}{x^n}+\frac{1}{x^{n-1}}+\cdots+\frac{1}{x^2}+\frac{1}{x}+1$ is less than $1+\frac{n}{x}$ when x and n are greater than unity.

15. If a be greater than b, $\frac{a+m}{b+m}$ is less than $\frac{a}{b}$.

16. If A be divisible by a, and B be not divisible by a, then neither is A − B nor A + B divisible by a.

17. Find the fourth power of $6x+5y$, and the cube of $\frac{7}{8}x^{\frac{5}{6}}-y$.

18. Find the greatest common measure of $a^6+a^4y^2-a^2y^4-y^6$, and $a^7+a^4y^3-a^3y^4-y^7$.

19. Reduce the fraction $\frac{6y^3-35y^2+48y+9}{7y^3-43y^2+69y-9}$ to its lowest terms.

20—42. Solve the following equations:

$$\frac{7x+2}{3}+5x=28+\frac{5x-6}{7}$$

$$\frac{18x-19}{28}+\frac{11x+21}{6x+14}=\frac{9x+15}{14}$$

$$\sqrt{x+16}=2+\sqrt{x}$$

$$\sqrt{1+x\sqrt{x^2+12}}=1+x$$

$$\left\{\begin{array}{l}4x+9y=51\\8x-13y=9\end{array}\right\}$$

$$\left\{\begin{array}{l}\frac{3x-1}{5}+3y-4=15\\\frac{3y-2}{6}+2x-8=7\frac{2}{3}\end{array}\right\}$$

$$\left\{\begin{array}{l}v+x+y+z=0\\3v+x-y-3z=0\\v+9x+9y+z=-2\\3v+9x-9y-3z=0\end{array}\right\}$$

$$\left\{\begin{array}{c}x^2-xy:xy-y^2::3:7\\xy^2=147\end{array}\right\}$$

$$\left\{\begin{array}{c}y\sqrt{x}+\sqrt{y}=10\\y+xy^2=68\end{array}\right\}$$

$$\left\{\begin{array}{c}x^3+y^3-xy(xy+1)=0\\x^6y=6561\end{array}\right\}$$

$$x^2-10x+17=1$$

$x^2-2abx+a^2=0$, and show that when b is less than unity, its roots are impossible.

$$\frac{2x+3}{10-x}=\frac{2x}{25-3x}-6\frac{1}{2}$$

$$x^{\frac{6}{5}}+x^{\frac{3}{5}}=72$$

$$x^4+ax^3-adx-d^2=0$$

$$(x-2)^2-6x^{\frac{1}{2}}(x-2)=24-14x+15x^{\frac{1}{2}}$$

$$\frac{2x+\sqrt{x}}{2x-\sqrt{x}}=3\frac{7}{15}-3\frac{2x-\sqrt{x}}{2x+\sqrt{x}}$$

$$x^{\frac{7}{3}}+41\sqrt[3]{x}=\frac{97}{\sqrt[3]{x^2}}+x^{\frac{5}{3}}$$

$$\epsilon^{4x}+\epsilon^{x}=1$$

$$\left\{\begin{array}{l}a^{xy}=b\\x+y=c\end{array}\right\}$$

$x^3+4x^2-9x-36=0$, two of whose roots may be represented $+a$ and $-a$.

$x^3+9x^2+23x+15=0$, whose roots are in arithmetic progression.

$x^3-3x^2+4x-12=0$, by Cardan's method.

43. If N be any number, and x the radix, find an expression for N $(x - 1)$ in terms of the radix and the digits of N, and by means of that expression point out a method of multiplying by 9, and apply this method in producing the following equations:

$$1 \times 9 + 2 = 11.$$
$$12 \times 9 + 3 = 111.$$
$$123 \times 9 + 4 = 1111.$$

$$123456789 \times 9 + 10 = 1111111111.$$

$$9 \times 9 + 7 = 88.$$
$$98 \times 9 + 6 = 888.$$
$$987 \times 9 + 5 = 8888.$$

&c.

$$98765432 \times 9 + 0 = 888888888.$$
$$987654321 \times 9 - 1 = 8888888888.$$

44. The difference of two numbers is half their sum, and the difference of their squares is twice their difference; what are the numbers?

45. It is always possible to take x so great that x^2 shall be greater than $Ax + B$, where A and B are any quantities whatever.

46. If any number of cows can eat a field of grass in 10 days, how long will they be in eating it when every night one-fourth as much grass as was eaten the day before grows afresh?

47. Prove that the quantity $ay^2 + bxy + cx^2$ can always be put under the form $a(y + Ax)^2 + Bx^2$, and find A and B in terms of a, b, and c.

48. $$\left.\begin{array}{r} a + 2b + 3c = 4 \\ 2a + 3b + 4c = 5 \\ 3a + 4b + 5c = 6 \end{array}\right\}$$ Prove that the number of values of a, b, and c is infinite.

49. Find two whole numbers whose product shall be divisible by their sum.

50. In how many ways can an equivalent to 13 dollars, at three shillings each, be given in English crowns and seven-shilling pieces.

51. The sum of an infinite geometric series is $\frac{8}{7}$, and the first term 1; find the ratio.

52. The sum of an arithmetic series is 280, the first term 1, and the common difference $\frac{1}{2}$; find the number of terms.

53—58. Sum the following series:

$1 + 2 + 3 + \cdots + n$

$1 + 3 + 5 + \cdots 2n - 1$

$a^n + a^{n-2}b^2 + a^{n-4}b^4 +$ &c. to n terms.

$\frac{11}{4} + \frac{47}{16} + \frac{191}{64} + \frac{767}{256} +$ &c. to n terms, where each numerator is four times the preceding $+ 3$, and where the denominators are in geometric progression.

$1 . 2 . 4 + 2 . 3 . 5 + 3 . 4 . 6 +$ &c. to x terms.

$\frac{1}{1^4} + \frac{1}{2^4} + \frac{1}{3^4} +$ &c. ad infinitum.

59. Extract the square root of $\frac{1}{a+b}$ by means of the binomial theorem.

60. Expand $(1 - x)^{\frac{5}{11}}$, and determine the law of the series.

61. Find the eleventh term of the expansion of $(a^2 + b^2)^{\frac{1}{2}}$.

62. Multiply together the series

$a_0 + a_1x + a_2x^2 +$ &c.

And $b_0 + b_1x + b_2x^2 +$ &c.

And prove that if the product be

$A_0 + A_1x + A_2x^2 + A_3x^3 +$ &c.

$A_0 = a_0b_0$

$A = ab + ab$

$A_2 = a_2b_0 + a_1b_1 + a_0b_2$

$A_n = a_nb_0 + a_{n-1}b_1 + \cdots\cdots + a_1b_{n-1} + a_0b_n$

63. Let $a - b + c - d$, &c. be a series the terms of which decrease, show that the sum of n terms of the series will differ from the sum of the series by a quantity less than the $(n+1)$th term.

64. Expand a^x algebraically, and prove that $a^x = e^{x \text{ Nap log } a}$

65. Prove from the series hyp log $(1+x)=x-\frac{x^2}{2}+\frac{x^3}{3}-$ &c. that $\log z = n\left\{\frac{z^{\frac{1}{n}}-1}{1}-\frac{(z^{\frac{1}{n}}-1)^2}{2}+\frac{(z^{\frac{1}{n}}-1)^3}{3}-\text{\&c.}\right\}$ where n is any number whatever, which, when n is very great, becomes $\log z = n(z^{\frac{1}{n}}-1)$ nearly.

66. Find the amount of 50*l.* in 5 years or 20 quarters, at 5 per cent. per annum compound interest, the interest being payable quarterly.

67. The present value of an annuity of 1*l.* for 2 years, compound interest, is $\frac{820}{441}$*l.*; find the rate per cent.

68. Find the present value of an annuity of A pounds, to commence after the expiration of n years and continue for ever.

69. Given the probability of an event happening in one trial, to find the probability of its happening exactly t times in n trials.

70. If a person deal 26 cards from a pack, what is the probability that one at least of two specified cards will be amongst them.

71. In the game of whist, what is the probability that an individual has at least one court card in his hand.

72. Find the present value of an annuity of 1*l.* to be paid as long as either of two specified individuals is living.

MENSURATION AND GEOMETRY.

73. Multiply 3 feet 5 inches by 5 feet 11 inches.

74. The base of a triangle is 10 chains 63 links, and the perpendicular 4 chains 9 links; find the area.

75. The area of a regular pentagon, whose side is 1, is 1·7204774; show how this number is obtained; and by means of it find the area of a regular pentagon whose side is 76·8.

76. The altitude of a cylinder is 2 feet 6 inches, and its diameter 10 feet; find the solid content in cubic inches.

77. Explain what is meant by the quadrature of the circle.

78. The radius of the base of a paraboloid is 1 foot, and the height 6 inches; find the solidity.

79. Parallelograms upon equal bases and between the same parallels are equal.

80. The opposite angles of any quadrilateral figure, inscribed in a circle, are together equal to two right angles.

81. If two triangles have one angle in one equal to one angle in the other, and the sides about the equal angles proportionals, the triangles shall be equiangular, and shall have those angles equal which are opposite to the homologous sides.

82. Find the angles which the sides of a regular pentagon make with each other.

83. In any triangle if a straight line be drawn from one of the angles to the bisection of the opposite side; twice the square of this line, together with twice the square of half the bisected side, are together equal to the sum of the squares of the other two sides.

84. The diagonals of an equilateral parallelogram are at right angles to one another.

85. Of all the triangles which can be inscribed in a circle having a given base, the greatest is that which hath the other two sides equal; and of all the triangles which can be inscribed in a circle, the greatest is that which is equilateral.

86. If two triangles have an angle common to both, their areas are to each other as the rectangles contained by the sides about that angle.

87. If any number of rectangles be inscribed in a given triangle, and upon their bases triangles be described similar to a given triangle, and similarly situated, prove that their vertices are all in the same straight line.

88. If two similar figures have their homologous sides parallel to one another, the lines joining their homologous points, two and two, will all meet in one point.

89. A circle is parallel to a given plane, and its centre is in a line at right angles to that plane. In the perpendicular, a light, considered as a point, is placed. Given the distance of the light from the circle and the plane and the area of the circle; find the area of the shadow cast upon the plane.

90. Having given the base of a triangle, and the ratio of the sides; find the locus of the vertex.

PLANE TRIGONOMETRY.

91. Prove that $\sin(A + B) = \sin A \cdot \cos B + \cos A \cdot \sin B$ (rad, 1).

92. The sides of a plane triangle are 500 feet, 560 feet, and 100 feet; find the natural cosine of the greatest angle to a radius of 10 feet.

93. Prove that $\tan(A - B) = \frac{\tan A - \tan B}{1 + \tan A \cdot \tan B}$.

94. Prove that $\cos\theta + \sin\theta = \sqrt{1 + \sin 2\theta}$, and $\cos\theta - \sin\theta = \sqrt{1 - \sin 2\theta}$; and from hence find the sine of 15°, knowing that of 30°.

95. $a\cos\theta + b\sin\theta = \frac{a\sin(\phi + \theta)}{\sin\phi}$ if $\tan\phi = \frac{a}{b}$; make use of this to solve the equation $\cos\theta + \sin\theta = 1.1$.

96. If A, B, and C be the angles of a triangle, and a, b, and c the sides opposite to A, B, and C, respectively; prove that $\sin(A - B) : \sin C :: a^2 - b^2 : c^2$.

97. A point of land was observed by a ship at sea to bear east-by-south, and after sailing north-east 12 miles, it was found to bear south-east-by-east. It is required to determine the place of that headland, and the ship's distance from it at the last observation.

98. If the height of the mountain called the Peak of Teneriffe be $2\frac{1}{3}$ miles, and the angle taken at the top of it as formed between a plumb line and a line conceived to touch the earth in the horizon or farthest visible point, 88° 2′; it is required from these measures to determine the magnitude of the whole earth, and the utmost distance that can be seen on its surface from the top of the mountain, supposing the earth a sphere.

99. $\log \operatorname{cosec} 2A - \log \operatorname{cosec} A = \log \sec A + \log 2$; find the radius.

100. Investigate a formula to find from proper observations the height of an object situated upon a horizontal plane, but which can only be approached within a certain distance. When this method fails, show that a solution may be obtained by measuring

a base transversely, and proper observations. Also find a formula for the height in this case.

101. If a be any arc of a circle, and R the radius, prove that

$$\left(\cos^n\frac{a}{2}\sin\frac{a}{2}\right)\left(\cos^{n-1}\frac{a}{2^2}\sin\frac{a}{2^2}\right)\cdots\left(\cos\frac{a}{2^n}\sin\frac{a}{2^n}\right)=\left(\frac{R}{2}\right)^{\frac{n(n+1)}{2}}\sin^n a$$

CONIC SECTIONS AND MATHEMATICAL GEOGRAPHY.

102. Find the equation of the parabola considered as a geometric curve.

103. In the parabola, if Pp be any parameter, S the focus, and PN, pn, ordinates to the axis; prove that $\frac{SN\cdot Sn\cdot \sec PSN}{Nn}$ is a constant quantity.

104. If a tangent and ordinate be drawn from any point in the ellipse, meeting the transverse axis, the semitransverse will be a mean proportional between the distances of the said two intersections from the centre.

105. The orthographic projection of a circle is an ellipse, when the plane of the circle is inclined to the plane of projection.

106. In a chart on Mercator's projection, the length of the meridian from the parallel of 30° to that of 60°, is to the radius of the sphere, as the natural logarithm of $\frac{\sqrt{3}+1}{3-\sqrt{3}}$ to 1.

N. B. The whole of the mathematical paper except two questions was cleared among the four senior scholars in the *First Division;* and three of the pupils in the *Second Division* answered one half of the questions. The decision of the prizes was made upon the 9th, 10th, and 11th of June, when the whole of the scholars underwent a private examination by two gentlemen of Trinity College, Cambridge.

A. J.

SPEECHES.

Brutus and Cassius.
Shakspeare.

Laudes Ruris.
Ex Tibullo.

Othello and Iago.
Shakspeare.

Mors Tibulli.
Ex Ovidio.

Diogenes et Alexander
Ex Luciani Dialogis.

M. de Pourceaugnac
Molière.

Syphax and Juba
From Addison's Cato.

Andromache, Hecuba, and Priam, on the Death of Hector.
Iliad, book xxiv.

Æneas relating his Adventures to Dido.
Æneid, book iii.

Hotspur and Glendower.
Shakspeare.

Hannibalis Oratio et Responsio Scipionis
T. Livii, lib. xxx. cap. 31, 32.

Præcepta Culinaria Epicureorum.
Horatii Satira IV. lib. ii.

WYKE-HOUSE.

YOUNG GENTLEMEN ARE BOARDED AND EDUCATED
AT THIS SCHOOL,
IN THE
CLASSICS AND MATHEMATICS,
UPON THE FOLLOWING TERMS:—

Under Ten Years of Age	60	Guineas per annum.
From Ten to Fifteen years of Age	70	
Above Fifteen Years of Age	80	

All the Scholars have Single Beds.

Pupils having separate Bed Rooms pay One Hundred and Fifty Guineas a Year.

There are no extra charges except for Masters in the French, Spanish, and Italian Languages, Drawing, Dancing, Music: the last eight, and the others six guineas a Year each.

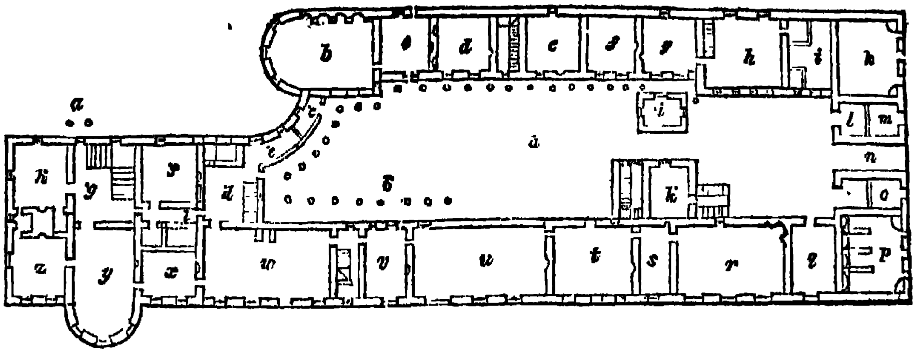
Every Pupil brings with him a knife and fork, a silver spoon, two pairs of sheets, six towels, a pair of slippers, a flannel dressing-gown, in addition to his wardrobe, which should be marked with its owner's name at full length.

The Holidays are Five Weeks at Midsummer, and the same period at Christmas; and a charge of Ten Guineas is made for residence during either of the vacations.

Three months' notice must be given before any Pupil is removed from the School.

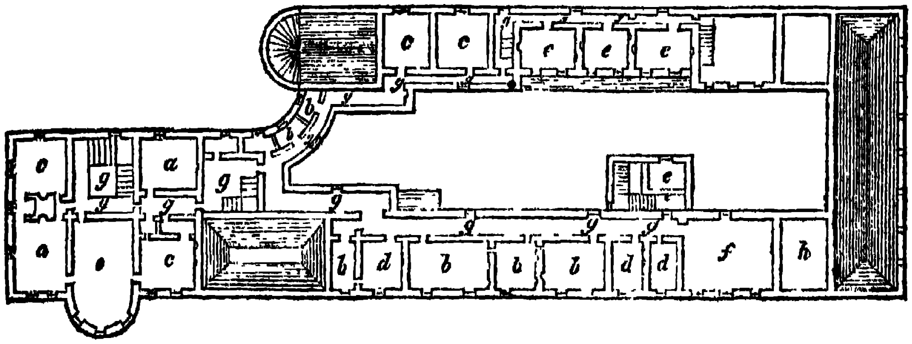
Applications for admission are to be addressed to Dr. Jamieson, under whose personal direction the Institution is conducted.

GENERAL PLAN OF WYKE-HOUSE.



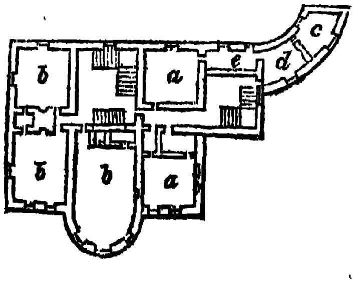
GROUND FLOOR.

a, Principal entrance.
b, Kitchen; 26 × 22 feet.
c, Housekeeper's room.
d, Scullery. *e*, Larder.
f, Bakehouse.
g, Mangling room.
h, Laundry.
i, Coal yard. *k*, Coach-house.
l, Knife-house. *m*, Harness room.
n, Domestic entrance.
o, Tool-house. *p*, Stable. *q*, Brewery.
r, Classical school room; 36 × 20 feet.
s, Hat room.
t, Mathematical school room; 26 × 20 ft.
u, Dining hall; 36 × 20 feet.
v, Buttery.
w, Chapel, 36 × 20 feet.
x, Study, &c.
y, Dining room, 28 × 18 feet.
z, Library, &c.
·*a*, Court yard.
·*b*, Colonnade.
·*c*, Shoe room.
·*d*, Entrance from court.
·*e*, Bathing room.
·*f*, Masters' room.
·*g*, Entrance hall.
·*h*, Parlour.
·*i*, Dairy. ·*k*, Fruit room.
·*l*, Passage, &c.



FIRST FLOOR.

a, Family bedrooms.
b, *c*, *f*, Scholars' bedrooms.
d, Masters' bedrooms.
e, Domestics' bedrooms.
g, Stairs, lobbies, galleries, &c.
h, Cistern that supplies the house with water; the height of the first floor.
i, Drawing room.



SECOND FLOOR.

a, *b*, Scholars' bedrooms.
c, *d*, Hospital.
e, Surgery.